TRANSACTIONS

OF THE

INTERNATIONAL MEDICAL CONGRESS.

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OF THE

INTERNATIONAL MEDICAL CONGRESS

OF

PHILADELPHIA.

1876.

EDITED FOR THE CONGRESS

BY

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PREFACE.

In explanation of the delay which has attended the publication of the present volume, the Editor would invite attention to the diversified character of its contents, which has necessitated the sending of proof-sheets to more than threescore authors, residing in all portions of the country, and has thus required a much greater expenditure of time than would have been needed under other circumstances.

The volume contains all the Addresses delivered before the Congress in its General Sessions, and, with few exceptions, all the Papers read in the meetings of the several Sections, and by them recommended for publication. Abstracts of the Discussions which ensued upon the reading of the several papers, are also appended in many instances, and, it is believed, will be found to add materially to the interest of the whole.

A few papers have been withdrawn by their authors, and three have been omitted by the Committee of Publication: one on account of its inappropriateness to the work of the Section before which it was read, and of its fragmentary nature, the paper to which it was really but an introduction not having been furnished by the author; another on account of the great expense which would have attended the reproduction of the drawings with which it was illustrated, and without which it would have been valueless; and a third because, though it was referred to the Committee by the Section before which it was read, yet the President and Secretary, and several prominent members, of the Section, afterwards united in declaring that it had been so referred by mistake, and that it was not designed for publication. One paper which was recommended for publication has not been received, and appears to have been lost before the records of the Congress were placed in the Committee's hands.

Two papers were considered too voluminous for the Transactions, and are, therefore, represented by condensed abstracts, one furnished by the author, and the other by the Secretary of the Section to which it had been presented. The Committee of Publication has not felt justified in rejecting any paper on the sole ground of want of merit, after it had been deliberately approved by the Section which heard it read, and to which the responsibility of accepting or refusing it properly belonged; but, in justice to itself, the Committee cannot avoid expressing the

opinion that, in some few instances, the censorship exercised by the Sections might properly have been more rigid.

To the actual proceedings of the Congress and of its several Sections, are prefixed Lists of the Officers and Members of the Congress, the Minutes of the General Sessions, the Address of Welcome of the President, Professor Gross, and, in order to complete the history of the Congress, a brief account of its inception and of the Centennial Medical Commission, prepared by Dr. Hutchinson.

Though authorized by a resolution of the Congress to call upon each delegate for a further contribution towards the expense of issuing the volume, the Committee of Publication determined, after mature deliberation, that it would be better not to make an additional assessment, but to endeavor, by the exercise of a careful supervision, to reduce the cost of the volume to the sum actually in hand from registration-fees and from subscriptions; this has been done, and the Committee has the satisfaction of terminating its labors with no deficit in the treasury.

The Editor offers his sincere thanks to the other members of the Committee of Publication for the cordial support which they have uniformly extended to him in the laborious, and occasionally ungrateful, duties of his office. For any errors which may be detected in his work, and he does not presume to hope that many such do not exist, he bespeaks the friendly indulgence of the reader; and, in bringing his task to a close, he ventures to express a hope that this volume, on which he has expended more than a year of arduous labor, may be found to furnish a not unsatisfactory memorial of the meetings of that important body of which it records the acts—the First International Medical Congress of America.

JOHN ASHHURST, Jr.

2000 West DeLancey Place,
Philadelphia, October, 1877.

THE

CENTENNIAL MEDICAL COMMISSION AND THE INTERNATIONAL MEDICAL CONGRESS.

BY

JAMES H. HUTCHINSON, A.M., M.D.,

OF PHILADELPHIA.

The project of holding an International Medical Congress in Philadelphia, during the Centennial Anniversary of the Declaration of Independence by the United States, seems to have been entertained by the profession as early as 1872, and to have grown out of a resolution originally introduced by Dr. J. G. Stetler, at a meeting of the Philadelphia County Medical Society, held in October of that year. This resolution proposed that a general conference of the different medical societies and colleges in the city should be held to determine what part the medical profession should take in the approaching celebration, which was just then beginning to excite general interest in the community. The form which the proposed celebration should assume, had not, however, been fully agreed upon, and the time at which it was to take place was still far off in the future. Moreover, its advocates had not yet succeeded in convincing our people that it was destined to be one of the great events in our history. Under these circumstances it is not surprising that Dr. Stetler's resolution did not bear immediate fruit, and that the subject was allowed to lie dormant for more than a year.

The question was, however, reopened in January, 1874, in the same society, which then appointed a committee, consisting of Drs. L. Turnbull, J. G. Stetler, and M. O'Hara, to take the matter into consideration. This committee, in due time, reported in favor of the project, and proposed the Fourth of July, 1876, as the day upon which the Congress should begin its sessions. It recommended, also, that the other medical societies of the city should be requested to appoint committees to co-operate with that of the County Society in preparing a plan. At this time, but one other society responded to the request; and the committee soon after, finding that it did not fully represent the profession in the city, extended invitations to several prominent physicians to take part in its deliberations. These invitations were in most instances cordially accepted. The committee, thus increased in numbers and influence,

organized on the 29th of March, 1875, as "The Centennial Medical Commission of Philadelphia," with the following officers:—

President.
Samuel D. Gross, M.D., LL.D., D.C.L. Oxon.

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It was at this time also decided that the Congress should meet in September, instead of in July, as originally suggested, and that it should continue in session for at least six days. It was, moreover, proposed that the mornings should be devoted to business and to the reading of discourses upon topics of general interest, and that in the afternoons the Congress should meet in Sections for the discussion of scientific questions.[1] After much discussion, it was determined that there should be nine Sections, as follows: (1) A Section on Medicine; (2) A Section on Biology; (3) A Section on Surgery; (4) A Section on Dermatology and Syphilography; (5) A Section on Obstetrics; (6) A Section on Ophthalmology; (7) A Section on Otology; (8) A Section on Sanitary Science; and (9) A Section on Mental Diseases. The Commission further decided that the Sections should meet on Monday, Tuesday, Wednesday, Thursday, and Friday afternoons of the week during which the Congress should be in session; that, on four of these afternoons, the deliberations of each Section should be opened by the reading of a paper upon a subject of scientific interest, by a gentleman specially chosen for that purpose, and that the paper should afterwards be submitted for discussion. The remaining afternoon was to be left free for the reception of voluntary papers. The elaboration of this plan, together with the choice of subjects to be discussed, was left to special committees.

At the meeting of the Commission which was held on the 19th of April, 1875, another and a successful effort was made to secure the co-operation of the other medical societies of the city, all of which appointed committees to

[1] The subjects finally chosen for the discourses, with the names of the gentlemen selected to deliver them, were as follows: 1. Medicine and Medical Progress, Dr. Austin Flint, of New York; 2. Surgery, Dr. Paul F. Eve, of Tennessee; 3. Obstetrics, Dr. Theophilus Parvin, of Indiana; 4. Therapeutics, Dr. Alfred Stillé, of Pennsylvania (declined); 5. Medical Jurisprudence, Dr. Stanford E. Chaillé, of Louisiana; 6. Medical Biography, Dr. Joseph M. Toner, of the District of Columbia; 7. Medical Institutions and Education, Dr. N. S. Davis, of Illinois; 8. Medical Literature, Dr. L. P. Yandell, of Kentucky; 9. Hygiene and Social Science, Dr. H. I. Bowditch, of Massachusetts; 10. Mental Hygiene, Dr. John P. Gray, of New York; and 11. Medical Chemistry, Dr. Theo. G. Wormley, of Ohio.

confer with the Commission. From this time until the meeting of the Congress, the project had the warmest support and sympathy of the whole body of the profession in Philadelphia.

The addition of so large a number of new members to the Commission, rendered that body rather unwieldy for the transaction of business, and a Committee of Conference, consisting of members from the various delegations, was therefore chosen for the purpose of harmonizing, as far as possible, different views. This Committee, after a thorough review of the work previously done by the Commission, reported that it would be inexpedient to make any material change in the plan already adopted. It recommended, however, in view of the large amount of labor likely to be imposed upon the secretaries, that two additional secretaries should be appointed. At the subsequent meeting of the Commission, Drs. WILLIAM GOODELL and ROBERT M. BERTOLET were chosen to fill these positions, all the other officers being re-elected by acclamation.

The Committee of Conference having done its work very satisfactorily, it was thought well by the Commission that thenceforward the perfecting of the plan of the Congress should be confided to a COMMITTEE OF ARRANGEMENTS,[1] consisting of the following gentlemen, who were chosen chiefly on account of the interest which they had shown in the project: Drs. S. D. Gross, Edward Hartshorne, Washington L. Atlee, Albert Fricke, Laurence Turnbull, W. W. Keen, I. Minis Hays, J. Solis Cohen, N. L. Hatfield, A. K. Minich, Thomas Geo. Morton, George Strawbridge, William Goodell, John S. Parry, R. G. Curtin, John H. Packard, James H. Hutchinson, Louis A. Duhring, Alfred Stillé, William Thomson, and Daniel G. Brinton. Prof. Gross was chairman of this Committee, and Dr. Wm. B. Atkinson acted as its secretary for the greater part of the time during which it was in existence. The meetings of the Commission were after the appointment of this Committee very infrequent; the larger body being called together only to decide questions of special importance.

A large and influential EXECUTIVE COMMITTEE was also formed about this time, and bore upon its roll the names of many of the most eminent men of the profession in various parts of the Union.

In order to secure as full a representation from foreign lands as possible, a COMMITTEE OF INVITATION was soon after appointed, to which was entrusted the duty of selecting the medical societies abroad, which should be requested to send delegates to the Congress, and also of designating the individual members of the profession, both in this and other countries, who, by the eminence which they had attained in medical science, were entitled to the compliment of a special invitation. A large number of such invitations was sent, and those to gentlemen in this country were with very few exceptions accepted. Unfor-

[1] Beside this Committee, two other Committees were appointed at this time, viz., (1) A Committee of Finance, whose duty it was to collect funds for the entertainment of the delegates; and (2) A Committee of Publication. The first Committee consisted of Drs. Caspar Wister, H. Lenox Hodge, Levi Curtis, Thomas Geo. Morton, T. Hewson Bache, Albert H. Smith, James Tyson, and Charles H. Burnett. The second Committee was never called together, and the selection of a Committee of Publication was thus left for the action of the Congress when it should assemble.

tunately, the loss of time and the great expense involved in a journey to America, prevented as many of our foreign brethren from entertaining the idea of attending the Congress as had been hoped; but the letters which they sent in answer, courteously acknowledged the compliment offered, and were expressive of the regret felt by the writers in being obliged to decline the invitation.[1]

In addition to the members specially invited, in our own country, it was finally agreed that delegates to the Congress should be received from the following bodies: (1) The American Medical Association, one delegate for every State and Territory in the Union; (2) The Association of Medical Superintendents of American Institutions for the Insane, twenty delegates; (3) The State Medical Societies, each as many delegates as its State had representatives in the Congress of the United States; (4) The Medical Staff of the Army, two delegates; (5) The Medical Staff of the Navy, two delegates; (6) The Marine Hospital Service, two delegates; (7) The American Public-Health Association, five delegates; and (8) The American Association for the Cure of Inebriates, two delegates. All the members of the Centennial Medical Commission, including members of the Executive Committee, were considered delegates by virtue of their position in the Commission.

The manner in which the officers of the Congress should be chosen, gave rise to some discussion in the Commission, some of the members holding that it would be a graceful compliment to bestow the Presidency upon a distinguished foreigner; but on the other hand, it was contended that it would be better to allow the Congress to select its own officers, and this view at length prevailed. It was made the duty of the President of the Commission to call the Congress to order when it should first meet, and, after welcoming the delegates, to appoint a Committee of Nominations, consisting of delegates from all parts of the world.

It is, perhaps, needless to add, in concluding this short sketch of the labors of The Centennial Medical Commission of Philadelphia, and of its Committees, that no pains were spared to render the visit of the delegates to the Congress a pleasant one, and that there is every reason to believe that the effort was not unsuccessful.

[1] An invitation was sent by the Commission, in September, 1875, to the International Medical Congress, then in session at Brussels, to hold its next meeting in Philadelphia. While this invitation was courteously declined, the Congress adjourned over until the year 1877, in order to give all its members, who might wish to do so, an opportunity to attend the sessions of the Centennial Congress in this country.

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Isaac Ray, M.D.,
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SECRETARY.

Walter Kempster, M.D.,
of Winnebago, Wisconsin.

LIST OF DELEGATES AND INVITED MEMBERS,

REGISTERED DURING THE MEETING OF THE CONGRESS.

NAME.	DELEGATE FROM
O. D. Abbott, M.D., Manchester, N. H.	State Medical Society.
William Adams, F.R.C.S., London, England.	Medical Society of London.
C. R. Agnew, M.D., New York, N. Y.	State Medical Society.
D. Hayes Agnew, M.D., Philadelphia, Pa.	Centennial Commission.
Harrison Allen, M.D., Philadelphia, Pa.	Centennial Commission.
J. W. Anawalt, M.D., Greensburg, Pa.	State Medical Society.
William Anderson, M.D., Indiana, Pa.	State Medical Society.
Abram B. Arnold, M.D., Baltimore, Md.	Medical and Chirurgical Faculty of Maryland.
John Ashhurst, Jr., M.D., Philadelphia, Pa.	Centennial Commission.
Wm. B. Atkinson, M.D., Philadelphia, Pa.	Centennial Commission.
John L. Atlee, M.D., Lancaster, Pa.	State Medical Society.
Washington L. Atlee, M.D., Philadelphia, Pa.	Centennial Commission.
H. P. Ayres, M.D., Fort Wayne, Indiana.	State Medical Society.
Francis Bacon, M.D., New Haven, Conn.	American Medical Association.
Henry T. Bahnson, M.D., Salem, N. C.	State Medical Society.
William H. Bailey, M.D., Albany, N. Y.	State Medical Society.
Henry B. Baker, M.D., Lansing, Mich.	State Medical Society.
A. S. Baldwin, M.D., Jacksonville, Fla.	State Medical Society.
Fordyce Barker, M.D., New York, N. Y.	State Medical Society.
John Barker, M.D., Dublin, Ireland.	Surgical Society of Ireland.
Robert Barnes, M.D., F.R.C.P., London, England.	Royal Medical and Chirurgical Society, and Obstetrical Society of London.
Gregorio Barroeta, M.D., San Luis Potosi, Mexico.	Medical Society of San Luis Potosi.
Roberts Bartholow, M.D., Cincinnati, Ohio.	State Medical Society.
Edwin W. Bartlett, M.D., Milwaukee, Wis.	State Medical Society.
J. K. Bartlett, M.D., Milwaukee, Wis.	State Medical Society.
J. M. Barton, M.D., Philadelphia, Pa.	Centennial Commission.
Fletcher Beach, M.B., London, England.	Invited member.
F. W. Beard, M.D., Vincennes, Ind.	State Medical Society.
C. E. Beardsley, M.D., Ottawa, Ohio.	State Medical Society.

NAME.	DELEGATE FROM
JOHN BELL, M.D., Montreal, Canada.	Medico-Chirurgical Society of Montreal.
R. M. BERTOLET, M.D., Philadelphia, Pa.	Centennial Commission.
JOHN S. BILLINGS, M.D., U. S. A., Washington, [D. C.	American Public-Health Association.
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F. BOGART, M.D., Sweetwater, Tenn.	State Medical Society.
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JOHN L. BRAY, M.D., Chatham, Ont. Canada.	Western and St. Clair Medical Society.
A. L. BREYSACHER, M.D., Little Rock, Ark.	State Medical Society.
GEO. E. BRICKETT, M.D., Augusta, Me.	State Medical Society.
CHARLES E. BRIGGS, M.D., St. Louis, Mo.	State Medical Society.
JOHN H. BRINTON, M.D., Philadelphia, Pa.	Centennial Commission.
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F. J. BUMSTEAD, M.D., New York, N. Y.	Centennial Commission.
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C. H. BURNETT, M.D., Philadelphia, Pa.	Centennial Commission.
ROBERT BURNS, M.D., Philadelphia, Pa.	Centennial Commission.
DAVID BURPEE, M.D., Philadelphia, Pa.	Centennial Commission.
GEORGE BURR, M.D., Binghamton, N. Y.	State Medical Society.
W. BURT, M.D., Paris, Ont. Canada.	Branch County Medical Association.
SAMUEL C. BUSEY, M.D., Washington, D. C.	Medical Society of the District of Columbia.
W. WEBSTER BUTTERFIELD, M.D., Indianapolis, [Ind.	State Medical Society.
JAMES D. BUTTON, M.D., Auburn, N. Y.	State Medical Society.
WM. H. BYFORD, M.D., Chicago, Ill.	State Medical Society.
A. W. CALHOUN, M.D., Atlanta, Ga.	State Medical Society.

NAME.	DELEGATE FROM
Francis W. Campbell, M.D., Montreal, Canada.	Invited member.
Henry Fraser Campbell, M.D., Augusta, Ga.	State Medical Society.
J. A. Campbell, M.D., Grafton, W. Va.	State Medical Society.
William Canniff, M.D., Toronto, Canada.	Canadian Medical Association.
John T. Carpenter, M.D., Pottsville, Pa.	State Medical Society.
Joseph Carson, M.D., Philadelphia, Pa.	American Medical Association.
Robert Brudenell Carter, F.R.C.S., London, [England.	Royal Medical and Chirurgical Society, and Pathological Society of London.
Franklin D. Castle, M.D., Philadelphia, Pa.	Centennial Commission.
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B. H. Catlin, M.D., West Meriden, Conn.	State Medical Society.
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James Collins, M.D., Philadelphia, Pa.	Centennial Commission.
Edward Cox, M.D., Battle Creek, Mich.	State Medical Society.
Francis D. Cunningham, M.D., Richmond, Va.	State Medical Society.
George Cupples, M.D., San Antonio, Texas.	State Medical Society.
R. G. Curtin, M.D., Philadelphia, Pa.	Centennial Commission.
John Curwen, M.D., Harrisburg, Pa.	Association of Medical Superintendents, etc.
J. M. Da Costa, M.D., Philadelphia, Pa.	Centennial Commission.
J. C. Dalton, M.D., New York, N. Y.	American Medical Association.
John Davis, M.D., Cincinnati, Ohio.	State Medical Society.
Nathan S. Davis, M.D., Chicago, Ill.	American Medical Association.
Richard Davy, F.R.C.S., London, England.	Medical Society of London.
Pierre Debaisieux, M.D., Louvain, Belgium.	Belgian Government.
Charles Denison, M.D., Denver, Col.	State Medical Society.
John R. Dickson, M.D., Kingston, Ont. Canada.	Canada Medical Association.
Henry D. Didama, M.D., Syracuse, N. Y.	State Medical Society.
Stephen Dodge, M.D., Halifax, Nova Scotia.	Dominion Medical Association.
J. Lewis Dorset, M.D., Genito, Va.	State Medical Society.
Greensville Dowell, M.D., Galveston, Texas.	State Medical Society.
Thomas M. Drysdale, M.D., Philadelphia, Pa.	Centennial Commission.
Thomas S. Duffy, M.D., Rutherfordton, N. C.	State Medical Society.
Louis A. Dugas, M.D., Augusta, Ga.	State Medical Society.
J. J. Dugdale, M.D., Montreal, Canada.	Medico-Chirurgical Society of Montreal.
Louis A. Duhring, M.D., Philadelphia, Pa.	Centennial Commission.
R. J. Dunglison, M.D., Philadelphia, Pa.	State Medical Society.

NAME.	DELEGATE FROM
Alexander Dunlap, M.D., Springfield, Ohio.	American Medical Association.
Charles W. Earle, M.D., Chicago, Ill.	State Medical Society.
S. Y. Earle, M.D., St. John, N. B.	Canada Medical Association.
J. C. Eastman, M.D., Hampstead, N. H.	State Medical Society.
James H. Eldredge, M.D., East Greenwich, R.I.	State Medical Society.
William Elmer, M.D., Bridgeton, N. J.	Centennial Commission.
S. Engelsted, M.D., Copenhagen, Denmark.	Medical Society of Copenhagen.
J. A. Estlander, M.D., Helsingfors, Finland.	Invited member.
Joseph A. Eve, M.D., Augusta, Ga.	State Medical Society.
Paul F. Eve, M.D., Nashville, Tenn.	Centennial Commission.
David S. Fairchild, M.D., Ames, Iowa.	State Medical Society.
Cyrus Falconer, M.D., Hamilton, Ohio.	State Medical Society.
P. J. Farnsworth, M.D., Clinton, Iowa.	State Medical Society.
A. G. Field, M.D., Des Moines, Iowa.	State Medical Society.
William Finlay, M.D., Edinburgh, Scotland.	Obstetrical Society of Edinburgh.
Emil Fischer, M.D., Philadelphia, Pa.	Centennial Commission.
George Jackson Fisher, M.D., Sing Sing, N.Y.	State Medical Society.
Simon Fitch, M.D., St. John, N. B., Canada.	Invited member.
Thomas Davis Fitch, M.D., Chicago, Ill.	State Medical Society.
Thomas M. Flandreau, M.D., Rome, N. Y.	State Medical Society.
Austin Flint, M.D., New York, N. Y.	American Medical Association.
Austin Flint, Jr., M.D., New York, N. Y.	State Medical Society.
Daniel E. Foote, M.D., Belvidere, Ill.	State Medical Society.
William H. Ford, M.D., Philadelphia, Pa.	Centennial Commission.
William Fox, M.D., Madison, Wis.	State Medical Society.
Albert Fricke, M.D., Philadelphia, Pa.	Centennial Commission.
John Frissell, M.D., Wheeling, W. Va.	State Medical Society.
F. T. Fuller, M.D., Raleigh, N. C.	Association of Medical Superintendents, etc.
Anatole de Gaine, M.D., St. Petersburg, Russia.	Invited member.
Frederic Henry Gerrish, M.D., Portland, Me.	State Medical Society.
Henry Gibbons, M.D., San Francisco, Cal.	Centennial Commission.
William Goodell, M.D., Philadelphia, Pa.	Centennial Commission.
H. Earnest Goodman, M.D., Philadelphia, Pa.	Centennial Commission.
Thomas W. Gordon, M.D., Georgetown, Ohio.	State Medical Society.
M. W. C. Gori, M.D., Amsterdam, Holland.	Invited member.
J. W. S. Gouley, M.D., New York, N. Y.	State Medical Society.
J. A. Grant, M.D., Ottawa, Canada.	Canada Medical Association.
John P. Gray, M.D., Utica, N. Y.	State Medical Society.
John Green, M.D., St. Louis, Mo.	Centennial Commission.
Traill Green, M.D., Easton, Pa.	Centennial Commission.
Wm. Warren Greene, M.D., Portland, Me.	State Medical Society.
Samuel D. Gross, M.D., Philadelphia, Pa.	Centennial Commission.
S. W. Gross, M.D., Philadelphia, Pa.	Centennial Commission.

NAME.	DELEGATE FROM
Francis M. Gunnell, M.D., U.S.N., Washing- [ton, D. C.	Medical Staff, U. S. Navy.
Josiah Hale, M.D., Owensboro, Ky.	State Medical Society.
Charles C. Hamilton, M.D., Cornwallis, Nova [Scotia.	Canada Medical Association.
Frank Hastings Hamilton, M.D., New York, [N. Y.	American Medical Association, and State Medical Society.
J. W. Hamilton, M.D., Columbus, Ohio.	State Medical Society.
D. W. Hand, M.D., St. Paul, Minn.	State Medical Society.
Edmund Hansen, M.D., Copenhagen, Denmark.	Invited member.
Charles J. Hare, M.D., F.R.C.P., London, [England.	Royal Medical and Chirurgical Society, and Pathological Society of London.
George C. Harlan, M.D., Philadelphia, Pa.	Centennial Commission.
Elisha Harris, M.D., New York, N. Y.	American Public-Health Association.
James F. Harrison, M.D., University of Va.	State Medical Society.
B. F. Hart, M.D., Marietta, Ohio.	State Medical Society.
Andrew Hartman, M.D., Baltimore, Md.	Medical and Chirurgical Faculty of Maryland.
Henry Hartshorne, M.D., Haverford Coll., Pa.	Centennial Commission.
N. L. Hatfield, M.D., Philadelphia, Pa.	Centennial Commission.
I. Minis Hays, M.D., Philadelphia, Pa.	Centennial Commission.
W. I. Heddens, M.D., St. Joseph, Mo.	State Medical Society.
Charles Heitzmann, M.D., New York, N. Y.	Invited member.
Alexander Allan Henderson, M.D., Ottawa, [Canada.	Ottawa Medico-Chirurgical Society.
George E. Hersey, M.D., Manchester, N. H.	State Medical Society.
Charles A. Hewitt, M.D., Red Wing, Minn.	State Medical Society.
Addinell Hewson, M.D., Philadelphia, Pa.	Centennial Commission.
Albert G. Heyl, M.D., Philadelphia, Pa.	Centennial Commission.
E. A. Hildreth, M.D., Wheeling, W. Va.	State Medical Society.
Wm. H. Hingston, M.D., Montreal, Canada.	Invited member.
Homer O. Hitchcock, M.D., Kalamazoo, Mich.	State Medical Society.
Johan Hjort M.D., Christiania, Norway.	Medical Society of Christiania.
Edward M. Hodder, M.D., F.R.C.S., Toronto, [Canada.	Invited member.
H. Lenox Hodge, M.D., Philadelphia, Pa.	Centennial Commission.
John T. Hodgen, M.D., St. Louis, Mo.	American Medical Association.
John H. Hollister, M.D., Chicago, Ill.	State Medical Society.
T. K. Holmes, M.D., Chatham, Ont. Canada.	Western and St. Clair Medical Association.
O. A. Horr, M.D., Lewiston, Me.	State Medical Society.
R. P. Howard, M.D., Montreal, Canada.	Invited member.
John C. Hubbard, M.D., Ashtabula, Ohio.	State Medical Society.
Stephen G. Hubbard, M.D., New Haven, Conn.	Centennial Commission.
R. F. Hudson, M.D., Ballarat, Australia.	Medical Society of Victoria.

NAME.	DELEGATE FROM
PROF. HUETER, M.D., Griefswald, Germany.	Med. Verein für Griefswald.
C. H. HUGHES, M.D., St. Louis, Mo.	Association of Medical Superintendents, etc.
E. W. HUGHES, M.D., Grenada, Miss.	State Medical Society.
J. C. HUGHES, M.D., Keokuk, Iowa.	State Medical Society.
EDWARD R. HUN, M.D., Albany, N. Y.	State Medical Society.
EZRA M. HUNT, M.D., Metuchen, N. J.	State Medical Society.
WILLIAM HUNT, M.D., Philadelphia, Pa.	Centennial Commission.
JOHN C. HUPP, M.D., Wheeling, W. Va.	Centennial Commission.
A. HURD, M.D., Findlay, Ohio.	State Medical Society.
W. S. HUSELTON, M.D., Allegheny City, Pa.	State Medical Society.
ALEXANDER HUTCHINS, M.D., Brooklyn, N. Y.	State Medical Society.
JAMES H. HUTCHINSON, M.D., Philadelphia, Pa.	Centennial Commission.
JOSEPH C. HUTCHISON, M.D., Brooklyn, N. Y.	State Medical Society.
FREDERICK HYDE, M.D., Cortland, N. Y.	State Medical Society.
JOSIAS A. IRELAND, M.D., Louisville, Ky.	State Medical Society.
WM. IRVIN, M.D., Breakneck, Pa.	State Medical Society.
T. ISHIGOURO, M.D., Tokio, Japan.	Japanese Government.
J. B. S. JACKSON, M.D., Boston, Mass.	State Medical Society.
HARVEY JEWETT, M.D., Canandaigua, N. Y.	State Medical Society.
P. A. JEWETT, M.D., New Haven, Conn.	State Medical Society.
A. H. JOHNSON, M.D., Salem, Mass.	State Medical Society.
H. A. JOHNSON, M.D., Chicago, Ill.	American Public-Health Association.
JOHN C. JOHNSON, M.D., Blairstown, N. J.	State Medical Society.
CHRISTOPHER JOHNSTON, M.D., Baltimore, Md.	Centennial Commission.
SAMUEL J. JONES, M.D., Chicago, Ill.	State Medical Society.
L. S. JOYNES, M.D., Richmond, Va.	State Medical Society.
W. W. KEEN, M.D., Philadelphia, Pa.	Centennial Commission.
WALTER KEMPSTER, M.D., Winnebago, Wis.	Association of Medical Superintendents, etc.
H. N. KENDALL, M.D., Quincy, Ill.	State Medical Society.
J. G. KERR, M.D., San Francisco, Cal.	Invited member.
E. L. KEYES, M.D., New York, N. Y.	Invited member.
S. B. KIEFFER, M.D., Carlisle, Pa.	State Medical Society.
G. KIMBALL, M.D., Lowell, Mass.	Invited member.
C. B. KING, M.D., Allegheny City, Pa.	State Medical Society.
R. A. KINLOCH, M.D., Charleston, S. C.	State Medical Society.
CHARLES J. KIPP, M.D., Newark, N. J.	Invited member.
THOMAS S. KIRKBRIDE, M.D., Philadelphia, Pa.	Centennial Commission.
DANIEL H. KITCHEN, M.D., New York, N. Y.	Association of Medical Superintendents, etc.
HERMAN KNAPP, M.D., New York, N. Y.	Invited member.
W. A. KOUKOL DE YASNOPOLSKY, M.D., [St. Petersburg, Russia.	Invited member.
C. LANGE, M.D., Copenhagen, Denmark.	Medical Society of Copenhagen.

NAME.	DELEGATE FROM
E. S. Lemoine, M.D., St. Louis, Mo.	State Medical Society.
James Leslie, M.D., Hamilton, Ont. Canada.	Hamilton Medical and Surgical Society.
James H. Letcher, M.D., Henderson, Ky.	State Medical Society.
Van S. Lindsley, M.D., Nashville, Tenn.	State Medical Society.
Joseph Lister, F.R.S., Edinburgh, Scotland.	University of Edinburgh, and Medico-Chirurgical Society of Edinburgh.
Edward G. Loring, Jr., M.D., New York, N. Y.	Invited member.
William T. Lusk, M.D., New York, N. Y.	Invited member.
Alfred A. Lutkins, M.D., Jersey City, N. J.	State Medical Society.
Thomas Lyon, M.D., Williamsport, Pa.	State Medical Society.
John Duff Macdonald, M.D., Hamilton, [Ont. Canada.	Hamilton Medical and Surgical Society.
William J. McDowell, M.D., Portsmouth, O.	State Medical Society.
Theodore A. McGraw, M.D., Detroit, Mich.	Centennial Commission.
Hunter McGuire, M.D., Richmond, Va.	State Medical Society.
John W. McIlhany, M.D., Warrenton, Va.	State Medical Society.
John H. Mackie, M.D., New Bedford, Mass.	State Medical Society.
John A. McKinnon, M.D., Selma, Ala.	State Medical Society.
Thomas F. McLean, M.D., Goderich, Ont. [Canada.	County of Huron Medical Association.
Hugh F. McNary, M.D., Princeton, Ky.	State Medical Society.
Thos. L. Maddin, M.D., Nashville, Tenn.	American Medical Association.
T. D. Manning, M.D., Waco, Texas.	State Medical Society.
S. Marks, M.D., Milwaukee, Wis.	State Medical Society.
Darius Mason, M.D., Prairie-du-Chien, Wis.	State Medical Society.
Theodore L. Mason, M.D., Brooklyn, N. Y.	American Association for Cure of Inebriates.
Claudius H. Mastin, M.D., Mobile, Ala.	Centennial Commission.
F. F. Maury, M.D., Philadelphia, Pa.	Centennial Commission.
George W. Mears, M.D., Indianapolis, Ind.	Centennial Commission.
J. Ewing Mears, M.D., Philadelphia, Pa.	Centennial Commission.
James Aitken Meigs, M.D., Philadelphia, Pa.	Centennial Commission.
Márcos de J. Melero, M.D., Havana, Cuba.	La Real Sociedad Económica de Amigos del Pais, Havanna.
H. P. Merriman, M.D., Chicago, Ill.	State Medical Society.
Thomas S. Michaels, M.D., Richmond, Va.	State Medical Society.
Julius F. Miner, M.D., Buffalo, N. Y.	State Medical Society.
Andrew K. Minnich, M.D., Philadelphia, Pa.	Centennial Commission.
Francis Minot, M.D., Boston, Mass.	State Medical Society.
H. Miyake, M.D., Tokio, Japan.	Japanese Government.
John F. Monmonier, M.D., Baltimore, Md.	Medical and Chirurgical Faculty of Maryland.
E. M. Moore, M.D., Rochester, N. Y.	American Medical Association.
Geo. R. Morehouse, M.D., Philadelphia, Pa.	Centennial Commission.

NAME.	DELEGATE FROM
Thomas G. Morton, M.D., Philadelphia, Pa.	Centennial Commission.
Alexander B. Mott, M.D., New York, N. Y.	Invited member.
R. B. Mowry, M.D., Allegheny City, Pa.	Invited member.
Paul F. Mundé, M.D., New York, N. Y.	Invited member.
H. J. Murphy, M.D., Chatham, Ont. Canada.	Western and St. Clair Medical Association.
George Murray, M.D., New Glasgow, Nova [Scotia.	Medical Society of Nova Scotia.
Robert D. Murray, M.D., Keywest, Fla.	Centennial Commission.
S. Nagayo, M.D., Tokio, Japan.	Japanese Government.
Andrew Nebinger, M.D., Philadelphia, Pa.	Centennial Commission.
William B. Neftel, M.D., New York, N. Y.	Invited member.
Charles H. Nichols, M.D., Washington, D. C.	Association of Medical Superintendents, etc.
R. J. Nunn, M.D., Savannah, Ga.	State Medical Society.
John A. Octerlony, M.D., Louisville, Ky.	State Medical Society.
Michael O'Hara, M.D., Philadelphia, Pa.	Centennial Commission.
Wm. Oldright, M.D., Toronto, Canada.	Canada Medical Association.
H. L. Orth, M.D., Harrisburg, Pa.	State Medical Society.
J. W. D. Osgood, M.D., Greenfield, Mass.	State Medical Society.
George A. Otis, M.D., U. S. A., Washington, [D. C.	American Medical Association.
John E. Owens, M.D., Chicago, Ill.	State Medical Society.
John H. Packard, M.D., Philadelphia, Pa.	Centennial Commission.
Wm. H. Pancoast, M.D., Philadelphia, Pa.	Centennial Commission.
Edward H. Parker, M.D., Poughkeepsie, N. Y.	State Medical Society.
Joseph Parrish, M.D., Burlington, N. J.	American Association for Cure of Inebriates.
Theophilus Parvin, M.D., Indianapolis, Ind.	State Medical Society.
R. S. Payne, M.D., Lynchburg, Va.	State Medical Society.
Enoch Pearce, Jr., M.D., Steubenville, Ohio.	State Medical Society.
E. R. Peaslee, M.D., New York, N. Y.	Centennial Commission.
William Pepper, M.D., Philadelphia, Pa.	Centennial Commission.
W. H. Philips, M.D., Kenton, Ohio.	State Medical Society.
Gideon L. Platt, M.D., Waterbury, Conn.	State Medical Society.
A. M. Pollock, M.D., Pittsburg, Pa.	American Medical Association.
J. H. Pooley, M.D., Columbus, Ohio.	State Medical Society.
Charles H. Porter, M.D., Albany, N. Y.	State Medical Society.
D. R. Porter, M.D., Kansas City, Mo.	State Medical Society.
Isaac G. Porter, M D., New London, Conn.	Invited member.
Wm. G. Porter, M.D., Philadelphia, Pa.	Centennial Commission.
Alfred C. Post, M.D., New York, N. Y.	Invited member.
Edwin Powell, M.D., Chicago, Ill.	State Medical Society.
David Prince, M.D., Jacksonville, Ill.	State Medical Society.
J. S. Prout, M.D., Brooklyn, N. Y.	Invited member.
Samuel S. Purple, M.D., New York, N. Y.	Invited member.
Sumner Putnam, M.D., Montpelier, Vt.	State Medical Society.

NAME.	DELEGATE FROM
G. Rawson, M.D., Buenos Ayres, Argentine [Republic.	Medical Association of Buenos Ayres.
Isaac Ray, M.D., Philadelphia, Pa.	Association of Medical Superintendents, etc.
L. S. Rayfield, M.D., Jefferson, Texas.	State Medical Society.
A. N. Read, M.D., Norwalk, Ohio.	State Medical Society.
Thomas B. Reed, M.D., Philadelphia, Pa.	Centennial Commission.
James T. Reeve, M.D., Appleton, Wis.	State Medical Society.
James E. Reeves, M.D., Wheeling, W. Va.	Centennial Commission.
Alexander P. Reid, M.D., Halifax, Nova [Scotia, Canada.	Medical Society of Nova Scotia.
Dudley S. Reynolds, M.D., Louisville, Ky.	State Medical Society.
George A. Rex, M.D., Philadelphia, Pa.	Centennial Commission.
Joseph G. Richardson, M.D., Philadelphia, Pa.	Centennial Commission.
T. G. Richardson, M.D., New Orleans, La.	Centennial Commission.
W. L. Richardson M.D., Montrose, Pa.	State Medical Society.
J. M. Ridge, M.D., Camden, N. J.	State Medical Society.
S. D. Risley, M.D., Philadelphia, Pa.	Centennial Commission.
Jacob Roberts, M.D., Philadelphia, Pa.	Centennial Commission.
D. Argyll Robertson, M.D., Edinburgh, Scot-[land.	Medico-Chirurgical Society of Edinburgh.
David Robertson, M.D., Milton, Ont. Canada.	Canada Medical Association.
E. Robillard, M.D., Montreal, Canada.	Invited member.
James D. Robison, M.D., Wooster, Ohio.	State Medical Society.
Thomas F. Rochester, M.D., Buffalo, N. Y.	State Medical Society.
Robert E. Rogers, M.D., Philadelphia, Pa.	Centennial Commission.
A. M. Rosebrugh, M.D., Toronto, Canada.	Canada Medical Association.
J. W. Rosebrugh, M.D., Hamilton, Canada.	Hamilton Medical and Surgical Society.
James Ross, M.D., Toronto, Canada.	Canada Medical Association.
John D. Ross, M.D., Williamsburg, Pa.	State Medical Society.
J. B. Rottot, M.D., Montreal, Canada.	Invited member.
Michael Rudnew, M.D., St. Petersburg, Russia.	Russian Government.
W. S. W. Ruschenberger, M.D., U. S. N., [Philadelphia, Pa.	Centennial Commission.
Gurdon W. Russell, M.D., Hartford, Conn.	State Medical Society.
Ira Russell, M.D., Winchendon, Mass.	State Medical Society.
John W. Russell, M.D., Mt. Vernon, Ohio.	State Medical Society.
Thomas P. Russell, M.D., Oshkosh, Wisconsin.	State Medical Society.
Thomas Edward Satterthwaite, M.D., New [York, N. Y.	Centennial Commission.
William Savery, M.D., Bryn Mawr, Pa.	State Medical Society.
Lewis A. Sayre, M.D., New York, N. Y.	Centennial Commission.
Henry S. Schell, M.D., Philadelphia, Pa.	Centennial Commission.
James M. Scott, M.D., St. Louis, Mo.	State Medical Society.
W. Scott, M.D., Cleveland, Ohio.	State Medical Society.
Edward Seguin, M.D., New York, N. Y.	American Medical Association.

NAME.	DELEGATE FROM
AMOS SEIP, M.D., Easton, Pa.	State Medical Society.
FREDERICK SEMELEDER, M.D., Vienna, Austria.	Invited member.
N. SENN, M.D., Milwaukee, Wis.	State Medical Society.
LEOPOLD SERVAIS, M.D., Antwerp, Belgium.	Belgian Government.
EDWARD O. SHAKESPEARE, M.D., Philadelphia, [Pa.	Centennial Commission.
CHARLES SHEPARD, M.D., Grand Rapids, Mich.	State Medical Society.
B. F. SHERMAN, M.D., Ogdensburg, N. Y.	State Medical Society.
EDWARD SHIPPEN, M.D., U.S.N., Philadelphia, [Pa.	Medical Staff, United States Navy.
J. H. SHOUT, M.D., Las Vegas, New Mexico.	Medical Society of New Mexico.
GEORGE E. SHUTTLEWORTH, M.D., Lancaster, [England.	Invited member.
ALEXANDER R. SIMPSON, M.D., Edinburgh, [Scotland.	University of Edinburgh, and Obstetrical Society of Edinburgh.
WHARTON SINKLER, M.D., Philadelphia, Pa.	Centennial Commission.
HENRY M. SKILLMAN, M.D., Lexington, Ky.	State Medical Society.
ALBERT H. SMITH, M.D., Philadelphia, Pa.	Centennial Commission.
ASHBEL SMITH, M.D., Houston, Texas.	State Medical Society.
DAVID P. SMITH, M.D., Springfield, Mass.	State Medical Society.
EUGENE SMITH, M.D., Detroit, Michigan.	State Medical Society.
HEBER SMITH, M.D., Stapleton, N. Y.	United States Marine-Hospital Service.
J. LEWIS SMITH, M.D., New York, N. Y.	Centennial Commission.
JOEL W. SMITH, M.D., Charles City, Iowa.	State Medical Society.
JOSEPH R. SMITH, M.D., U. S. A., Fort Monroe, [Va.	Medical Staff, United States Army.
FRANCIS G. SMYTH, M.D., Philadelphia, Pa.	Centennial Commission.
EDWIN M. SNOW, M.D., Providence, R. I.	Centennial Commission.
S. FLEET SPEIR, M.D., Brooklyn, N. Y.	Invited member.
H. N. SPENCER, M.D., St. Louis, Mo.	State Medical Society.
EDWARD R. SQUIBB, M.D., Brooklyn, N. Y.	State Medical Society.
A. J. STEELE, M.D., St. Louis, Mo.	State Medical Society.
LEWIS H. STEINER, M.D., Frederick City, Md.	American Public-Health Association, and Medical and Chirurgical Faculty of Maryland.
GEORGE T. STEVENS, M.D., Albany, N. Y.	Invited member.
JAMES A. STEUART, M.D., Baltimore, Md.	Medical and Chirurgical Faculty of Maryland.
J. L. STEWART, M.D., Erie, Pa.	State Medical Society.
ALFRED STILLÉ, M.D., Philadelphia, Pa.	Centennial Commission.
JOSEPH A. STILWELL, M.D., Brownstown, Ind.	State Medical Society.
BARTON WARREN STONE, M.D., Hopkinsville, [Ky.	Association of Medical Superintendents, etc.

NAME.	DELEGATE FROM
George Strawbridge, M.D., Philadelphia, Pa.	Centennial Commission.
S. S. Stryker, M.D., Philadelphia, Pa.	Centennial Commission.
Absalom B. Stuart, M.D., Winona, Minn.	State Medical Society.
D. F. Stuart, M.D., Houston, Texas.	State Medical Society.
G. E. Sussdorff, M.D., Macon, Georgia.	State Medical Society.
George Sutton, M.D., Aurora, Ind.	State Medical Society.
Joseph Swartz, M.D., Duncannon, Pa.	State Medical Society.
V. H. Taliaferro, M.D., Atlanta, Ga.	State Medical Society.
B. W. Taylor, M.D., Columbia, S. C.	State Medical Society.
M. A. Taylor, M.D., Austin City, Texas.	State Medical Society.
R. W. Taylor, M.D., New York, N. Y.	Centennial Commission.
Lake J. Tefft, M.D., Syracuse, N. Y.	State Medical Society.
S. H. Tewksbury, M.D., Portland, Me.	State Medical Society.
William Thomson, M.D., Philadelphia, Pa.	Centennial Commission.
James Thorburn, M.D., Toronto, Canada.	Invited member.
F. Walton Todd, M.D., Stockton, Cal.	State Medical Society.
S. S. Todd, M.D., Kansas City, Mo.	State Medical Society.
Joseph M. Toner, M.D., Washington, D. C.	American Public-Health Association.
Edward H. Trenholme, M.D., Montreal, Canada.	Medico-Chirurgical Society of Montreal.
Jolliffe Tufnell, F.R.C.S., Dublin, Ireland.	Surgical Society and Pathological Society of Ireland.
Horace Tupper, M.D., Bay City, Mich.	State Medical Society.
Charles S. Turnbull, M.D., Philadelphia, Pa.	Centennial Commission.
Laurence Turnbull, M.D., Philadelphia, Pa.	State Medical Society.
Edward Berriam Turnipseed, M.D., Columbia, S. C.	State Medical Society.
James Tyson, M.D., Philadelphia, Pa.	Centennial Commission.
G. Underwood, M.D., Pittston, Pa.	State Medical Society.
William H. VanBuren, M.D., New York, N. Y.	Invited member.
Joseph H. Vandeman, M.D., Chattanooga, Tenn.	State Medical Society.
S. Oakley Vanderpoel, M.D., Stapleton, N. Y.	State Medical Society.
Arthur Van Harlingen, M.D., Philadelphia, Pa.	Centennial Commission.
Thomas H. Van Valzah, M.D., Lewiston, Pa.	State Medical Society.
Theodore R. Varick, M.D., Jersey City, N. J.	Centennial Commission.
Alexander M. Vedder, M.D., Schenectady, N. Y.	State Medical Society.
H. D. Vosburgh, M.D., Lyons, N. Y.	State Medical Society.
Clement A. Walker, M.D., Boston, Mass.	Association of Medical Superintendents, etc.
James P. Walker, M.D., Mason City, Ill.	State Medical Society.
Charles D. Watson, M.D., Covington, Ind.	State Medical Society.
Wm. Watson, M.D., Dubuque, Iowa.	State Medical Society.
W. Murray Weidman, M.D., Reading, Pa.	State Medical Society.
Faneuil D. Weisse, M.D., New York, N. Y.	Centennial Commission.

NAME.	DELEGATE FROM
James C. White, M.D., Boston, Mass.	Centennial Commission.
James P. White, M.D., Buffalo, N. Y.	State Medical Society.
Luther C. White, M.D., Van Buren, Ark.	State Medical Society.
George Wilkins, M.D., Montreal, Canada.	Canada Medical Association.
De Forest Willard, M.D., Philadelphia, Pa.	Centennial Commission.
E. Williams, M.D., Cincinnati, Ohio.	State Medical Society.
Henry W. Williams, M.D., Boston, Mass.	Invited member.
J. M. Willis, M.D., Waco, Texas.	State Medical Society.
Ellwood Wilson, M.D., Philadelphia, Pa.	Centennial Commission.
F. N. Wise, M.D., Covington, Ky.	Invited member.
Caspar Wister, M.D., Philadelphia, Pa.	Centennial Commission.
Horatio C. Wood, Jr., M.D., Philadelphia, Pa.	Centennial Commission.
H. D. Wood, M.D., Angola, Ind.	State Medical Society.
Alfred H. Woodill, M.D., Halifax, Nova [Scotia.	Halifax County Medical Society.
Ashbel Woodward, M.D., Franklin, Conn.	State Medical Society.
J. J. Woodward, M.D., U. S. A., Washington, [D. C.	Medical Staff, United States Army.
John M. Woodworth, M.D., Washington, D. C.	Centennial Commission.
Algernon Woolverton, M.D., Hamilton, Ont. [Canada.	Hamilton Medical and Surgical Society.
Theo. G. Wormley, M.D., Columbus, Ohio.	Centennial Commission.
Fred. H. Wright, M.D., Toronto, Ont., Canada.	Canada Medical Association.
Henry P. Wright, M.D., Ottawa, Canada.	Ottawa Medical Chirurgical Society.
R. F. Wright, M.D., Dalton, Ga.	State Medical Society.
Lunsford P. Yandell, M.D., Louisville, Ky.	State Medical Society.
H. P. Yeomans, M.D., Mt. Forest, Ont. Canada.	Canada Medical Association.

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SECTION ON SANITARY SCIENCE.

SECTION ON MENTAL DISEASES.

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ADDRESS OF WELCOME.

BY

S. D. GROSS, M.D., LL.D., D.C.L. Oxon.,
PRESIDENT OF THE CENTENNIAL MEDICAL COMMISSION.

Gentlemen of the International Medical Congress:—

My colleagues have confided to me, as President of the Centennial Medical Commission, the agreeable and honorable duty of opening this International Medical Congress, so long the object of their solicitude and earnest labor. In their name, then, as well as in my own and in that of the entire American medical profession, whose great heart this day throbs in unison with ours, I extend to you the right hand, and bid you a thrice cordial welcome to the "City of Brotherly Love." The occasion which has brought us together this morning is one of no ordinary kind; it is one also which has been long and, I may add, anxiously anticipated. It might, perhaps, seem ungracious if I were to tell you how much time and labor have been bestowed by the Commission, through its Committee of Arrangements, upon the organization of the Congress; how often it has met to devise plans and interchange views; how earnestly and thoughtfully it has performed its work; in a word, how faithfully and conscientiously it has discharged the great trust confided to it by the different medical bodies of the city and county of Philadelphia, in which, nearly two years ago, the Congress originated. Not a little embarrassment has often attended our progress; and it has, therefore, not been without a profound sense of relief, such as a weary traveller may be supposed to experience at the end of a long and tedious journey, that at length we have found that our task has been brought to a successful close. If the organization is less complete than to some of you it may seem to be, no blame will, I am sure, be ascribed to the Commission on account of any shortcomings. There might, possibly, have been wiser and more experienced heads at work; but warmer hearts, or more conscientious men, never were, I venture to affirm, engaged in a noble enterprise. Such, then, as the work is, we cordially submit it to your consideration, satisfied that it will be accepted by you in the same kindly spirit in which it is tendered, and that any deficiencies which may mar its character will be duly rectified by your superior wisdom.

It is at all times a source of gratification to welcome friends, especially when they are united by the bonds of a common brotherhood; but on this occasion, so pregnant with important events, the feeling is vastly heightened by the fact that we have assembled around us brethren not only from every section of this great continent, but from various foreign climes—from the British Dominion, from England, Ireland, and Scotland, from France, Belgium, and Germany, from Russia, Sweden, Norway, and Finland, from Greece, from China and

Japan, from Australia, from Mexico and South America, and, I had almost said, from every country in the world. It is interesting to know that some of these delegates come here accredited by their respective governments. The invitations sent out by the Commission covered every prominent medical society and every distinguished medical man in the four quarters of the globe. The object was to bring together representative men from all nationalities to participate in our proceedings, and to afford us the benefits of their wisdom, and the results of their experience and scientific investigations. If all these, or even a respectable minority of these, representative men could have been here, what a glorious spectacle would be presented in this hall this morning! Men laying aside for a while their ordinary pursuits, crossing vast continents and perilous seas, congregating to unite with us in celebrating our first Medical Centennial, in interchanging cordial salutations, in deliberating upon the best means of promoting the dearest and holiest interests of our profession, and in laying their contributions, the accumulations of years of study and observation, upon a common altar for the common good! In its wide range, the present Congress is without a parallel. Similar bodies have repeatedly met, but none on so grand a scale, or with such a cosmopolitan outlook.

In organizing the Congress, the Commission may have been guilty of undue partiality towards its own country; and yet, perhaps, such a tendency was, after all, only natural. However this may be, certain members felt an irresistible desire to show the world what the country, since the establishment of our independence as a free and sovereign people, had accomplished for scientific medicine. For this purpose, topics illustrative of the progress and present condition of the different branches of medicine in the United States, have been assigned, as subjects for addresses, to gentlemen of acknowledged rank in the profession in different sections of the Union. These exercises will, it is believed, add greatly to the interest of the occasion. Time was, and that not at all remote, when we had no medical literature—no medical science—and but few medical schools—when we were utterly helpless, and wholly dependent upon the aid derived from our European brethren, especially the English, whose language, practice, and habits, we naturally made our own. The poverty of the country, in these respects, cannot be better illustrated than by the fact that we had no native works on medicine and the collateral sciences until after the commencement of the present century. Many of you will recall the words of the great English lexicographer who, in 1769, in speaking to a friend of the American colonies, exclaimed: "Sir, they are a race of convicts, and ought to be thankful for anything we allow them short of hanging." The Abbé Raynal, writing in the latter part of the last century, declared that America had not yet produced a single man of genius; and the question of a celebrated English writer: "Who reads an American book, or goes to an American play, or looks at an American picture or statue?" uttered at a more recent period, is still fresh in the memory of many of the present race of men. The discourses which will be delivered before you on the progress of American Medicine, will serve to show that the profession of the United States has earned for itself an enviable reputation, and that it is fully abreast of all the other pursuits that adorn the human mind and shed lustre upon the scientific character of the nation. They will serve to show that we have passed the stage of medical provincialism, and that we stand upon a

lofty platform, to which we need not be ashamed to invite the representative men of the profession of foreign countries, however illustrious, or however advanced in the arts of civilization.

The different Sections, organized by the Commission, must speak for themselves. It is in them that the work of the Congress is mainly to be done, in them that the interchange of scientific ideas is to be perfected, and from them that the meeting is to derive its chief glory as an international body of educated and enlightened men.

It will be recollected that attempts have been made of late years, in different quarters, to establish a uniformity of scientific nomenclature, of weights, measures, and records of disease for the medical profession in all parts of the civilized world. The plan, if carried out, could not fail to advance, in an eminent degree, the interests of medical science; and I am happy to be able to state that it is proposed to discuss the subject fully in one of the Sections.

We are upon the threshold of a new centennial era. One hundred years have passed away since the grand old bell upon Independence Hall announced to the world the birth of a new nation and liberty, not only to our own citizens, but to all the peoples of the earth. The century that has just elapsed is the most wonderful in all that pertains to human progress, to discovery, to inventions, to improvements, to refinement and intellectual culture; in short, to all that exalts and ennobles human nature, in its various aspects and phases, that has been vouchsafed to man since God said "Let there be light." The science of medicine has been completely revolutionized within our day. The saying, "Old things are passed away; behold all things are become new," has literally been fulfilled. The microscope, chemical analysis, clinical observation, and experiments upon the inferior animals, are leading on the medical mind with wondrous velocity in the pursuit of knowledge, and are daily adding new facts to our stock of information far beyond what the wildest fancy could have conceived, even a third of a century ago. Dogmatism, once so dominant in the schools, has ceased to exist, and no unacknowledged theories are any longer received or entertained by the scientist. Facts, resting upon the broad basis of observation and experiment, repeated and varied in a thousand ways, alone are relied upon as worthy of acceptance and as safe guides in practice. Hippocratic medicine is the order of the day. Everything bows before its divine behests.

In every corner of the habitable globe, penetrated by the light of civilization, busy, active minds, endowed with high culture and actuated by the noblest resolves, are at work, exploring the mysteries of disease, and devising means or methods of treatment for the relief of suffering, and the prolongation of life. The busy bee was never more industriously engaged in gathering honey from the flower of the field than the modern physician is in gathering knowledge at the bedside of the sick, and garnering it for future use. Much of what is considered by many as established, must be reviewed in the light of modern science; new avenues must be opened; and the ball, composed of myriads of threads more delicately formed than any ever spun by Penelope, must be pushed onward and upward by the united efforts of the medical profession in all parts of the world. How far the Centennial International Medical Congress shall promote these desirable objects, time alone can determine. It may safely be predicted that, if it do not fulfil all the promises of hope that are expected from it, it will at all

events accomplish a vast deal of useful work, and thus afford the world an earnest of its interest in the advancement of scientific medicine, and in international unity and kindly feeling. Science can have no higher mission than that of strengthening the bonds and securing the co-operation of its votaries in various portions of the globe, assembled to deliberate upon everything calculated to promote its holiest interests.

Among the many objects of an International Congress, not the least is the interchange of kindly feelings on the part of its members, the formation of new friendships, and the cementing of old ties. It is well that men of different nationalities should occasionally come together, to look at one another, to shake hands, and to see how they stand in public estimation, as well as in their own; what the world thinks of them, and what they think of the world; what they have done to further the interests of scientific progress, to lighten the burdens of human suffering, and to extend the boundaries of human happiness. All these and many other things, which need not to be here specified, are objects well worthy of engaging earnest attention on such an occasion as this.

It has often occurred to me that, if these international reunions were more frequent and more largely attended, they would be a vast deal more serviceable in preventing war and international misunderstandings than any arbitrations that could be inaugurated for the settlement of international difficulties. Much of the pleasant feeling at present existing between the United States and Europe is due to the enlarged intercourse which has been going on since the establishment of steam navigation between the two countries, and the consequent interchange of hospitality and courtesy on the part of their citizens. It is, therefore, to be hoped that this may be only one of many such reunions on this side of the Atlantic.

It needs hardly to be stated that the medical profession and the citizens of Philadelphia will do all that they can to make your time pass pleasantly, as well as profitably, during your sojourn among us. Cards of invitation will be issued to you to inspect the various institutions of interest in and around the city; and, after the work of the Congress is over, the International Exposition will no doubt claim, as it assuredly deserves, the earnest attention of every member of this body. And now that the labor of the Centennial Medical Commission is completed, it only remains for the Congress, which I now declare open, to perfect its organization by the election of officers for its own government. To facilitate this object the Commission has appointed a committee on nominations, consisting of thirteen members, nine American and four foreign, whose duty it will be to report the result of their deliberations at the earliest possible moment this morning.

MINUTES.

THE Congress assembled at noon of Monday, September 4, 1876, in the Chapel of the University of Pennsylvania. Dr. SAMUEL D. GROSS, President of the Centennial Medical Commission, called the Congress to order, and introduced the Right Reverend WILLIAM BACON STEVENS, D.D., LL.D., Bishop of the Protestant Episcopal Church in the Diocese of Pennsylvania, who opened the session with prayer. Dr. W. S. W. RUSCHENBERGER, U. S. N., one of the Vice-Presidents of the Centennial Medical Commission, having been then called to the Chair, Dr. Gross read an Address of Welcome (see page xxxiii.), after which he announced the appointment of the following Committee of Nomination:—William Adams, Esq., of England; Prof. Engelsted, of Denmark; Prof. Hueter, of Germany; Prof. Rudnew, of Russia; Dr. J. A. Grant, of Canada; Dr. H. I. Bowditch, of Massachusetts; Prof. L. A. Dugas, of Georgia; Prof. J. T. Hodgen, of Missouri; Prof. Christopher Johnston, of Maryland; Prof. Austin Flint, of New York; Dr. W. S. W. Ruschenberger, U. S. Navy; Dr. J. R. Smith, U. S. Army; Dr. Edwin M. Snow, of Rhode Island.

Dr. AUSTIN FLINT, of New York, then delivered an Address on Medicine and Medical Progress in the United States (see page 1).

The Committee of Nominations presented the following partial report of nominations for Officers of the Congress:—

President—Dr. S. D. Gross, Philadelphia.

Vice-Presidents—Dr. Paul F. Eve, Tennessee; Jolliffe Tufnell, Esq., Ireland; Dr. W. L. Atlee, Pennsylvania; Dr. C. Lange, Denmark; Dr. T. G. Richardson, Louisiana; Dr. W. H. Hingston, Canada; Dr. J. P. White, New York; Dr. H. Miyake, Japan; Dr. N. R. Smith, Maryland; Dr. M. Rudnew, Russia; Dr. J. M. Toner, District of Columbia; Dr. Hueter, Germany; Dr. J. B. Johnson, Missouri; Dr. F. Semeleder, Austria; Dr. Hunter McGuire, Virginia; Dr. Johan Hjort, Norway; Dr. G. L. Collins, Rhode Island; Dr. R. F. Hudson, Australia; Dr. H. Gibbons, California; Dr. Pierre De Baisieux, Belgium; Dr. N. S. Davis, Illinois; William Adams, Esq., England; Dr. L. A. Dugas, Georgia; Dr. A. R. Simpson, Scotland; Dr. J. K. Bartlett, Wisconsin.

Honorary Vice-Presidents—Dr. Joseph K. Barnes, Surgeon-General U. S. A.; Dr. Joseph Beale, Surgeon-General U. S. N.

Secretary-General—Dr. I. Minis Hays, Philadelphia.

Secretaries of Meeting—Drs. William B. Atkinson, R. J. Dunglison, R. A. Cleemann, W. W. Keen, R. M. Bertolet.

Section on Medicine—President, Dr. Alfred Stillé, Philadelphia; Secretary, Dr. J. Ewing Mears, Philadelphia.

Section on Biology—President, Dr. J. C. Dalton, New York; Secretary, Dr. James Tyson, Philadelphia.

Section on Surgery—President, Joseph Lister, Esq., Edinburgh; Secretary, Dr. J. H. Packard, Philadelphia.

Section on Dermatology and Syphilography—President, Dr. J. C. White, Boston; Secretary, Dr. A. Van Harlingen, Philadelphia.

Section on Obstetrics—President, Dr. Robert Barnes, London; Secretary, Dr. William Goodell, Philadelphia.

Section on Ophthalmology—President, R. Brudenell Carter, Esq., London; Secretary, Dr. John Green, St. Louis.

Section on Otology—President, Dr. L. Turnbull, Philadelphia; Secretary, Dr. H. N. Spencer, St. Louis.

Section on Sanitary Science—President, Dr. Stephen Smith, New York; Secretary, Dr. E. M. Hunt, New Jersey.
Section on Mental Diseases—President, Dr. J. P. Gray, Utica; Secretary, Dr. W. Kempster, Wisconsin.

The report was accepted, and the nominations confirmed.

Dr. GROSS then assumed the chair as President of the Congress. He thanked that body for the honor conferred upon him, and said that no other would be dearer to him during the remainder of his life than that of presiding over its deliberations. He considered it not solely an honor bestowed on him, but a tribute to the profession of Philadelphia, which had been instrumental in organizing this Congress.

On motion the Congress then adjourned until Tuesday at 10 A. M.

SECOND DAY'S SESSION.

The President called the Congress to order at 10 A. M. The minutes of the preceding day were read and approved. The Secretary-General presented a list of nearly three hundred delegates who had been registered up to this date.

Reports were then received from the various Sections, and, on motion, the conclusions adopted by them, after debate upon the topics assigned for discussion, were taken up *seriatim*, and adopted by the Congress, as follows:—

From the SECTION ON MEDICINE:—The Section considered the question, "Typho-Malarial Fever; is it a Special Type of Fever?" The debate was opened by J. J. WOODWARD, M.D., Surgeon U. S. A., and the following conclusion was adopted by the Section:—

Typho-malarial fever is not a special or distinct type of disease, but the term may be conveniently applied to the compound forms of fever which result from the combined influence of the causes of the malarial fevers and of typhoid fever.

From the SECTION ON DERMATOLOGY AND SYPHILOGRAPHY:—The consideration of the subject, "Variations in Type and in Prevalence of Diseases of the Skin in different countries of equal civilization," was introduced by JAMES C. WHITE, M.D., of Boston, and the following conclusions were adopted:—

1. Certain obscure affections, the etiology of which is little if at all understood, even in those parts of Europe to which they are mostly confined, may be regarded as practically non-existent among us—such are *prurigo*, *pellagra*, and *lichen exudativus ruber*.
2. Certain diseases, directly connected with and dependent upon poverty and habits of personal uncleanliness, are less prevalent in the United States than in those parts of Europe of which we have sufficient statistical information for comparison. Examples of this class are the *animal parasitic affections* especially.
3. Some cutaneous affections of grave character, which are dependent upon or a part of serious constitutional disorders, are of less frequent occurrence and of milder type amongst us than in Europe in general, or those parts of it where they are endemic. *Lupus*, the *syphilodermata*, and *leprosy* are the most marked instances of this class.
4. Certain disorders of the skin, especially those of its glandular system, and those connected more immediately with its nervous system, are apparently more prevalent with us than in Europe. The most notable examples of the former are *seborrhœa*, *acne*, and possibly the heat rashes; of the latter, *herpes*, *urticaria*, and *pruritus*.

In addition to the above-mentioned conclusions, the following additional proposition was adopted by the Section:—

5. The type of certain acute congestive and nervous diseases of the skin is more severe in this country than abroad.

From the SECTION ON OPHTHALMOLOGY:—A discussion on "The Comparative Value of Caustics and Astringents in the Treatment of Diseases of the Conjunctiva, and the best mode of applying them," was introduced by HENRY W. WILLIAMS, M.D., of Boston, and the following conclusions were adopted by the Section:—

1. In a considerable number of essentially transient affections of the conjunctiva, and in pterygium or other growths, no active treatment by caustics or astringents is required.
2. When disease affects only a limited portion of the conjunctiva, as in phlyctenular inflammation, the mildest stimulating or astringent remedies are usually sufficient.

3. In the acute and chronic forms of general conjunctivitis, astringents are, as a rule, safer as well as more efficacious than caustics, and are therefore better adapted to the requirements of the general practitioner.

From the SECTION ON OTOLOGY:—The subject of the "Importance of Treatment of Aural Diseases in their early stages, especially when arising from the Exanthemata," was presented by Dr. ALBERT H. BUCK, of New York, and the following conclusions were adopted by the Section:—

1. Chronic Otorrhœa is at the present time a very common disease, due in most cases to the want of proper treatment during the acute stage of the affection.
2. It is by no means a harmless affection.
3. It may be fairly classed as a preventable disease; at least among those who possess a healthy constitution.
4. Paracentesis of the membrana tympani, if resorted to during the first few days of the acute attack, and if not carried out too timidly, *i. e.*, if a free incision be made and not a mere prick, is almost a sure preventive of the subsequent chronic disease.
5. The profession at large, and especially the medical schools, should give the subject more earnest thought than they have in the past.

From the SECTION ON SANITARY SCIENCE:—"The Present Condition of the Evidence concerning Disease-germs," was reported upon by Dr. THOMAS E. SATTERTHWAITE, of New York, and the following conclusions were adopted:—

1. As far as inquiry has been made as to the results of the active principles in infective diseases, it is probable that in a certain number the matter is particulate or molecular in form, and, in the instances named, in no sense a soluble substance.
2. In regard to the causes of septicæmia, pyæmia, puerperal fever, erysipelas, and hospital gangrene, and of cholera, smallpox, the carbuncular diseases of men and animals, typhoid and relapsing fevers, and diphtheria, there is not satisfactory proof that they are necessarily connected with minute vegetable organisms.
3. The real nature of these causes is still uncertain.

Dr. LAURENCE TURNBULL, of Philadelphia, announced that, with a desire to give the posts of honor to gentlemen from a distance, he had resigned the position of President of the Section on Otology, and that Dr. CLARENCE J. BLAKE, of Boston, had been chosen by the Section to preside over its deliberations.

Dr. T. G. RICHARDSON, of New Orleans, moved that the Congress should hold itself in no way responsible for the conclusions adopted by the Sections, but that the whole responsibility in the matter should rest with the Sections themselves.

On motion of Dr. N. S. DAVIS, of Chicago, this resolution was amended, so that, upon the submission of the reports of the Sections to the Congress, the question should be merely upon their acceptance and reference for publication.

On motion of Dr. WILLIAM BRODIE, of Michigan, the previous action of the Congress in adopting the conclusions reported by the several Sections, was reconsidered, and the reports accepted and referred to the Committee of Publication.

The Secretary-General then read letters of greeting from the IMPERIAL SOCIETY OF ST. PETERSBURG, Russia, and from the UNIVERSITY OF CHRISTIANIA, Norway. He also read the following letter from Dr. J. S. BILLINGS, U. S. Army:—

WAR DEPARTMENT, SURGEON-GENERAL'S OFFICE,
WASHINGTON, D. C., July 31, 1876.

SIR: By direction of the Surgeon-General, U. S. Army, I have the honor to forward herewith to your address six (6) copies of a specimen fasciculus of a catalogue of the library of this office, and to request that they may be presented to the International Medical Congress. While the object in view in forming this library has been to make a collection of sufficient extent and completeness to meet the wants of the physicians of the United States, an attempt is being made to prepare a catalogue and index of its contents, whose practical usefulness shall not be confined to this country, but shall be, as far as the material

available will permit, international and cosmopolitan. The manuscript of the entire work is so nearly completed that the printing can be commenced next spring if Congress shall see fit to give the necessary authority.

Very respectfully, your obd't servant,

JOHN S. BILLINGS,
Assist. Surgeon U. S. Army, in charge of Library.

Whereupon Dr. AUSTIN FLINT, of New York, offered the following preamble and resolutions, which were adopted:—

Whereas, The institution of a National Library containing all the important bibliographical and periodical publications relating to medicine and the collateral sciences, in the past and present time, is of importance, not alone to the medical profession, but to persons in other pursuits who may desire to refer to works treating of topics embraced in these departments of knowledge, and also concerns greatly the public welfare in so far as this is involved in the elevation of the standard of medical education; and

Whereas, Through the wisdom of the National Legislators of the United States Government, a medical library has been established, containing, at the present time, about 40,000 volumes, and about the same number of single pamphlets; and

Whereas, Experience has already shown the practical advantages of the present library, and, at the same time, the great need of its being made more complete by increasing at least tenfold the number of publications which it now contains; and

Whereas, In order to render such a library available for reference, especially to medical men and others residing at a distance, a catalogue wherein publications are classified after the names of authors and subjects, the scope of each publication being stated, is indispensable; and

Whereas, Such a catalogue of the National Medical Library has been prepared under the direction of the Surgeon-General of the United States Army by Assistant Surgeon J. S. Billings, copies of a specimen fasciculus having been forwarded for examination by the members of the International Medical Congress; therefore,

Resolved, First. That the members of this International Medical Congress regard with great interest the institution of a National Medical Library in the City of Washington, and respectfully petition the Congress of the United States to provide for additions to the number of volumes and periodical publications, until the library is made as complete as possible.

Second. That in view of the necessity of what is known as a *Catalogue raisonné*, in order to render the library properly available for reference, this International Medical Congress urges the importance of an early completion and publication of such a catalogue.

Third. That the Specimen Fasciculus of the Catalogue, which is stated to be nearly ready for the press, affords evidence of great labor and care, and the arrangements for convenience of reference it is believed will prove in all respects satisfactory.

Fourth. That those of the delegates to this International Medical Congress who are citizens of the United States, and other members of the Medical Profession in this country, are urged individually to exert their influence to secure the enlargement of the library, and the speedy publication of the Catalogue.

The Committee of Nominations then presented the following supplementary report, which was accepted, and the nominations confirmed:—

Committee of Publication (with power to choose its chairman and an editor)—Dr. John Ashhurst, Jr., Dr. Richard J. Dunglison, Dr. William Goodell, Dr. James H. Hutchinson, Dr. Caspar Wister, all of Philadelphia.

Treasurer—Dr. Caspar Wister, Philadelphia.

Vice-Presidents of the Sections.

Section on Medicine—Dr. R. P. Howard, Canada; Dr. J. J. Woodward, U. S. Army.

Section on Biology—Dr. Austin Flint, Jr., New York; Dr. F. W. Campbell, Canada.

Section on Surgery—Dr. J. A. Grant, Canada; Dr. J. Ashhurst, Jr., Philadelphia.

Section on Dermatology and Syphilography—Dr. S. Engelsted, Copenhagen; Dr. E. Shippen, U. S. Navy.

Section on Obstetrics—Dr. Alexander R. Simpson, Edinburgh; Dr. W. H. Byford, Illinois.

Section on Ophthalmology—Dr. William Thomson, Philadelphia; Dr. Henry W. Williams, Boston.

Section on Otology—Dr. A. H. Buck, New York.

Section on Sanitary Science—Dr. J. S. Billings, U. S. Army.

Section on Mental Diseases—Dr. E. Grissom, North Carolina; Dr. I. Ray, Philadelphia.

Dr. HENRY I. BOWDITCH, of Massachusetts, then read an Address on Hygiene and Preventive Medicine (see page 21), Vice-President PAUL F. EVE, of Tennessee, occupying the Chair; and afterwards Dr. THEO. G. WORMLEY, of Ohio, delivered an Address on Medical Chemistry and Toxicology (see page 49).

The Congress then adjourned until Wednesday at 10 A. M.

THIRD DAY'S SESSION.

The Congress met at 10 A. M.; the President in the Chair. The minutes of the previous day were read and approved.

On motion of Dr. JOHN L. ATLEE, of Lancaster, Pa., the Secretary was instructed to transmit copies of the Address of Dr. Bowditch, when printed, to the Governor of each State and Territory, with the request that it be transmitted to the Legislature of such State or Territory; and, on motion of Dr. TRENHOLME, of Montreal, the government of the Dominion of Canada and those of its several provinces were included in the terms of the resolution.

On motion of Dr. TRAILL GREEN, of Easton, Pa., Dr. Wormley's Address was referred to the Committee of Publication, and the name of Dr. Wormley's wife, who had assisted him in his labors, was ordered to be entered on the rolls of the Congress.

The Secretary-General reported 382 names of delegates on the register. He announced the reception of a telegram from the NATIONAL TEMPERANCE ASSOCIATION, respectfully inviting an expression of opinion from the Congress regarding the use of alcohol.

The Secretary-General also read invitations to the members of the Congress to visit the following institutions:—The Academy of Natural Sciences of Philadelphia, Dr. W. S. W. Ruschenberger, President; The University of Pennsylvania, Dr. Robert E. Rogers, Dean; The Jefferson Medical College, Dr. John B. Biddle, Dean; The Pennsylvania Hospital; The College of Physicians of Philadelphia; and The Archæological Association; and from Dr. William Pepper, Medical Director of the Centennial Exposition, extending courtesies to visiting members. These invitations were, on motion, accepted.

An invitation was also read from the Council of the National Forest Association to attend its convention about to assemble at Cape May, N. J., on September 7 and 8.

The Secretaries of Sections then reported the conclusions adopted by their several Sections as follows:—

By the SECTION ON BIOLOGY:—The consideration of "The Excretory Function of the Liver," was opened by Dr. AUSTIN FLINT, Jr., and the following conclusions were adopted:—

1. Cholesterine exists in health in the bile, blood, and nervous matter, also in the crystalline lens, the spleen, and meconium.
2. Cholesterine is formed for the most part in the nervous matter, from which it is passed into the blood. The blood gains cholesterine in its passage through the brain. Its formation is constant, and it is always found in the blood.
3. Cholesterine is separated from the blood by the liver, and discharged with the bile. It pre-exists in the blood, serves there no useful purpose, and, if allowed to accumulate, blood poisoning results.
4. The bile has two separate and distinct functions, one connected with nutrition, to which the so-called biliary salts, glycocholate and taurocholate of soda, contribute; these do not exist preformed in the blood, but are products of *secretion*. The second function of the bile is *excretory*, connected with depuration or excretion; this is accomplished by the removal of the cholesterine which it obtains from the blood.
5. Normal feces do not contain cholesterine. The latter substance is represented by *stercorine*, formerly called *séroline*, into which it is converted in its passage down the intestine. The conversion of cholesterine into stercorine does not, however, take place when digestion is arrested or when it is not necessary, as is shown by the presence of cholesterine in its own form in the feces during fasting, and in the meconium.
6. The difference between the two varieties of jaundice, one mild and the other severe,

is dependent on obstruction of the bile-ducts in one instance, with reabsorption of the biliary coloring matters, while in the other there is retention of cholesterine in the blood in consequence of destruction of the parenchyma of the liver.

7. That condition of the blood dependent upon the presence of cholesterine in the blood is called *cholesteræmia*. It is characterized by symptoms referable to the brain, and may or may not be attended with jaundice.

8. Cholesteræmia does not occur in every disorder of the liver, because, even when a part of the organ is disorganized, there may remain a part still capable of performing the functions of excreting cholesterine.

9. In cases of simple jaundice, even when fæces are decolorized, there is no accumulation of cholesterine in the blood.

10. Cholesterine bears the same relation to the liver that urea does to the kidneys.

By the SECTION ON DERMATOLOGY AND SYPHILOGRAPHY:—The debate on the question, "Are Eczema and Psoriasis Local Diseases, or are they Manifestations of Constitutional Disorders?" was opened by Dr. L. DUNCAN BULKLEY, of New York, and the following conclusions were adopted:—

1. Eczema and psoriasis are distinct diseases. The former is to be clearly distinguished from artificial dermatitis, and the latter from the eruptions of syphilis, scaly eczema, and leprosy.

2. Eczema and psoriasis cannot own a double causation or nature at one time local and at another constitutional; but, with other diseases, may have a twofold cause, a predisposing and an exciting.

3. Eczema and psoriasis in many of their features resemble the accepted constitutional diseases more than they do those recognized as local.

4. Eczema is most properly likened to catarrh of the mucous membranes; it is very probable that some attacks called catarrh are eczema and psoriasis of the mucous tissue.

5. Both eczema and psoriasis resemble gout and rheumatism in certain respects, and are dependent upon a somewhat similar, although as yet unknown, constitutional cause; much of the skin lesion must be looked upon as the local result or remains of the diseases.

6. There as yet exists no microscopical or physiological proof that eczema and psoriasis are the sole result of local cell disorder, either congenital or acquired, or due alone to perverted nerve action.

7. Local causes play a very important part in the etiology of eczema. They are probably inoperative in psoriasis.

8. Local treatment is often insufficient alone to remove the lesions of eczema and psoriasis, and cannot prevent or delay relapses; its success does not necessarily demonstrate the local nature of these affections.

9. Constitutional treatment, alone and singly, can cure many cases of eczema and psoriasis, and prevent or delay relapses in a certain proportion of cases; under constitutional treatment is included every agency not properly classed among local measures.

10. The total weight of evidence and argument is that eczema and psoriasis are both manifestations of constitutional disorders and not local diseases of the skin.

By the SECTION ON OBSTETRICS:—The discussion of "The Mechanism of Natural and of Artificial Labor in Narrow Pelves," was introduced by Dr. WILLIAM GOODELL, of Philadelphia, and the following conclusions were adopted by the Section:—

1. The unaided first-coming head and the aided after-coming head observe in a flat pelvis precisely the same general laws of engagement and of descent. Hence, version here means art *plus* nature.

2. The forceps, however applied in a flat pelvis, antagonizes more or less with the natural mechanism of labor. Hence, the forceps here means art *versus* nature.

3. The aided and the unaided first-coming head observe in a uniformly narrowed pelvis precisely the same laws of engagement and of descent. But version violates these laws. Hence, the forceps here means art *plus* nature; version, art *versus* nature.

4. At, or above, the brim of a flat pelvis, the fronto-mastoid, or even the fronto-occipital, application of the forceps interferes less with the moulding of the head, and violates the natural mechanism of labor less, than the biparietal application.

5. In the flat pelvis, the vectis aids the natural mechanism of labor, and, therefore, meets the indications better than the forceps.

By the SECTION ON MENTAL DISEASES:—The consideration of the subject of the "Responsibility of the Insane for Criminal Acts," was introduced by Dr. ISAAC RAY, of Philadelphia, and the following conclusions were adopted by the Section:—

1. There is at present a manifest tendency to hold the insane responsible for criminal acts.
2. This tendency is unjust, unphilosophical, and contrary to the teachings of pathology, which clearly point out that insanity is but the expression of disease.

The SECTION ON MEDICINE also submitted the following resolutions for the action of the Congress, and on motion of Dr. BRODIE, of Michigan, they were adopted :—

Resolved, That the International Medical Congress of 1876 recognizes the advantages which would accrue from the introduction of a gradual uniformity in the multiple and heterogeneous elements of physic, as posology, nomenclatures, etc., and in the means and records of medical observation.

Resolved, That, in consequence, this Congress authorizes the President to appoint three delegates to the International Congress of 1877, with the special mission of presenting a schedule of the means of uniformity in physic actually applicable in all countries, and another of those which could soon be made acceptable by the profession at large.

Resolved, That the said delegates be advised to invite the co-operation of the men who have already worked for the same cause at the International or National Medical or Pharmaceutical Congresses of Paris, Vienna, St. Petersburg, Brussels, and Buffalo.

Under the above resolutions, the following delegates were subsequently appointed by the President: Dr. Henry I. Bowditch, of Boston, Dr. J. J. Woodward, U. S. A., of Washington, and Dr. E. Seguin, of New York.

Dr. PAUL F. EVE, of Tennessee, then delivered an Address on Surgery (see page 73), Dr. RUDNEW, of St. Petersburg, Russia, Vice-President, occupying the Chair; after which Dr. J. M. TONER, of the District of Columbia, delivered an Address on Medical Biography (see page 91), while Dr. L. A. DUGAS, of Georgia, Vice-President, occupied the Chair.

On motion the Congress then adjourned until Thursday at 10 A.M.

[On Wednesday evening, at 7.30 P.M., Dr. J. J. WOOWARD, U. S. Army, delivered in the Lecture Room of Jefferson Medical College an Address on The Medical Staff of the United States Army, and its Scientific Work (see page 286).]

FOURTH DAY'S SESSION.

The Congress met at 10 A. M., the President in the Chair. The minutes of the previous day were read and approved. The Secretary-General reported 422 names of delegates on the Register.

Dr. H. I. BOWDITCH, of Boston, presented the following preamble and resolutions, which were adopted :—

Whereas, The work already accomplished by the officers connected with the Bureau of the Surgeon-General of the United States Army, in the establishment of a medical library, and in the preparation of its complete and unique catalogue, in the formation of an anatomical museum from which important scientific results have already been obtained, and which have been not only a source of honor to these United States, but of value to foreign lands wherever science is cultivated ; and,

Whereas, This Congress learns with regret that, owing to the lack of a sufficient clerical force and of pecuniary means, not only some of the work already in progress has been suspended, but that other work of equal value cannot be undertaken, although ample materials for the same are now lying unused in the Surgeon-General's office ; therefore,

Resolved, That a committee of three be appointed to prepare a memorial to be presented to the Congress of the United States, at the earliest day possible, at its next session, urging efficient support to these most important works.

Resolved, That it is desirable that said memorial should be signed by the President, Vice-Presidents, and Secretary-General of this body.

The President appointed Drs. Bowditch, of Boston, Rudnew, of St. Petersburg, and N. S. Davis, of Chicago, as the Committee to prepare this Memorial.

On motion of Dr. J. P. WHITE, of Buffalo, New York, it was ordered that the printed pamphlet containing Dr. Bowditch's Address on Hygiene and Preventive Medicine be sent to the Presidents of State and Territorial Medical

Societies and Sanitary Boards of the United States, and of the Societies and Sanitary Boards of the Dominion of Canada.

The conclusions adopted by the several Sections were then reported as follows:—

By the SECTION ON SURGERY:—The subject of the "Medical and Surgical Treatment of Aneurism," was introduced by Dr. WILLIAM H. VAN BUREN, of New York, and the following conclusions were adopted:—

1. Tufnell's treatment of aneurism, by rest, position, and restricted diet, offers a valuable resource in thoracic and abdominal aneurisms.
2. It should always be tried in innominate, subclavian, subclavio-axillary, and iliac aneurisms, before resorting to measures attended by risk to life.
3. For aneurisms of the subclavian and iliac arteries, the Hunterian operation, with our present means of preventing secondary hemorrhage, is not justifiable.
4. For reasons formally set forth by Holmes and Henry Lee, the "old operation" cannot properly be formally substituted for the Hunterian operation in these cases, but should be held in reserve for special cases.
5. It is the most safe and surgical resource in gluteal aneurism, if the circulation can be commanded by the hand *in recto*.
6. The mode of cure by embolism, aimed at in the method of manipulation, is a not unfrequent explanation of what is called spontaneous cure of aneurism.
7. The value of Esmarch's bandage in the treatment of aneurism is probably not fully estimated.
8. In view of the promising features presented by the cases of Levis and Bryant, in which horse-hair was introduced into an aneurismal tumor, the repetition of this operation, or the substitution for horse-hair of Lister's prepared catgut or other animal substances, may be properly tried.

By the SECTION ON DERMATOLOGY AND SYPHILOGRAPHY:—The consideration of the subject of "The Virus of Venereal Sores; its Unity or Duality," was opened by Dr. F. J. BUMSTEAD, of New York, and the following conclusions were adopted by the Section:—

1. The virus of venereal sores is dual.
2. Venereal sores may be due to the inoculation of the syphilitic virus, and also to the inoculation of products of simple inflammation.
3. These two poisons may be inoculated simultaneously.
4. (Additional.) The present state of science has demonstrated that suppurating inflammatory lesions resembling chancroids may be produced on various portions of the body by inoculation with simple pus from various lesions.

By the SECTION ON OTOLOGY:—Dr. H. N. SPENCER, of St. Louis, opened a discussion of the question, "In what Percentage of Cases do Artificial Drum-membranes prove of Practical Advantage?" The following were the conclusions adopted by the Section:—

1. Of the various forms of artificial drum-membrane in use, the cotton pellet is preferable for its greater simplicity and its easier introduction, for the greater uniformity of its effect, and the comparative safety in its employment.
2. It has an advantage over all other forms of artificial drum-membrane in that, in addition to the functional gain which may be derived, there may be added its value as a means of treating the tympanum, and this therapeutical use of the artificial membrane has a great future in otology.
3. The continued use of the artificial drum-membrane as a means of improving the hearing is indicated in rare conditions which can only be determined by the aural surgeon.

By the SECTION ON SANITARY SCIENCE:—"The General Subject of Quarantine, with Particular Reference to Cholera and Yellow Fever," was considered by Dr. J. M. WOODWORTH, of the U. S. Marine Hospital Service, and the following conclusions were adopted by the Section:—

1. The supervision of ocean travel ought to be directed to securing good sanitary conditions of vessels at all times, out of as well as in port.
2. A system of *Port Sanitation* should be adopted and administered for each country or place, separately, modified in particular cases by taking into account the liability of the port to infection, the period of incubation of the disease, the length of time consumed in the voyage, and the measures enforced by the vessel *en route*.

3. In some countries the detention of passengers and crews of ships hailing from infected ports is unwarranted, but for such time only as is necessary to complete the period of incubation of cholera or yellow fever, counting from the date of departure from an infected port, or of landing from an infected vessel; but in no instance should passengers or sailors be held for observation on board an infected vessel, and such vessel should not be detained beyond the period required for inspection and thorough disinfection and cleansing.

4. Recognizing the fact that the morbific causes of infectious diseases may sometimes elude the most vigilant sanitary supervision of shipping, the importance of wisely-directed internal sanitary measures can scarcely be over-estimated.

5. As far as America is concerned, it is desirable that prompt and authoritative information should be had of the shipment of passengers or goods from districts infected with cholera or yellow fever, thereby insuring the thorough disinfection of infected articles.

6. The endemic homes of cholera and yellow fever are the fields which give the greatest promise of satisfactory results to well-directed and energetic sanitary measures, and to this end an international sentiment should be awakened, so strong as to compel the careless and offending people to employ rational means of prevention.

By the SECTION ON MENTAL DISEASES:—The consideration of the subject of the "Simulation of Insanity by the Insane," was opened by Dr. C. H. HUGHES, of St. Louis, Mo., and the following conclusion was adopted by the Section:—

It is not only not impossible for the insane to simulate insanity for a purpose in any but its gravest forms of profound general mental involvement, but they actually do simulate acts and forms of insanity for which there exists no pathological warrant that we can discover in the real disease affecting them.

On motion of Dr. T. G. RICHARDSON, of Louisiana, Dr. J. J. WOODWARD, U. S. A., was requested to repeat his lecture of the previous evening, as many of the members had failed to read the announcement of the lecture on the Programme, and had, therefore, been absent.

Dr. THEOPHILUS PARVIN, of Indiana, then delivered an Address on Obstetrics (see page 138), Dr. H. MIYAKE, of Tokio, Japan, Vice-President, occupying the Chair, and, after a very brief recess, Dr. STANFORD E. CHAILLÉ, of Louisiana, delivered an Address on Medical Jurisprudence (see page 167), while Dr. J. P. WHITE, of Buffalo, New York, Vice-President, occupied the Chair.

The Congress then adjourned until 10 A.M. on Friday.

FIFTH DAY'S SESSION.

The Congress met at 10 A. M., the President in the Chair. The minutes of the preceding day were read and approved. The Secretary-General reported 430 names of delegates on the Register.

On motion of Dr. PAUL F. EVE, of Nashville, it was

Resolved, That no papers or addresses read before this body, and ordered to be printed, shall be furnished either in abstract or otherwise for publication in any journal prior to the publication of the Transactions.

The conclusions adopted by the several Sections were then reported as follows:—

By the SECTION ON BIOLOGY:—The consideration of "The Mechanism of Joints," was introduced by Dr. HARRISON ALLEN, of Philadelphia, and the following conclusions were adopted by the Section:—

1. Starting with the idea that joints are of dynamic and static values, it is shown that in most movable joints the ball-and-socket arrangement predominates. When the ball is supported by the socket, as at the occipito-atloid articulation, *rest* is suggested. But when the ball is suspended from the socket, as at the temporo-maxillary articulation, *motion* is suggested.

2. Articular surfaces are of three kinds: *axial, actinic*, and *lateral*. The *axial* or primary surfaces are those situated upon proximal and distal ends of a bone in the line of its longitudinal axis. The *actinic* or secondary (rarely seen) are those placed in a line which is deflected from the longitudinal axis. The *lateral* or tertiary are those situated upon the sides of the shaft or body of a bone, and serve for articulation with corresponding surfaces of other bones.

3. Axial surfaces, it is believed, are static; actinic surfaces are dynamic; while lateral

surfaces have subordinate degrees of value—some of them being adventitious. The outer femoral condyle is active in extension = static; the inner femoral condyle is active in flexion = dynamic; but the lateral facets have no independent action.

4. Joints are fixed or locked at extremes of flexion and extension, and are most relaxed at the intervals between these extremes.

5. When a facet is actively employed, it enters into a combination with which the entire limb is in harmony. Hence, in the study of any one facet, its relations to all others of its kind, as well as to the bones, muscles, and fasciæ of its limb, become essential.

6. A correct knowledge of the symptomatology and treatment of diseases of the joints is dependent upon a true conception of the complex nature of articular surfaces.

By the SECTION ON SURGERY:—Dr. LEWIS A. SAYRE, of New York, opened the discussion of the subject of the "Treatment of Coxalgia." The Section adopted the following conclusions:—

1. Morbus coxarius is a disease most frequently met with in early childhood, or the age of reckless indifference.

2. It is almost always of traumatic origin, and not necessarily connected with a vitiated constitution.

3. Rest and freedom from pressure of the parts involved, while at the same time the rest of the body is allowed free exercise in the open air, and a nutritious diet, is the best treatment that has yet been devised for this disease.

4. If this plan of treatment be adopted in the early stages of this disease, the majority of cases will recover, with nearly if not quite perfect motion, and without deformity.

5. In the advanced second stage of the disease, when absorption of the effused fluid cannot be produced, then it is better to puncture or aspirate the joint and remove its contents, than to leave it to rupture by ulceration.

6. In the third stage of the disease, when the treatment recommended in this paper has been properly applied without satisfactory improvement, but progressive caries continues, the exsection of the diseased bone is not only justifiable, but in many cases absolutely necessary.

7. That the operation of exsection of the hip is easily performed, and in itself attended with little or no danger.

8. That after exsection of the hip-joint in cases of progressive caries, the recovery is much more rapid and certain, and infinitely more perfect, as to form, motion, and the usefulness of the joint and limb, than when left to the slow process of nature.

[NOTE.—In the second conclusion, the Section does not unanimously coincide.]

By the SECTION ON DERMATOLOGY AND SYPHILOGRAPHY:—After a debate, opened by Dr. E. L. KEYES, of New York, on "The Treatment of Syphilis with special reference to the Constitutional Remedies appropriate to its various stages; the Duration of their Use, and the question of their Continuous or Intermittent Employment," the Section adopted the following:—

Negative conclusions, for which there would seem to be no foundation in fact:

1. Syphilis commencing mildly needs but little treatment, and does not require mercury.
2. Mercury given internally is necessarily debilitating.
3. Mercury is only useful in secondary syphilis.
4. Iodide of potassium is of considerable value in secondary syphilis.
5. Iodide of potassium is of no value unless preceded by the use of mercury.
6. Iodide of potassium acts by liberating mercury which has been lying latent.

Positive conclusions, which, in the present state of our knowledge, may be affirmed:

1. Mercury is an antidote to the syphilitic poison, and of service in controlling all its symptoms in all, even the latest stages of the disease; its power over gummata being least, and not to be relied upon.
2. Mercury in minute doses is a tonic.
3. Iodine cures certain symptoms of syphilis, but does not prevent relapses.
4. Mercury, long continued uninterruptedly, as far as practicable, in small doses from the time of the earliest eruption, constitutes the best treatment of syphilis.

By the SECTION ON OPHTHALMOLOGY:—A discussion on the question, "Are Progressive Myopia and Posterior Staphyloma due to Hereditary Predisposition, or can they be induced by Defects of Refraction, acting through the Influence of the Ciliary Muscle?" was opened by Dr. E. G. LORING, Jr., of New York, and the following conclusions were adopted by the Section:—

1. From the fact that so many children are myopic, whose parents are not near-sighted, while the myopia increases directly with the amount of increased tension of the eyes, and

from the fact that an interchange of refraction may occur, whereby an eye which is not congenitally myopic may become so in spite of hereditary tendency against it, it would seem to follow that hereditary predisposition, though undoubtedly a potent cause, is not only not the sole cause, but that it is not even the predominating cause.

2. The action of the ciliary muscle, taken by itself, exerts but little influence on the production of myopia, and still less on the formation of the cone.

[NOTE.—Of these conclusions, the first was adopted by the Section unanimously, and the second by a majority of fifteen to seven.]

Dr. N. S. DAVIS, of Chicago, offered the following preamble and resolutions, which were adopted :—

Whereas, This Congress marks an era in the history of medicine in the United States of America, the addresses as delivered presenting a summary of progress in the various departments which will be of great historical value in all coming time ; and

Whereas, It is highly probable that these addresses, in connection with the many very valuable papers read and discussed in the Sections, will require for their early and proper publication more funds than are at present in the hands of the Treasurer for the purpose ; therefore,

Resolved, That the Committee of Publication be authorized and instructed, as soon as practicable after the final adjournment of the Congress, to ascertain the probable cost of publishing the full Transactions in a style appropriate for the work, and, if the money on hand be found deficient, to address a circular letter to the American members of the Congress, asking for such additional sum, not exceeding $10 for each of such members, as will supply the deficiency ; and that said committee be authorized to withhold the volume or volumes, when published, from any member who may neglect to pay the additional sum required.

Resolved, That the Committee of Publication be authorized and requested to exercise a careful and liberal discretion in preparing and revising the proceedings and reported discussions in the several Sections, for publication in the Transactions of the Congress.

The Secretary General announced the receipt of communications from the Woman's National Christian Temperance Union, and from the Friends Temperance Union of New York, respectfully inviting an expression of opinion from the Congress concerning the use of alcohol.

On motion of Dr. N. S. DAVIS, these were referred to the Section on Medicine.

On motion of Dr. J. M. TONER, of Washington, D. C., the communication previously received from the National Temperance Association was similarly referred.

After a recess of ten minutes, Dr. J. P. GRAY, of Utica, N. Y., read an Address on Mental Hygiene (see page 205), Dr. HUNTER MCGUIRE, of Virginia, Vice-President, occupying the Chair; and afterwards Dr. LUNSFORD P. YANDELL, of Louisville, Kentucky, delivered an Address on Medical Literature (see page 223), while Dr. T. G. RICHARDSON, of Louisiana, Vice-President, occupied the Chair.

On motion, the various addresses read before the Congress were referred to the Committee of Publication, and the Congress then adjourned until 10 A. M. on Saturday.

SIXTH DAY'S SESSION.

The Congress met at 10 A. M., the President in the Chair. The minutes of the preceding day were read and approved. The Secretary-General reported 447 names of delegates on the register (see page xv.), and, on motion of Dr. TONER, these were all confirmed as members of the Congress.

The Secretary-General read a request from Dr. RUDNEW, of Russia, asking the privilege of access to the minutes and proceedings of the Congress, with a view of reporting them on his return to Russia, which was granted.

The Section on Medicine recommended that the Secretary be directed to send, in reply to the various Temperance Associations which had sent communications to the Congress, the following conclusions which had been approved by the Section, and which were contained in a paper read before it by

Dr. E. M. Hunt, of New Jersey, entitled "Alcohol in its Therapeutic Relations as a Food and as a Medicine."

1. Alcohol is not shown to have a definite food value by any of the methods of chemical analysis or physiological investigation.
2. Its use as a medicine is chiefly that of a cardiac stimulant, and often admits of substitution.
3. As a medicine, it is not well-fitted for self-prescription by the laity, and the medical profession is not accountable for such administration, or for the enormous evils arising therefrom.
4. The purity of alcoholic liquors is, in general, not as well assured as that of articles used for medicine should be. The various mixtures, when used as medicines, should have a definite and known composition, and should not be interchanged promiscuously.

On motion of Dr. J. P. White, of Buffalo, it was

Resolved, That the Centennial Medical Commission of Philadelphia is hereby tendered the cordial thanks of this Congress for the most excellent manner in which its members have discharged the arduous duties devolved upon them, and by which our pleasure and profit have been so much enhanced.

Resolved, That the President and other Officers of the International Medical Congress of 1876 are hereby tendered the cordial thanks of the Congress for the excellent manner in which they have discharged the arduous duties devolved upon them, and by which our pleasure and profit have been so much enhanced.

Resolved, That the Officers and Trustees of the University of Pennsylvania are hereby tendered our cordial thanks for the very liberal use of their excellent buildings for the meetings of this International Medical Congress.

Resolved, That the Officers and Trustees of the Jefferson Medical College are hereby tendered the cordial thanks of this Congress for the use of their lecture-room for the most interesting lecture of Dr. J. J. Woodward, U. S. A.

Resolved, That the cordial thanks of the International Medical Congress are especially due to Drs. Thomson, Wilson, and Strawbridge, and to Messrs. Henry C. Lea and J. B. Lippincott, for their generous hospitality.

On motion of Dr. H. I. Bowditch, of Boston, the following resolution was adopted:—

Resolved, That we, a brotherhood of physicians from the North, South, East, and West of this country, hereby tender to our associates from other lands our most earnest wishes that they may have safe and happy returns to their homes, and we would suggest the hope that they will carry back many pleasant memories of this fraternal meeting, now closing, and which has been, most appropriately, held in this generous and noble city of Philadelphia.

Dr. J. A. Grant, of Ottawa, Canada, stated that at a meeting of the members of the Canadian medical delegates, held yesterday, the following resolutions were adopted unanimously:—

Resolved, That the Canadian members of the International Medical Congress desire to express their sense of the great consideration and urbanity with which they have been treated by the officers and members of the Centennial Medical Commission, and beg, by this resolution, to tender their warm thanks for the same.

Resolved, That the Canadian members of the International Medical Congress most cordially join with the other members of the Congress in thanking the physicians and citizens of Philadelphia for the generous hospitality extended to its members throughout the present session.

Dr. Charles J. Hare, of London, read the following expression of congratulation from the delegates of Great Britain:—

The delegates from Great Britain to the International Medical Congress of Philadelphia beg to congratulate the President and the several committees on the complete success of the Congress, on the high value of the various addresses presented to it, and on the forward impulse which it has given to the progress of medicine in the widest sense of that word. They desire at the same time to express in the strongest and warmest terms their sense of and their thanks for the unmeasured kindness and courtesy and the unbounded hospitality with which they have been received on this Centennial occasion, and to add that they will carry back with them a most grateful recollection of that warm right hand of fellowship which has been so warmly extended to them by their brethren of the United States.

Signed on behalf of the British delegates by Charles J. Hare, M.D. Cantab., F.R.C.P.,

late Professor of Clinical Medicine in University College, and Physician to University College Hospital; R. BRUDENELL CARTER, F.R.C.S. Eng., Hunterian Professor of Surgery to the Royal College of Surgeons of England; WILLIAM ADAMS, F.R.C.S., President of the Medical Society of London.

Dr. L. A. SAYRE, of New York, offered the following:—

Resolved, That this International Medical Congress requests its President, Professor Samuel D. Gross, to sit for his portrait, and that the Committee of Publication be instructed to have the same engraved and printed as a frontispiece to the Volume of Transactions. Adopted.[1]

The Secretary-General stated that a circular had been received announcing that an International Medical Congress would be held in Geneva in September, 1877.

Dr. N. S. DAVIS, of Illinois, then delivered an Address on Medical Education and Medical Institutions (see page 265), Dr. HENRY GIBBONS, of California, Vice-President, occupying the Chair.

Dr. GROSS, President, announced that the official programme of the Congress had been completed. No new business being presented, he addressed the Congress as follows:—

GENTLEMEN: Before I put the question of the final adjournment of the Congress, I desire to say a few parting words. First and foremost, I must be permitted again to thank you for the great honor which you have done me in electing me as your presiding officer. It is the last honor which I can reasonably expect from my professional brethren, who have always been so kind to me in the bestowal of their favors and of their good opinion. For these courtesies I shall never cease to be grateful, for they have served to cheer me in my labors, and have been as a balm to my soul.

The International Medical Congress of 1876 is about to pass into history as a thing of the past; but, although its exercises are at an end, its work will live and form an interesting era in our profession as marking the reunion of a great body of men in the Centennial year of American Independence. We have listened to a number of valuable and instructive discourses, illustrative of the progress of American medicine and surgery; have been engaged in profitable debates affecting some of the most vital interests of society; have performed a large amount of earnest work in the Sections; have met at the festive board; have clasped hands, and have formed warm and, as I trust, lasting friendships, all without any discord, one unkind expression, or even one word of misunderstanding. Altogether, we have every reason for self-congratulation. When another Centennial century shall have passed away, the men who shall then be upon the stage will not fail to commemorate our meeting, and to bless us for what has been done this week in the interests of humanity and of medical science. In dissolving this meeting, as I now do, permit me to invoke upon our labors the choicest blessings of Almighty God, and to wish each and all of you a safe return to your homes, and a happy reunion with your families and friends. May we not hope that you may long, if not forever, retain pleasant memories of our meetings in this Chapel, and that in your leisure moments your minds may occasionally revert to those of us from whom you are about to separate?

The Congress then adjourned *sine die*.

I. MINIS HAYS, M.D.,
Secretary-General.

WILLIAM B. ATKINSON, M.D.,
RICHARD J. DUNGLISON, M.D.,
RICHARD A. CLEEMANN, M.D.,
WILLIAM W. KEEN, M.D.,
R. M. BERTOLET, M.D.,
Secretaries of the meeting.

[1] [In deference to Prof. Gross's expressed wish, the portrait is omitted.—EDITOR.]

NOTICE.

The Committee of Publication thinks it proper to say that the Congress is to be held in no way responsible for the statements, reasonings, or opinions set forth in the various papers published in its Transactions.

ADDRESSES.

ADDRESS ON MEDICINE AND MEDICAL PROGRESS IN THE UNITED STATES.

BY

AUSTIN FLINT, M.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE, AND CLINICAL MEDICINE, IN BELLEVUE HOSPITAL MEDICAL COLLEGE, NEW YORK.

An important object of the Philosophy of History is the study of agencies concerned in originating and modifying past events. From this standpoint I propose to survey "Medicine and Medical Progress in the United States" during the past one hundred years. Whence were derived influences affecting the progress of Medicine in this Centennial period? I shall submit certain historical facts and reflections in answer to this inquiry. Another object of the Philosophy of History, of greater practical importance, is the study of agencies with reference to future events. Regarding Medicine in the latter aspect, the inquiry arises, by what influences is the progress of Medicine in our country to be promoted? This inquiry opens up a wide range of thought, to which some incidental reference is all that the limits of my discourse will permit.

Medicine, as I use the term in this discourse, distinguishes a province of knowledge and art from Surgery and Midwifery. In this restricted sense it denotes the province called in former times "the theory and practice of physic." I assume that medicine has steadily progressed in our country during the past one hundred years; the rapidity, at different periods, during this space of time, and the extent of progress, will naturally enter into reflections upon the influences affecting it. The scope of the term progress, as applied to Medicine, claims preliminary explanation. In what consists, and what are the criteria of, medical progress in this country, in past time? This question cannot be summarily answered; the answer will lead at once to the influences affecting the progress which has taken place, and to those which tend to its promotion now and hereafter.

The most striking evidence of progress is a great discovery, the practical benefits of which are immediate and palpable. Discoveries entitled to this distinction, within the past century, are vaccination, auscultation, and the separate functions of different components of the nervous system. This brief list cannot be much lengthened. Pre-eminently great discoveries in medicine are so distributed in respect of intervals, as to bear the impress of special gifts vouchsafed by Providence. We can boast of but one discovery which belongs in this category, the annihilation of pain by anæsthetic inhalations. It is true the brilliancy of this discovery is especially conspicuous in its applications to surgery; but the

benefits conferred on medicine are not vastly inferior, and would alone entitle it to rank with discoveries to be characterized as pre-eminently great.

Evidence of progress equally obvious, although less striking, is afforded by enlargement of the boundaries of knowledge in different directions. Under this head are embraced all facts relating to the nature of diseases, their anatomical characters, their causation, their discrimination from each other, their natural history, the principles of treatment, the relative value of remedial or curative agents, the introduction of new remedies or therapeutical methods and effective measures of prophylaxis. Not stopping to consider in these relations the history of Medicine in the United States during the past Centennial period, I pass to another aspect of progress.

The progress of Medicine in any country is shown by the prompt recognition of advancements in knowledge and improvements originating in other countries. After, for example, Harvey's great discovery, tardiness of its recognition in any country betokened lack of progress. So with regard to discoveries of every grade, and improvements great or small, it is proof of progress when they are promptly recognized in countries other than those in which they originated. Readiness in the importation of knowledge is thus a criterion, as it is, of course, a means of progress. I need not add that the rapid diffusion throughout a country of knowledge, either imported or of native growth, is both a criterion and a means of progress. Further, a spirit of progress is shown by the prompt recognition and diffusion, not alone of actual advancements in knowledge and real improvements, but of doctrines and novelties which fail to stand the test of time. Thus, in the eighteenth century, a spirit of progress must have been wanting wherever but little was known of the theories of Stahl, Hoffmann, and Boerhaave. This statement is not inconsistent with the belief that the prevalence of these theories impeded advancement in actual knowledge of medicine and real improvements in medical practice.

With these explanations of the term progress in Medicine, let us survey the medical history of the United States.

We celebrate in this anniversary the lapse of a hundred years of our existence as an independent people. A hundred years is but a small space in the life-history of a nation. We have passed through infancy; we are hardly beyond adolescence, and, as we hope and believe, senescence is far in the future. Our infantile and our adolescent condition are, of course, to be taken into account in our reflections upon the influences affecting the progress of Medicine.

When, in 1776, by a Cæsarean section, we were separated from the mother country, Medicine existed in a state ready for progressive development and growth. The number of educated and able physicians was by no means insignificant. Conspicuous among these were Rush, Morgan, Shippen, Thomas Bond, of Philadelphia; John and Samuel Bard, Peter Middleton, Richard Bailey, Jacob Ogden, Cadwallader Colden, of New York; Benjamin Gale, of Connecticut; Joseph Warren, William Aspinwall, and the centennarian Holyoke, of Massachusetts. At our nation's birth, the lives and labors of these men and their coadjutors had a potential influence in determining the commencing progress of Medicine. Medical education had an embryonic existence in two medical schools, one in Philadelphia and the other in New York, and there were germinations of medical literature.

In the history of American Medicine prior to the Revolution we find in operation a source of influence which has largely affected medical progress in this country during the last hundred years, namely, the recognition and adoption of advancements, real or apparent, in other countries. A striking instance in the colonial period was the introduction of the inoculation of smallpox by Zabdiel Boylston in 1771. Boylston inoculated his son about two months after the date of the inoculation, at the instance of Lady Mary Wortley Montague, of her daughter, and without knowledge of the latter fact. Boylston himself inoculated two hundred and forty-seven persons within a year after his first inoculation, whereas, in England, during that period, and prior to the inoculation of the daughters of the Princess Caroline, it had been practised in only twenty cases. Virulent as was the opposition on this side of the Atlantic, Boylston's life even being endangered, the practice in England was still more violently opposed. Its sinfulness was proclaimed from the pulpit, it being likened on one occasion to the smiting of Job with boils by Satan, "the delnded preacher," quoting from Boylston's biographer (Thacher), "wishing to have it understood that the Devil was the first inoculator, and that poor Job was his first patient."

The Revolutionary struggle, while it suspended teaching in medical schools, labors in literature, and studies requiring tranquil leisure, doubtless gave an impetus to medical as well as surgical progress. It enlarged opportunities for, and enforced closer attention to, the observation of certain diseases; it enhanced the urgency of prompt and successful methods of treatment; it opened up the subject of hospitals, and it stimulated to measures of prophylaxis. Herein is, in some degree, a compensation for the barbarism of war! It is not surprising, then, that, with the antecedent history of American Medicine, medical progress was renewed with much activity after the achievement of independence. As illustrations of this activity, after the war had ended (1783), and prior to the beginning of the 19th century, two rival medical schools in Philadelphia had gone into operation, coalesced, and been united with the University of Pennsylvania, the faculty then consisting of Rush, Kuhn, Wistar, Shippen, Benjamin Smith Barton, and James Hutchinson; the Philadelphia Dispensary for the relief of the sick poor, the first institution of its kind in the United States, had been established; the trustees of Columbia College in New York had organized a medical school, which, however, from local difficulties, had not accomplished much; a medical department of Harvard University had been instituted, with John Warren, Waterhouse, and Aaron Dexter as professors; medical societies had been incorporated in Massachusetts, New Jersey, and New Hampshire, and the publication of the "Medical Repository," a quarterly journal, which lived to the completion of twenty-three volumes, had been commenced. It is worthy of note that, prior to this century, Dr. Ezekiel Hersey, of Hingham, Massachusetts, had bequeathed a thonsand pounds, his widow the same sum, his brother, Dr. Abner Hersey, of Barnstable, and Dr. John Cuming, of Concord, each five hundred pounds, to be applied to the support of a Professor of Anatomy and Surgery in the Harvard Medical College. The sum of one thousand pounds toward the support of an additional professor had been bequeathed by William Erving, Esq., of Boston. It would have been pleasant to have cited these examples as precedents of many similar contributions for the promotion of medical education; but, it must be confessed, the subsequent history of Medicine in the United States furnishes little to boast of in this regard. It is, how-

ever, a question, the discussion of which would be here out of place, how far the interests of Medicine in this country would have been advanced by the endowment of professorships in medical colleges. As an illustration of interest in medical progress felt by persons not members of the profession, in 1798 Ward Nicholas Boylston secured to the Harvard School a fund yielding an annuity, for annual prizes for competitive dissertations on medical subjects, of one hundred and thirty-three dollars. These prizes are continued at the present day. The same public-spirited citizen of Boston founded the medical library which bears his name, and contributed in varions ways to the promotion of medical education. Another conspicuous illustration was the active co-operation of the great Franklin in founding and in the management of the Medical College of Philadelphia. Franklin was president of the board of trustees, and the meetings were held at his house until within a few months of his death.

During the period in our history of which I am now speaking, Cullen was the master-spirit in Medicine. Not content with being an expounder of the doctrines of Boerhaave, or a follower of his commentator Van Swieten, he promulgated a system of his own. He had the boldness to abandon giving lectures in the Latin language. His teachings, with those of his able colleagues Monro, Gregory, Black, Young, and Home, had made Edinburgh the centre of medical education. The College of Philadelphia, which in 1791 was merged in the Medical Department of the University of Pennsylvania, had been modelled after the school in Edinburgh, the latter having been modelled after the famous school of Leyden. All the professors of the Philadelphia College had graduated at Edinburgh. Hence, in this period of our history, the Cullenian system was adopted, although, as will presently be seen, its supremacy was not long maintained. It is evidence of activity at this period that an American edition of Cullen's work entitled "First Lines of the Practice of Physic" was published in 1781, his lectures on the Materia Medica having been published in Philadelphia in 1775.[1] Rush, in a letter to Cullen, written in 1783, said, in relation to the American edition of the "First Lines," that it "had a rapid sale and a general circulation throughout the United States. It was read with peculiar attention by the physicians and surgeons of our army, and in a few years regulated in many things the practice in our hospitals." He added: "Thus, sir, you see you have had a hand in the revolution, by contributing indirectly to save the lives of the officers and soldiers of the American army."

I have cited illustrations enough to show the activity of medical progress prior to the nineteenth century. The experience acquired in the war of the Revolution, the enthusiasm arising from the sense of national independence, the number of American physicians who had completed their medical education in European schools and imbibed the spirit of master minds in the profession, the pressing need of educated and skilful practitioners—these were potential influences affecting the progress of Medicine in the early days of the Republic.

Let me refer in this connection to the position held by the city in which we are assembled, in her relations to the progress of Medicine in the United States. At the time of the Revolution, Philadelphia was distinguished for her able and learned physicians and surgeons. They inau-

[1] For these and other facts relating to the history of this period, I am indebted to Carson's History of the Medical Department of the University of Pennsylvania.

gurated systematic medical instruction on this continent prior to the Revolutionary war, and again directly peace was declared. At the time to which our survey of the history of Medicine has extended, Philadelphia was the acknowledged seat of medical education. This pre-eminence she has held from that time to the present. In the number of medical men who have been educated at her schools; in the great preponderance of her medical literature, and in her large proportion of the distinguished representatives of the different departments of Medicine, she has had no compeer in the new world. To the influence of her example is to be attributed much of the activity of progress in other cities of the Union. If, in the future, she should cease to preserve the relative position which she now deservedly holds, it will be, in no small measure, from the spirit of honorable emulation awakened and sustained by her admirable example. In saying what I have said, I feel that I may assume to speak in behalf of the medical profession of the United States. It was most fitting that an International Medical Congress, in celebration of our centennial anniversary, should assemble in the city of Philadelphia.

We have now to inquire concerning influences affecting medical progress after the commencement of the nineteenth century. At the end of the first quarter of this century, there were at least twenty colleges in active operation, distributed among sixteen of the States of the Union, the number of students in the year 1825 exceeding two thousand. Of these twenty colleges, six may be distinguished as Metropolitan, being situated in Boston, New York, Baltimore, or Philadelphia. Let us do justice to the so-called Provincial schools as affecting medical progress. They furnished formal instruction to many who were unable to bear the expenses of long journeys and a residence in metropolitan towns. A considerable proportion of students then, as now, pursued their studies in part at institutions near at hand, and, in part, at schools in the cities just named. The provincial schools, each within a certain area, developed interest, inquiry, and study among members of the profession, thus raising the standard of medical acquirement. They awakened an ambition to become teachers by affording opportunity for the display of talent in that direction. If it be said that in the establishment of these schools the founders were not altogether disinterested, but may have been actuated by motives relating to personal distinction and success; this may be said with not less pertinency of metropolitan schools, and, moreover, such motives are not unworthy. If it be said that they have occasioned local jealousies and feuds; this is not less true of metropolitan schools. Thacher states with respect to the conferring of degrees upon the two first graduates of the Harvard Medical College, that, "from a spirit of envy and jealousy towards the professors, great opposition was made, and it was by the address and perseverance of John Warren that the object was finally accomplished." The success of medical schools in the city of New York was for several years retarded by local opposition; and the same difficulties were experienced in the early history of the schools in the "city of brotherly love." Not a few of the professors in the larger schools of our country first tested their powers, and acquired experience as teachers in the smaller schools. The latter have had not an inconsiderable share in promoting the progress of Medicine.

Within the first quarter of the present century, about twenty medical

journals were established. These contributed largely to the promotion and diffusion of medical knowledge. The subsequent multiplication of journals has not been proportionate to the increase and extension of our population, those now in existence not much exceeding twofold the number which had existed in 1825. Hitherto, as now, a considerable proportion have had a limited local circulation, having been issued generally in places where medical schools were established, and being in a certain sense the organs of the latter. These facts are not stated by way of disparagement. Of the great service rendered to medical progress by the larger and widely distributed journals, it is not necessary to speak; but, in justice to those comparatively small, and circulating within restricted limits, it should be said that they have in no small measure been serviceable. They have aided in diffusing medical intelligence. They have incited to reading, thought, and profitable discussion. They have increased the number of contributors to medical literature. It would be easy to cite numerous instances of valuable contributions which have appeared in journals relatively obscure.

The bibliography of the first quarter of the present century shows a fair list of original works, foremost in number and influence being the writings of Rush. Some of the works of this voluminous author, and other contributions to medical literature during this period, were republished abroad and translated into different languages. The profession of this country were by no means wholly dependent on foreign works. It would, however, have betokened a lack of the spirit of progress had an acquaintance with foreign authors been restricted by prejudice or indifference. It was far otherwise. English works were the authorities in Medicine, and many of those who could afford the time and expense, crossed the Atlantic (the voyages requiring at least two months) in order to finish their studies in Edinburgh or London, the schools in these cities at that time taking precedence of those on the continent of Europe. The reminiscences of the Revolutionary struggle, and a renewal of war with the mother country during this period, had no repressing influence on the attachment to the medical literature and the distinguished teachers of Great Britain. Is it unpatriotic to say this with gratification and pride? May we not claim in behalf of Medicine that it tends to raise its followers above political contentions! May I not refer to our late domestic strife as affording illustrations of a brotherhood which the legitimate objects of Medicine render impregnable!

As a source of influence affecting the progress of Medicine, the formation of medical societies in the early, and still more in the latter, part of the present century is to be mentioned. Without going into historical details, suffice it to say, that thereby the advancement and diffusion of knowledge have been greatly promoted. Moreover, to voluntary associations is due, in a great measure, the character of the profession as regards medical ethics and its position in relation to illegitimate systems of practice. I must be content with simply referring, in this connection, to one of the most important of the agencies concerned in medical progress.

In pathology and therapeutics during the early part of the present century, the influence of Cullen, surviving his death, was still great. Rush, however, a pupil and a fervent admirer of Cullen, was too bold and independent a thinker to be satisfied with being an expounder of his teacher's doctrines. In those days an object of medical philosophy was

to reach a theory which should reduce to a simple system the practice of medicine—an object which, were it attainable, would obviate many of the difficulties in the way of diagnosis, embarrassing the practitioner of the present day. Rush was the author of a theory, the unity of disease. According to this theory all morbid phenomena are the varied manifestations of a single entity. This is not the time nor place to enter into any discussion of the merits of medical theories, or to consider their practical effects. Whatever may be thought of the "theory and practice of physic" as taught by Rush, he influenced largely pathological views and therapeutics during his lifetime, and the influence of his writings continued after his death. The system of Brown, the opponent of Cullen, had its advocates in this country—a system leading to rules of practice directly in conflict with those inculcated by Cullen and Rush. Even the extravagances of Darwin's Zoonomia, which was republished in New York and Philadelphia in the latter part of the eighteenth century, with an introductory address and an appendix by Charles Caldwell, were not without influence. Toward the close of the first quarter of the present century (1823) the "Study of Medicine," by John Mason Good, was republished in Boston, a work inaugurating the "physiological system" in nosology and practice. It is evidence of a true spirit of progress that, of the different systems, none had exclusive sway, and that, although there were enthusiastic partisans of each, many, if not the majority of, thinking minds were guided by a rational eclecticism, selecting from any whatever commended itself to reason and experience. With regard to this point I quote from the worthy historian of "Medicine in America," Thacher, the following, written in 1828: "We recognize in our institutions no uniform theoretical system as the rule of practice. Medical history affords abundant evidence of the instability of human systems. Every age has teemed with theories or visionary hypotheses fleeting as the wind, scarcely surviving their authors, but yielding to others as transient and unsubstantial as themselves. The medical authorities most respected are Cullen, Rush, and Good. These, modified and improved according to the judgment and views of the respective professors, are adopted and taught in the various American universities."

The introduction of vaccination in the beginning of this century was a striking instance of the prompt recognition and adoption of a great discovery. Jenner's "Inquiry into the Causes and Effects of the Variolæ Vaccinæ" was published in London in 1799. In the following year Benjamin Waterhouse vaccinated four of his children with virus received from Jenner, and tested their insusceptibility to smallpox by exposing them to infection in a hospital for the treatment of patients with the disease. In 1801, Valentine Seaman commenced vaccination in New York. Its speedy employment throughout the United States was unattended by the violent opposition which characterized its early history in the country honored by this greatest of all discoveries, as measured by its influence on the physical welfare of mankind.

In 1817, an important event denoting progress was the project for the formation of a National Pharmacopœia. This project originated with Lyman Spalding, and the initiatory steps were taken by the New York County Medical Society. In 1820, a convention of delegates from the different medical colleges and societies throughout the Union assembled at Washington and adopted an American Pharmacopœia, which, with decennial revisions, has been continued up to this date.

The second quarter of the present century is an eventful period in the history of Medicine. The great American discovery of the annihilation of pain by anæsthetic inhalations was made in this period. It is significant of a cosmopolitan spirit pervading the medical profession throughout the civilized world, that this discovery was at once recognized and adopted in other countries. The first application to capital operations in surgery was in Boston, in September, 1846; and in the following January the brilliant success of this application was demonstrated in the hospitals of London and Paris. Directly following the application to surgery, anæsthetic inhalations entered into the treatment of diseases both here and abroad.

Surveying the more memorable of the events relating to the medical knowledge of this period, it began with the advancement of morbid anatomy to the rank of a distinct department of medical study. This was the result of the labor of Bichat, which gave birth to General Anatomy. That great work, the "*Anatomie Générale*" of Bichat, translated by George Hayward, was published in Boston in 1822. Bayle and Hollard's Manual of General Anatomy was translated by Gross in 1828, and Béclard's Elements of General Anatomy, which appeared in 1823, was translated by Joseph Togno, and published in Philadelphia in 1830. The Profession of this country were prepared by these works, together with the older works of Baillie, Meckel, Portal, Laennec, and others, for the grand development of this study by Cruveilhier, Andral, Lobstein, in France; by Mueller, Gluge, Otto, Henle, Vogel, Hasse, Rokitansky, in Germany; and by Carswell, Craigie, Hodgkin, and Hope, in Great Britain. With the writings of these distinguished authors the Profession of this country became immediately conversant. Some of the French and German works were translated, and all were brought to the notice of medical readers by means of analytical reviews. The spirit of progress was shown by the efforts to keep pace with the advancement in other countries. To compete with these in the bibliography of this study was not to have been expected; yet in 1829 appeared "A Treatise on Pathological Anatomy," by Horner, in two volumes; and in 1839 an elaborate work by the honored President of this International Congress, then a professor in the State which gave to the profession McDowell. Horner and Gross were thus pioneer authors in this branch of American medical literature. It is noteworthy that, for several years prior to the publication of the work by Gross, he had held a chair of pathological anatomy in the Cincinnati Medical College. I am fully warranted in saying that this field of observation was diligently cultivated by not a few, and that there was a general appreciation of its importance in relation to the knowledge of diseases. To the study of Pathological Anatomy during nearly all the period to which our attention is now directed, and up to the present moment, have been devoted the labors of one who, had he not been, unfortunately (I would almost say, with the utmost respect, blamably), satisfied with the gratification pertaining to his attainments, together with his usefulness as a professor, and his invaluable services as curator of one of the richest of the museums in this country, if not in the world, his name would have become so identified with the study in the medical literature of every country that it would have been superfluous to refer in this manner to J. B. S. Jackson.

The discovery of auscultation can hardly be said to have had much efficient influence on the progress of Medicine prior to the second quarter

of the present century. It is true, Laennec's great work was published in 1819, and the translation by Forbes appeared in London in 1821; but the discovery met with distrust, and was disparaged by some authoritative authors. Forbes's translation was republished in this country in 1830, and from this date the great value of auscultation began to be recognized. Soon after this date James Jackson, Jr., opened up the study of the abnormal modifications of expiratory sounds, which Laennec had, in a great measure, overlooked. In 1835 Gerhard published a treatise on the diagnosis of thoracic diseases to "supply the wants of his classes and of others who felt an interest in the subject." In 1836 the Massachusetts Medical Society published dissertations, by Oliver Wendell Holmes, Robert W. Haxall, and Luther V. Bell, submitted in competition for a Boylston prize, which was adjudged to Holmes. As early as in 1832 James Jackson, in his visits at the Massachusetts General Hospital, always carried a stethoscope, the counterpart of that which Laennec was accustomed to make with his own hands; and the daily record dictated at the bedside of each patient embraced signs obtained by percussion and auscultation. It was not long before physical exploration by these methods entered more or less into clinical and didactic teaching in most of our medical colleges, and thence speedily found its way into private as well as public practice. The Profession of this country were not tardy in accepting the fruits of Laennec's discovery, and this branch of practical medicine has ever since had with us not a few zealous votaries. Our medical literature, during the past forty years, contains a good proportion of contributions relating to it.

The physical diagnosis of diseases of the heart, which at the present time has reached such perfection, may be said to have had its point of departure in the works of Bouillaud and Hope. Hope's treatise, issued in London in 1832, was republished, with notes by the late Dr. C. W. Pennock, in Philadelphia in 1842. In the interim Dr. Pennock and Prof. E. M. Moore, of Rochester, N. Y. (1839), had devised and carried out a series of accurate experiments to elucidate the mechanism of the heart sounds. At the end of the second quarter of the present century, the progress in this province of medicine had been made familiar to American medical readers by the republished works of Andry, Raciborski, Barth and Roger, Aran, Stokes, Latham, Walshe, Blakiston, Watson, and by the writings of Gerhard, Bowditch, and Swett in our own country.

A memorable event in the medical history of this period was the publication, in 1827, by Richard Bright, of his "Reports of Medical Cases." A full analysis of this work appeared in the February number of the first volume of the "American Journal of the Medical Sciences," in 1828. The editor concluded the article with these words: "As Dr. Bright's volume is so costly as to preclude its circulation in this country, and as its contents are highly interesting, we have been induced to extend our notice beyond the usual limits, and have placed in our periscope some of the most interesting cases." I make this reference to show how quickly important contributions to our knowledge abroad were brought to the notice of medical readers on this side of the Atlantic. The name *Morbus Brightii* had not as yet been applied to affections, the anatomical characters of which Bright was the first to describe. Naturally, the morbid anatomy of these affections for some time engrossed attention; the fruits of their clinical study could not be gathered at once. It was not until the publication of Christison's work, in 1839, that the profession in Great Britain and this country began to grasp the scope of

their pathological relations. Bright himself could hardly have dreamed of the extent of the new field in pathology which his researches opened up. Our present knowledge of uræmic conditions in etiology and symptomatology, acquired chiefly during the last quarter of the present century, he could not have anticipated. I think it not too much to say that the progressive developments in this field of pathology have been as fully appreciated and their influence on the practice of medicine has been as marked in this as in any country.

The quotation just made from the "American Journal of the Medical Sciences" leads me, in this connection, to refer to analytical reviews and bibliographical notices as affecting medical progress. This journal, succeeding the "Philadelphia Journal of the Medical and Physical Sciences," in fact a continuation of the latter, dates from the year 1827. In the advertisement accompanying the first volume it was said: "Arrangements have been made to import regularly from London and Paris every new medical work, and to obtain in exchange almost every medical journal published in Europe. These will be placed in competent hands for analysis or review, and whatever is valuable in their contents shall be laid before our readers as early as possible." Writing, as I do, after having examined the volumes issued during the second quarter of the present century, I am impressed with the fidelity with which this promise was fulfilled. They contain reviews or notices of the important publications in England, France, Germany, and other countries during that period. This journal, continued up to the present time, under the same title, issued by the several successors of the first publishers, Lea & Carey, for more than forty years under the editorship of Isaac Hays (who, although a veteran in journalism, is not yet entitled to be called venerable), has had a powerful influence on the progress of Medicine in this country. This influence has been exerted not less by its original communications than by its reviews, notices, and general intelligence. Without any disparagement of the numerous able and useful periodicals during the last half century, may we not, with a feeling of pride, refer especially to a journal which, with such a history, is now the oldest representative of medical periodical literature, and which has probably a circulation larger than that of any other in Europe or America?[1]

The physiological discoveries of Magendie, Charles Bell, and Marshall Hall, relating to the nervous system, forerunners of a further unfolding of the functions of this system within late years by Bernard, Brown-Séquard, and others, had a considerable influence on pathology and practice; an influence which, by means of republications, reviews, and controversial articles, was perhaps felt as speedily here as abroad. But a memorable event in physiology, having direct practical bearings, was of American origin, namely, the observations and experiments of Beaumont in the case of Alexis St. Martin, published in 1833. Beaumont's prolonged, patient, careful, conscientious researches, demonstrating more completely than had yet been done the agency in digestion of the gastric juice, furnished important facts in relation to its quantity, its composition, the circumstances affecting its secretion, together with the relative digestibility of different articles of food, and the influence upon digestion of local and general morbid conditions. These facts have, in the main, been corroborated by experimental physiologists who have since studied

[1] The American Journal of the Medical Sciences is the oldest of living journals, considering it as a continuation of the Journal of the Medical and Physical Sciences.

the functions of the stomach by means of artificial gastric fistulæ in inferior animals. Looking at the fruits of his labors as affecting the progress of medicine, may we not claim for them sufficient value to entitle the name of Beaumont to be mentioned in connection with the names of Magendie, Bell, and Hall?

The writings of Broussais are to be reckoned among the memorable events of the period under survey. The "History of Chronic Phlegmasiæ" was translated by Isaac Hays and R. Eglesfeld Griffith in 1831; in the same year his "Treatise on Physiology applied to Pathology" was translated by John Bell and La Roche, and in 1832 his work entitled "Propositions Embodying Principles of Physiological Medicine" was translated by Hays and Griffith. By these publications the profession in this country were made acquainted with the so-called new physiological doctrine more commonly known as Broussaism. For a time it had not a few ardent supporters, prominent among whom was the late Professor Samuel Jackson, whose work, entitled "Principles of Medicine," was based upon it. The circulation of this work was arrested by the author, as was understood, because it became inconsistent with his more matured views, an act which reflected honor upon his character as an honest inquirer after truth. Like the systems of Rush, Brown, and Cullen, Broussaism had its day; but, unlike the preceding systems, this has had no successor. With the fall of Broussaism the race of *isms* became extinct; theoretical dogmas as the basis of different schools or sects have been relinquished to the illegitimate *pathys*.

Broussais, however, was not the sole last survivor of the race. Coeval with this doctrine was one of American birth, namely, the doctrine of Cooke. John Esten Cooke, a man of much ability and of sterling moral worth, at that time Professor of the Theory and Practice of Medicine in the worthily distinguished Transylvania University, in 1831 published at Lexington "A Treatise on Pathology and Therapeutics." After commenting on the extreme proneness of medical philosophers to frame hypotheses instead of following the true spirit of inductive logic, he enunciated the doctrine that hepatic congestion is the *fons et origo* of most of the phenomena of disease. This doctrine was not entirely new, but it was carried by Cooke to the extreme point of culmination. Its practical effect on therapeutics was an excessive use of purgatives, and especially calomel in large doses. In those days of hypercatharsis, patients were purged by the followers of this system as patients had never been purged before. Cooke was afterward Professor of Practice in the Medical School at Louisville, and for a long time a dose of calomel which he had prescribed was preserved as a curious specimen in the museum of that school. With the audacious extravagance which characterizes American humor, it is related of a Kentucky yeoman that, finding in one of his fields crude mercury in considerable quantity, visions of wealth from a mine of quicksilver loomed up in his imagination; but they were dispelled when it was ascertained that the spot had been a burial ground for the remains of patients treated in accordance with this doctrine!

An influence which contributed to prevent the formation of new systems of medicine, and to promote in not a small degree, true medical progress, belongs to the works of Louis. Louis's "Researches on Phthisis," translated by Cowen, revised and altered by Bowditch, his "Researches on the Typhoid Affection," translated by Bowditch, and his "Researches on the Effect of Bloodletting in some Inflammatory Diseases," translated by Putnam, with a preface and appendix by James Jackson, were

published in this country in 1836. His "Researches on Yellow Fever," translated from the manuscript by Shattuck, appeared in 1839. I need not enter into an exposition of the method of study which Louis exemplified in these works. It would be out of place, even if time permitted, to discuss here the merits of this method. Of its influence, suffice it to say that it has led to a large proportion of the actual knowledge of the clinical history of diseases, of their anatomical characteristics, of the diagnostic value of symptomatic phenomena and of therapeutical agencies, which has been acquired within the past forty years. Of the permanency of acquisitions in knowledge to which it has already led, let this statement suffice; the works of Louis, within the limits of the means of observation available in his time, are as valuable to-day as at the date of their appearance. The method of study inaugurated by him was a new departure of the Philosophy of Medicine, offering now, and in all time, rich returns for honest work.

The study of medicine in the direction given to it by the labors of Louis, led to an appreciation of the importance of knowing the natural history of diseases. Jacob Bigelow struck a key-note in his discourse on "Self-limited Diseases" in 1835. From that time our teachers and writers have been alive to the truth that knowledge of the intrinsic tendencies of diseases must underlie actual progress in therapeutics. The true spirit of medical philosophy, after its emancipation from the thraldom of theoretical systems, speedily found expression in the works of Stillé, Bartlett, Ware, Holmes, and others. The influence was soon felt in medical practice. Progressively, in this country, practitioners became more reserved in the use of heroic measures of treatment, and polypharmacy fell into disrepute; they became less ambitious to be the masters, and more content to remain the servants of nature; efforts were made to bring experience to the test of the numerical method of investigation, and "expectation," using this term in its true sense, took its proper place in clinical medicine. Is it presumptuous to affirm that these criteria of veritable progress have been not less, and perhaps, in some regards even more marked in the medical practice of American physicians than of those in the older countries, by whose writings and oral teachings we have largely profited?

Warned by the length already of this discourse, I must not linger with other topics relating to the history of the second quarter of the present century. There are two events of importance, belonging to this period, to be added to those already noticed, namely, the publication of the United States Dispensatory and the organization of the American Medical Association. The first edition of the Dispensatory by Wood and Bache was published in 1833. With the additions and improvements called for in the successive thirteen editions, this work has met fully the wants of the practitioner, holding its place still, without a competitor, and, to say the least, comparing favorably with similar publications in other countries. The American Medical Association was instituted in 1847, by the Profession, "for the protection of their interests, for the maintenance of their honor and respectability, for the advancement of their knowledge and the extension of their usefulness."[1] Since its organization annual meetings have been held, excepting a portion of the period during which intercourse of the Profession of the North and the

[1] Quoted from the resolution adopted by the Convention which preceded the first meeting of the Association.

South was suspended by civil war. At these meetings representative members of the profession in all parts of the country have been brought together under circumstances conducive to fraternal union and good fellowship; and the twenty-six volumes of "Transactions" contain much that is valuable, more especially in relation to climatology, and the diseases prevalent in different parts of the United States. The Association has encouraged original papers by prizes, and has kept the Profession informed of important contributions to knowledge by reports on Medical Science and Literature, and on the Progress of Medicine, Surgery, and Midwifery. In these different ways its influence has been not inconsiderable.

The literature of any province of knowledge gives the truest expression of progress, while it does most toward promoting it. In this period, the increase of publications was proportionate to the growth of our nation in population and material interests. I shall enumerate some of the valuable contributions to practical medicine, without attempting to give a full list of them. My distinguished friend who will address the Congress on the subject of American Medical Literature will supply deficiencies here and elsewhere.

The "Practical Esssay on Typhous Fever" by Nathan Smith, although printed in 1824, may be reckoned as entering into the literature of this period. One of our most accomplished writers (Bartlett) in his work on the Fevers of the United States, published in 1842, says of this "modest unpretending essay:" "To an American practitioner it is worth infinitely more than all the modern English treatises put together." The attainments and skill of that great man, united with perfect integrity and simplicity of character, excite our admiration; nor can we less admire the energy and pluck which led him to establish the medical school of Dartmouth College, himself the sole professor for ten years, a faculty of one, but that one Nathan Smith, prominent among the medical heroes of the early age of our republic.

The progress of knowledge of the essential fevers, within this period, was in no small measure affected by contributions to American medical literature. Gerhard, Pennock, in 1836, and Shattuck, in 1839, were the first to contribute ample clinical observations, showing the essential points of distinction between typhus and typhoid fever. To these observers belongs the credit of the doctrine of the non-identity of these fevers, a doctrine which, after twenty years of hesitation and discussion, was generally recognized as sufficiently established. James Jackson's Report, in 1838, and Enoch Hale's paper, in 1839, embodied the results of the application of the method of study inaugurated by Louis to the common continued (typhoid) fever of New England. The treatise on the Fevers of the United States by Elisha Bartlett, published in 1842, the fourth edition revised after the author's death by Alonzo Clark, has been, almost up to the present time, a standard work, and is too well known to need any commendation. Meredith Clymer, in an article on typhoid fever, published in 1846, was the first to describe in this country the disease now known as relapsing fever. It may, perhaps, be assumed that the communicability of typhoid fever was first demonstratively shown by the circumstances attending an outbreak of the disease in a small hamlet in Western New York, in 1843.[1]

[1] Account of an epidemic fever which occurred at North Boston, Erie Co., N. Y., in October and November, 1843. American Journal of the Med. Sciences, July, 1845.

Important contributions relating to the differential characters of remittent fever and its clinical history were made by Stewardson, in 1841 and 1842; by Swett, in 1845; by Anderson, Frick, Stillé, and Boling, in 1846. Our present knowledge of this disease rests mainly on the facts contained in these contributions.

The doctrine of the combination of different fevers, or the "blending of types," was taught by Dickson and Drake, in contradiction of the theory of Hunter, which denied that the special causes of two or more diseases can act conjointly in the organism. The doctrine is now sufficiently established by clinical proof. Drake, in 1832, contributed a series of papers in advocacy of the animalcular hypothesis of the origin and diffusion of epidemic cholera, and subsequently, in his great work, he argued at length, and cogently, for this hypothesis as applied to the etiology of other diseases. And in this connection the remarkable work of Mitchell on the "Cryptogamous Origin of Malarious and Epidemic Fevers" may be referred to. This work was published in 1849. If, as is perhaps more than probable, the truth of the "germ theory," as it is now termed, be hereafter confirmed by direct observation, the writings of Mitchell and Drake on this subject will be cited as evidence of an insight into the mysteries of etiology beyond that of their contemporaries; and at the psesent moment, when this theory is the subject of so much interest, these writings claim careful study.

In referring to Drake, I cannot forbear a passing tribute of admiration of the talents and labors of that truly great man. As an eloquent expounder of medicine he was without a superior; and when we look at the work which he performed, as a writer, a medical teacher, and in travelling over large sections of our country, gathering from personal inquiries and observations materials for his treatise on the "Principal Diseases of the Interior Valley of North America," which he did not live to complete, we must consider his name as worthy to be enrolled in the list of our medical heroes.

The treatment of the periodical fevers with large doses of quinia, together with the discontinuance of the so-called preparatory treatment; the extent to which this drug may be given without risk of harm; the entire safety of its use in the state of pyrexia; and the advantage of arresting the paroxysms as speedily as practicable—points at that time in opposition to the prevalent practice—were set forth in this country as early as 1841.[1] It may be added that the views which have since prevailed in reference to these points were adopted earlier, and diffused more quickly with us than in other countries.

The literature of this period embraces many contributions relating to yellow fever. Prominent among the contributors are the names of Barton, Barrington, Cartwright, Dickson, Nott, Simons, C. H. Stone, Ashbell Smith, and Fenner. Our medical literature from the earliest period of our history has abounded in writings on this subject, to which justice has been done in that marvel of laborious erudition, the Treatise on Yellow Fever, by La Roche, published in 1855. The contributions by the authors just named, and others, led to the present prevailing belief in the non-communicability of the disease, but the portability of its special cause, and in the injudiciousness of perturbatory measures of treatment.

Of contributions other than those relating to the essential fevers, and

[1] *Vide* article in the American Journal of Medical Sciences, No. for October, 1841.

in addition to those referred to in other connections, I shall mention several, by no means undertaking to enumerate all, or even the greater part, of those which promoted progress by extending, more or less, the boundaries of knowledge.

John Ware, in an analysis of his recorded experience published in 1831, brought to bear the numerical method upon the use of opium in the treatment of delirium tremens. The results of the analysis have ever since been a basis of practice in that disease. The same writer in a series of articles on croup, published in the latter part of this period, delineated clearly the different affections then embraced under that name. The distinctions derived from his recorded experience and founded in nature, are as valid to-day as at the date of their publication.

John B. Beck's treatise on infant therapeutics in 1849, had much influence in restraining the over-use of potential measures in the treatment of diseases in infantile life. The article by Cammann and Clark on auscultatory percussion, published in 1840, introduced a new method in physical exploration, and gave the results of its employment in obtaining accurate measurements of internal organs. The treatise by Amariah Brigham on "The Influence of Mental Cultivation on Health," published in 1832, and the "Inquiry concerning the Diseases and Functions of the Brain, Spinal Cord, and Nerves," published in 1840, contained original views important in their practical applications. Horace Green's treatise on "Diseases of the Air Passages," published in 1846, originated the practice of making topical applications within the larynx, the feasibility of which was accepted after considerable distrust and acrimonious discussion. In 1841 Alonzo Clark began to treat acute peritonitis with large doses of opium, excluding calomel and venesection. He carried the use of opium further than had previously been done, and ascertained that the disease in some cases causes a marvellous tolerance of this remedy. The treatment exclusively or chiefly by opium has been generally adopted in this country, and may properly be called the American method. Moreover, the success of the use of opium freely in peritonitis, has led to the useful employment of this remedy in other affections. Bowditch's first employment of suction for the withdrawal of liquid from the pleural cavity, through a small canula, was a few weeks after the end of this period; but he then carried into effect a procedure which he had previously maturely considered and resolved upon. I may, therefore, in this connection, refer to an improvement in the practice of medicine, the merits of which it were needless at this day to discuss. The use of the term *aspiration*, in place of the homely word *suction*, and the invention of more complicated instruments, have recently given to the procedure a new and more attractive aspect. The successful practical application of the method, however, belongs to the history of American medicine; and, taking into view the wide range of the application, it is certainly worthy of being reckoned among the important events affecting medical progress within the past century.

These contributions, cited from personal recollection, without searching medical annals in order to extend the list, suffice to show that during this eventful period, the field of practical medicine in this country was not without efficient workers, and not wholly unproductive. Directing attention to another aspect of medical literature, the evidence and the means of progress are shown by the number of works relating to the Principles and Practice of Medicine. Original works in this department of literature began to appear prior to the second quarter of the present

century; but the number within this period was considerable, namely, the works of Dewees, Eberle, Dunglison, Cooke, Fort, Gallup, Hosack, Chapman, Paine, Dickson, Stillé, and Wood. A considerable number of republished works, generally with notes or additions by American editors, were added to those of native production. In this list are the works of Marshall Hall, revised and much enlarged by Jacob Bigelow and Oliver Wendell Holmes, Craigie, Billings, Elliotson, with notes by Stewardson; Gregory, with notes by Peixotto, Alison, Williams, and Latham; Graves's System of Clinical Medicine, with additional lectures by Gerhard, and Stokes's lectures with an equal number added by Bell; Andral's Clinique Médicale, translated by Spillan; Chomel's Pathologie Générale, translated by Oliver and Morland, and the lectures by Watson. In addition to works of this character were several on a larger scale designed for reference in regard especially to the practice of medicine, namely, the London Cyclopædia of Practical Medicine, edited with large additions by Dunglison; Copland's Medical Dictionary, with copious notes by Lee; the Library of Practical Medicine by Tweedie, with American notes and additions by Gerhard. To this class of works belongs the American Cyclopædia, edited by Hays. When to these productions, native and foreign, we add the original treatises by Dewees, Eberle, Condie, and Stewart on the diseases of children; not a few works, original and imported, treating of particular classes of diseases; the works on materia medica by Barton, Bigelow, Coxe, Chapman, Eberle, Dunglison, Griffith, Carson, Paine, Ellis, Beck, Bell, Harrison (all native productions), and the medical dictionary, by Dunglison, have we not, in this point of view, evidence of an activity in the means of progress creditable to a nation under the age of fourscore years? Was not the growth of medical literature, in this aspect, proportionate, to say the least, to the increase of population, the extent of territory populated, and the advancement in other provinces of practical knowledge?

Survey for an instant this period from one other standpoint, namely, the number and character of prominent members of the medical profession. We must recognize in the personal influence exerted by individuals in their respective spheres as teachers, practitioners, and members of society, one of the most potent of the agencies concerned in progress. Some of those who, regarded from this standpoint, have a record most honorable to themselves, are still with us—may they live long to receive the honor which is their due! On the list of those who have departed for another life are such names as James Jackson, Samuel Jackson, Samuel Jackson of Northumberland, Chapman, Mitchell, Hosack, Ware, Gerhard, the two Becks, McNaughton, Pennock, Revere, Drake, Joseph M. Smith, Francis, Parrish, the elder Shattuck, Dunglison, Dickson, Fenner, Stearns, Bartlett, Moreton Stillé, Caldwell, LaRoche, Lee, Swett, Twitchell, Tully, Mussey; and to these, and such as these, might be added the names of a host of practitioners who, without engaging in the labors of literature, or in medical schools, exerted within greater or lesser spheres, an influence promoting the diffusion of knowledge and ennobling the character of the physician.

Having reached in our survey the third quarter of the present century, I hasten to say that the concluding part of my address will be brief. I shall not enter into historical details for reasons which are obvious, in addition to the desire not to presume too much upon your patience. The professional lives of a considerable proportion of those whom I address

embrace the last twenty-five years, and everything of importance pertaining to the history of this period is familiar to them. Moreover, to particularize events, to notice contributions to medical literature, and to refer to individual members of the profession, would involve questions of delicacy which I prefer to avoid. I shall therefore limit myself to a few remarks on topics suggested by the inquiry, what are some of the characteristics of medical progress in the United States within the last quarter of a century and at the present time?

The predominant and most distinctive characteristics of progress in the medical science of this period, are derived from the developments in histology. Pathological histology followed the acquisition of a new territory in anatomy by Schwann in 1839, as the morbid anatomy of the tissues had followed the *Anatomie Générale* of Bichat; and the developments by means of the microscope in the study of disease are to be dated chiefly after the middle of the present century. Prior to this, however, microscopy had enlisted the interest of medical teachers and students in this country, as well as in Europe; and as early as 1851, a prize was awarded to Burnett (whose untimely death was a loss to science), by the American Medical Association, for an essay on "The Cell; its Physiology, Pathology, and Philosophy," embracing deductions from elaborate original investigations. Histology entered at once into the formal instruction of many, if not most, of our medical schools. Our pilgrim sons who previously had paid homage at Paris, Edinburgh, Dublin, and London, for the past quarter of a century have gathered in larger numbers at Vienna and Berlin. Histological researches having been prosecuted with most zeal by our Austrian and Prussian brethren, German medical literature has thereby acquired a prominence and an influence which might be cited as a characteristic of Medicine at the present time. I need hardly say that it would not be fitting on this occasion to inquire into the actual increase of our knowledge to be ascribed to the progress of histology. That a vast and fruitful domain in physiology and pathology has been brought under cultivation, and that it has already yielded rich harvests, cannot be doubted. Herein will the history of Medicine in our day be ever memorable. And the future is full of promise when obstructions from doctrines connected with bioplasm, cyto-blastema, autogenesis, and the so-called cellular pathology shall have disappeared. With improved methods and increased power of microscopical investigation, it may not be long before the *germs* (using this word in either a literal or a figurative sense) of infectious and contagious diseases will be brought within the scope of ocular demonstration. When this is accomplished, it requires no prophetic sagacity to foresee that the influence on the progress of pathology, therapeutics and prophylaxis must be immense. Perhaps in the developments of the future, the labors of our zealous workers will bear an honorable part. I will venture to add that we are not to expect from histology a royal road through pathological mysteries; but, profiting by past experience, and avoiding doctrines based largely on hypotheses, our researches in this direction should go hand in hand with the tedious, but, if properly conducted, safe studies in clinical medicine.

I have alluded to pilgrimages to other countries. The desire to profit by the distinguished schools abroad has always been an American characteristic. In the first half century of our national existence, the schools in Great Britain were chiefly resorted to; in the third quarter of the century, those of France had the preference, and in the last quarter the

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German schools. Some members of the Profession look with disfavor on this yearning for foreign instruction. It is thought to be antagonistic to a proper sense of a national independence, implying a sort of vassalage unfavorable to original developments. I do not share in these views. For us, in our adolescent age, to regard with a certain degree of deference the institutions of older countries, is not a confession of dependence, but a becoming modesty. It is true that opinions which have travelled across the Atlantic acquire in the estimation of many an exaggerated importance; the weight of far distant authority is apt to be over-estimated. This is, in a measure, counteracted by judgments formed after personal inspection. A discriminating mind judges fairly from intercourse with distinguished teachers and writers of their claims as authorities. In this point of view our sojourners abroad benefit, not only themselves, but their brethren at home. In some instances, it must be admitted, personal admiration or attachment leads to a discipleship which is exceedingly unfavorable to the progress of the disciples; yet these render service by diffusing the opinions of their masters among those who, uninfluenced by partisan zeal, consider and judge without prejudice. That our independence in thought and judgment is not compromised, is shown by what I believe may be stated as a fact, namely, no one who has returned from abroad and remained a mere satellite of a foreign luminary, has ever attained to any great distinction in the minds of his countrymen.

A characteristic of the past quarter of a century, and of the present time, relates to the increase of American medical literature. Native productions have been and are steadily increasing in number, and this not to the exclusion of, if, indeed, there have been any decrease in, the republication of foreign works. In order to appreciate the significance of this increase, as showing the progress of native literature, we must consider that American productions are in immediate competition with English works, the merits of which have been established, and for the republication of which the authors can claim no royalty. Much has been said of the injustice done to American authors by the want of an international copyright law. Without entering into any discussion of this topic, I will simply say that such a law is called for, not so much for the encouragement of our own literature, as in behalf of the just claims of foreign authors. The medical profession of this country have given ample evidence of a disposition to receive with discriminating favor native productions. I presume I am not in error in saying that our medical publishers are always ready to print any work which, from its intrinsic merits, may offer a fair promise of success. This is evidence of a demand for native works, inasmuch as it would imply a marvellous disinterestedness to suppose that publishers are actuated solely by a desire to promote a national literature. It is noteworthy that not long after French authors were held in special favor, the proportion of medical readers who were able to study the works of these authors in the language in which they were written, became so large as to render translations unprofitable to publishers; and it is probable that ere long this will be found to be the case in regard to publications in the German language. There will then be no occasion for a remark which I have heard imputed to a medical teacher in this country, that to know the German tongue is as essential for the student of medicine as a knowledge of anatomy!

I cannot forego a few words respecting the disposition of our English-speaking foreign brethren toward American medical literature. Many years ago an English reviewer and humorist, in a moment of irritation

from circumstances of a personal nature which rendered it perfectly natural, exclaimed, "Who reads an American book?" This has been over and over again quoted as a fair expression of the estimate in England of our bibliography. It should never be quoted except to add that its author was more unjust to his own countrymen than to us. All who are familiar with the medical periodical literature of Great Britain must have been impressed with the great candor and consideration generally shown in reviews and notices of American works. Not unfrequently these have received far more approbation abroad than at home. This is certainly a fitting occasion to declare that there is no ground to impute a prejudice, either in Great Britain or any other country, against our national literature.

That our works and periodical literature have in general a practical character is readily accounted for by our youth as a nation, and the rapid extension of population over a vast continent. With increase of years, and the altered circumstances incident thereto, our future in medical literature, it may safely be predicted, will show a wider scope. Thus far in our progress, the pressing claim upon our writers has been the wants of our widely separated brethren as regards qualification for the active duties of the practitioner.

The statement just made suggests the leading characteristic of our medical schools, namely, practical instruction. I shall not encroach upon the ground of the distinguished physician who is to address us on the subject of American medical education. He may, perhaps, point out defects in our educational institutions. That there are defects, is not to be denied. But, that we may not do injustice to our predecessors and our contemporaries, let it be considered how peculiar is the situation of the profession of this country in this regard. The whole responsibility for medical education, inclusive of the means for its promotion, rests, for the most part, with the Profession. Medical schools are without the jurisdiction of the general government, and our numerous States can only control, of course, those within their respective borders. Hence, uniformity throughout the country in legislating in behalf of medical education is impracticable. Legislative provisions for the increase and diffusion of medical knowledge are apt to be regarded in the light of appropriations for the benefit of a particular class of citizens. A diploma or a license to practise medicine from some legally authorized body is easily obtained, and the law practically places no obstacle in the way of practising without either. Now, in view of these facts, which are, of course, familiar to all of us, how are we to explain the annual gatherings of medical students and practitioners in large numbers in Philadelphia, New York, and other cities, many coming from distant parts of the Union, incurring expenses which often involve much exertion and self-denial? What other explanation is there than, *first*, that certain of our medical schools afford large opportunities in the way of practical instruction; and, *second*, that there is a wide-spread yearning for these opportunities simply on account of their intrinsic worth? Have we not here the evidence of a spirit of progress in both teachers and pupils, which is creditable to the past and present, as, also, an earnest for the future? Let us strive to remove defects, to correct abuses, and to improve, as fast as practicable, our system of instruction; but let us not be led into a disparagement of what has been done, and is now being done, in behalf of medical education.

A few words in conclusion, concerning characteristics which relate to medical ethics. At the Convention held in 1847, resulting in the organization of the American Medical Association, a code of ethics was adopted, and it has since been formally recognized as having binding force upon members of state and local societies throughout the Union. This code embodies rules which were in accordance with the usages of worthy members of the profession in this country, and with established ethical principles in other countries. It defines simply and clearly the line of conduct to be followed in the relations of medical men to each other and the profession at large, to their patients, to the public, and to those who, adopting a practice based on an exclusive dogma, or from unworthy acts, are not to be considered as professional brethren. This code has remained unaltered and with undiminished force for more than a quarter of a century. I state this fact as a characteristic which reflects honor on the representatives of American Medicine. I claim, in behalf of the Medical Profession of the United States, that the great majority of its members have been loyal to the code of ethics. With no penalty for violations of ethical rules beyond the loss of professional fellowship, a penalty which may be made subservient to selfish advantage, loyalty is secured by motives of self-respect, and regard for the power of the Profession. It may perhaps be claimed that in no other country is the line dividing legitimate and illegitimate medicine more distinctly defined than with us; and nowhere are instances of affiliation with irregular practitioners more infrequent. The same may perhaps be said of the number of those who, having once been in respectable professional standing, have yielded to the temptation of originating secret nostrums and patenting remedies or inventions. These characteristics are consistent with the fact that in no other country does the Profession of Medicine hold a higher social position or exert a greater public influence than in these United States of America.

Gentlemen, Members of the International Medical Congress: At this our Centennial Anniversary it has seemed to me appropriate to survey "Medicine and Medical Progress in the United States" from the points of view which excite feelings of gratification, hope, and faith. We should by no means shut our eyes to errors, defects, and short-comings; but there are other occasions more suitable for regarding these. The history of American Medicine for the past century contains much that we may rightly be proud of; we have no reason to be ashamed of its present condition, and need we doubt that the spirit of progress which has thus far guided it, will lead onward to the fulfilment of a glorious destiny!

ADDRESS ON HYGIENE AND PREVENTIVE MEDICINE.

BY

HENRY I. BOWDITCH, M.D.,

PRESIDENT OF THE STATE BOARD OF HEALTH OF MASSACHUSETTS.

Gentlemen of the International Medical Congress:

I have been requested to speak to you on Public Hygiene and its great resultant, State Preventive Medicine, as it appears to an observer looking back upon the Centennial Period now just closing.

As a measure of vital importance to the well-being of any community, and, as such, worthy to be cheerfully and amply sustained by great cities and States, Public Hygiene, as we now understand that term, has, till within a very short time, been wofully neglected, save when, under the stimulus of some great and terrible epidemic, frantic but temporary efforts have been made to stay the plague by Hygienic or by other means.

Of late, however, a new and better era seems opening to our view, and State Preventive Medicine affords us higher hopes for all coming time.

To this last, this noblest phase of Public Hygiene, its very gradual evolution out of the dogmatism and skepticism of the past, its present status, our duties relative thereto, and our golden hopes for its future, I crave your candid consideration during the brief hour I shall have the honor of addressing you. If, at the termination of my remarks, our foreign associates feel that I have given them but little information, and my countrymen find that I have said but little in praise of my country, one and all of you, I trust, will generously allow me the credit of having endeavored at least to speak the exact truth about the various States, and the United States as a Nation, in their relation to these matters. May I dare to hope that the plain unvarnished story of our present condition and short-comings, in regard to practical public hygiene, may do something to stimulate each State and the United States governments, to do more thoroughly the plain duties that lie open before them?

Before laying down the propositions I intend to defend, let me say we cannot, in the consideration of them, confine our view simply to this country, but must frequently refer to the men and ideas of other countries, as well as to those of our own.

And let me add that in citing the names of men who have been prominent here or elsewhere, I shall do so without intending either praise or blame, but simply as those of illustrious representatives of great systems of medicine or of wide currents of thought, upon which the greater part of mankind has been borne almost unconsciously along.

NATURAL DIVISIONS OF THE CENTENNIAL PERIOD.

In its Medical and Social ideas, the past centenary easily divides itself into three unequal epochs, viz.: I. From 1776 to 1832, the era of theory and of dogmatism; II. From 1832 to 1869, or that of strict observation,

and of bold, often reckless, skepticism; III. From 1869 to 1876, which is destined to continue and progress while the nation itself lives, the noblest and most beneficent of all, viz., that in which the profession, joining heartily with the laity and aided by the material and intellectual resources of great States, will study to unravel the primal causes of all disease with the object of preventing it. It is the epoch of State Preventive Medicine.

First Epoch, or that of Medical System-making, filled with an Overweening Confidence in our Art, with little or no Faith in the Vis Medicatrix Naturæ.

(From 1776 to 1832, or thereabouts.)

The influences which governed this period and the previous centuries, may be briefly sketched as follows: The illustrious Boerhaave began to enunciate his doctrines of disease at Leyden in 1701. His doctrines held sway in America until about 1765, or ten years before the opening of our centennial period. Following closely after him came Hoffmann, Cullen, Brown, and Darwin, and each with his own peculiar system.

Our ingenious and renowned countryman, Benjamin Rush, in 1790, proclaimed his own idea, viz., that a convulsive motion of the arteries is the proximate cause of all fevers, however different the causes may be.[1] Benjamin Rush was one of the most noteworthy men this centenary has produced, and as he had more influence than any other one person upon medical opinion during the first epoch; as he is called by one of his admirers "the American Sydenham," it seems to me not improper that I should on this occasion mention some facts of his life. He was born Dec. 24, 1745, on his father's plantation, fourteen miles from Philadelphia: consequently he was just entering the prime of manhood in 1776. Seven years before that, at the early age of twenty-four, he had been appointed Professor of Chemistry in the University of Pennsylvania.

During the Revolutionary war he resigned this professorship, and was made Medical Director of the Middle Department of the Continental Army. In 1789 he resumed his professorship of chemistry, and in October of that year became Professor of the Theory and Practice of Medicine, which office he held until his death in 1813. During all these years he energetically and ably defended his system, as he had previously upheld Cullen's. He has left accounts of various epidemics of yellow fever and other observations and essays, which are valuable as records of the period and as evidences of his intellectual and moral worth. He was essentially a medical system-maker like his predecessors. He believed fully in heroic remedies, and rather scoffed at Nature's influence in disease. His pupils spread themselves over some of the Eastern but more over the Middle, Western, and Southern States, carrying with them not only the wisdom but the errors of their great master. He thus had vast influence on the medical ideas of the country. This becomes still more evident when we remember that he held the chief professorship in the foremost medical school in the Union, and that, towards the end of his career, the number of students that listened to his eloquent and per-

[1] Rush's Med. Inquiries and Observations, Philadelphia, vol. iii. p. 11.

suasive tongue, amounted to about four hundred, annually drawn to Philadelphia from every part of the country.

The reputation of our profession among the more intelligent of the laity, when Dr. Rush was in his full glory, is brought out in the following extract from a letter from Thomas Jefferson to his friend Dr. Wistar in 1807. He writes, "I have myself lived long enough to see the fashions of Hoffmann, Boerhaave, Stahl, and Brown succeed one another like the shifting figures of a magic lantern, and their fancies like the dresses of the doll-babies from Paris; becoming from their novelty the vogue of the day, and yielding to the next novelty their ephemeral favor."[1] We may well fear that Mr. Jefferson's opinion of our profession was not much improved when Dr. Rush dogmatically proclaimed, during a heated discussion with some physicians of Philadelphia, that the time must and will come when the general use of calomel, jalap, and of the lancet without a physician's advice, will be considered "among the most essential articles of the knowledge and the rights of man"!

But Dr. Rush's theory and dogmatism were destined to fall under the influence of the fascinating theories of Broussais. Broussais was born in 1772, when Rush had already given evidence of his great powers as a teacher. This great Frenchman died in 1838, after his authority, which had been world-wide, had begun to decline. Broussaism spread widely in America, but less in New England, which had been trained more to close observation under the great masters in medicine, Dr. E. A. Holyoke, Dr. James Jackson, and others, than it did in the Middle, Western, and Southern States. In or about 1832, the system began to fall in America, as it had been previously falling in Europe. With its fall our first epoch terminates. The whole tenor of medical opinion during the period, as you readily perceive, with its theories and systems concocted in great minds, was not in the smallest degree suited to the growth of State Preventive Medicine.

Second Epoch; or that of Observation, and accurate recording and subsequent analysis of Facts; with an extreme confidence in Nature's power in Disease; a corresponding skepticism in regard to the use of Drugs; and, finally, dim presages of Preventive Medicine.

(From 1832 to 1869.)

The second epoch presents characteristics precisely the reverse of the first. Louis and his numerical method stand prominent in it.

Louis may be called the lineal scientific descendant of such men as William and John Hunter, Morgagni, etc. His nature rebelled against the dogmatism of the past, unsupported as it was by well-ascertained and recorded facts. You all know the great and wide-spread influence of his works in Europe; Broussaism fell before them. The epoch in America commenced when Dr. Gerhard, of Philadelphia, and Dr. James Jackson, Jr., of Boston, and other pupils of the observation school, returned to America. Drs. Gerhard and Jackson had high mental powers finely cultivated. Both came back devoted friends, and admirers of Louis as a man and as a medical reformer. They were full of the new ideas, and enthusiastic defenders and expounders of them. The influence of these

[1] Works of Thomas Jefferson, Washington, 1854, vol. ix. p. 107.

ideas on the currents of medical thought in America was immense. It extended gradually over the whole country.

It must be admitted that at times there was something unwarrantably supercilious in the terms in which some of Louis's disciples, rather than Louis himself, spoke of the past. It seemed as if they would, if they could, wholly ignore the past in their desire to build up the medicine of the present and of the future upon the firm basis of strictly recorded fact. By this exclusive spirit of many of the "Numerical School," while infinite good has been done, a great apparent evil to practical medicine has arisen. I allude to the more or less complete skepticism on the part of many of the profession, and of the laity, in regard to every species of medical treatment, except perhaps that of good nursing. The pendulum of human thought, as always happens in such cases, has swung far beyond the line of exact truth, so that from a blind faith in the supreme power of our art, noticeable in our first epoch, many have passed into an equally blind and fanatical skepticism in reference to it. Nevertheless I presume all will admit that Louis's school was needed to sweep away all false systems unsupported by well-recorded and sufficiently numerous facts.

Louis's doctrines, though vehemently opposed by some who had grown old under the previous epochs, were received by most young men with joy, and philosophical minds among the elders were charmed with the accuracy observable in Louis's Numerical Method, as shown in his writings. Under the direct or indirect influence of his writings and Numerical Method, which Dr. Guy[1] considers the guiding power of the present time, a very large number of independent observers and workers have been educated in this country and in Europe. The mottoes selected by Louis as his own guides are identical with those of modern science.

In addition to the specific disciples of Louis, there were three others, all men of great intellectual ability, who by their writings and teachings powerfully influenced this epoch. I allude to Jacob Bigelow and Elisha Bartlett, of America, and John Forbes, of England. Two of this trio are now dead. Dr. Bigelow entered his 90th year last February. His life nearly spans the century, and as it closely unites the first and second epochs, I hope you will permit me to allude to him somewhat as I did to Dr. Rush. He was born February 27, 1787; that is, two years before Dr. Rush resumed his professor's chair in the University of Pennsylvania, at the close of the Revolutionary war. After a most honorable and successful life as a physician, and as professor of the Harvard Medical School, Dr. Bigelow has been, for years, totally blind, and hopelessly unable to move from his bed for more than eighteen months. To a casual visitor he seems as bright and as witty as ever, while his beautiful and uncomplaining submission to what most persons would deem a sad fate, raises him in the eyes of all beholders even higher than he was at the period of his greatest energy and fame. A visit to his bedside affords a delightful sense of repose from the bustle of daily toil. It was my happiness to have a long conversation with him a few months ago. I found he retained a vivid reminiscence of the days of his youth. He was one of Dr. Rush's class, and bears full testimony to the eloquence and earnestness with which Dr. Rush advocated his medical theories more than half a century since, and only a few years before he died.

[1] Public Health; Wm. A. Guy, M.B., F.R.S., London, 1874.

Dr. Bigelow fully sustains me in my position relative to Dr. Rush's views of Nature and of our Art.

Dr. Bigelow's influence upon this second epoch was diametrically opposed to Dr. Rush's, during the first. Dr. Bigelow earnestly supported the principles and methods advocated by Louis and the Numerical School. By his writings and teachings he had great power over New England directly, and indirectly over the medical opinions of the whole country, by means of his pupils, who migrated to our most distant States. The skepticism of Louis's School was augmented thereby.

Whilst Dr. Bigelow was thus sapping our faith in drugs, or rather in the polypharmacy which had come down to us from the past, there was sent forth from the chief medical journal of that day, viz., The British and Foreign Medical Review,[1] and apparently under the sign manual of its able editor, John Forbes, one of the boldest avowals of the same creed. I allude to Dr. Forbes's article entitled "Homœopathy, Allopathy, and Young Physic." Admired by the few, ignored or violently opposed by the many, it has nevertheless stood the test of time, as one of the strongest of protests against the drugging of the day, and an eloquent defence of the power of Nature in the treatment and cure of disease.

Notwitstanding my high estimate of this manifesto by Dr. Forbes, I think that very few young men would be led by it to the study of our most noble profession. It is too destructive of all earnest faith in our art. It was, however, exactly what was needed at the time. The polypharmacy of a past age then and there received its most deadly blow. Nature and her powers were glorified. And no one who reads carefully the record of the preceding time can doubt that such a blow was needed previously to our entering upon a new phase of development of medical science, expressed by the term State Preventive Medicine. Dr. Forbes, indeed, frequently and in glowing terms, hints at the "reformation" that is impending. He is no prophet, although he sees dimly, what is to be "the result of mature reflection, and of the labor of many years and many hands" (p. 262); "we must," he says, "direct redoubled attention to hygiene, public and private, with the view of preventing disease on a large scale." "Here the surest and most glorious triumphs of medical science are achieving and to be achieved" (p. 263).

The third individual, who by his writings and as a teacher, seems one of the noblest types of men of this epoch, is Dr. Elisha Bartlett. He was undoubtedly one of the most philosophical medical men America has produced during the Centenary. And his heart was devoted to medical reform. He was born at Smithfield, R. I., in 1805, and died July, 1855.[2] He practised the profession for some years in Lowell, Massachusetts, and was respected as an able and most accomplished physician. He felt, however, as he said to me, "called to teach." He was soon summoned to the chair of Theory and Practice at Pittsfield. Subsequently he held a professorship at Dartmouth College, New Hampshire; at the schools at Lexington, and at Louisville; subsequently at Baltimore, and finally at New York, in the University and with the College of Physicians and Surgeons.

From these various chairs he was able to influence North, East, South, and West of our country. He was an admirable and attractive teacher, winning belief by his earnestness, his fairness of statement, and fasci-

[1] British and Foreign Med. Rev., vol. xxi. p. 502.
[2] Appleton's Cyclopædia.

nating address. Though not educated under Louis, he thoroughly appreciated his methods and his writings, and proclaimed them as the dawn of a new and great era in the history of medicine. In Dr. Bartlett's Philosophy of Medical Science, published in Philadelphia in 1844, he says "that to find out the best methods of preventing, of modifying," as well as "curing" disease (page 287), "is the great mission which lies directly before us, and this is to constitute the great work of the next and of succeeding generations." He, in these words, appears as one of the prophets of the present epoch, now in its infancy, which delights and encourages us rather by its anticipations than by its actual accomplishments.

Thus with beautiful prophecies for the future, I close the list of men whom I deem most prominent in carrying forward the ideas underlying this second epoch. But no one of them saw what we now see. Their mission was chiefly destructive, or constructive only in some degree of the natural history of disease. The general tendency of their writings, so far as they bore upon medical practice, was to utter skepticism not only in regard to the manifest absurdities of our fathers, but likewise in reference to the good things suggested by them. Although they sometimes, as we have seen, hinted at prevention of disease, they did not dream of the noble idea of State Preventive Medicine in its widest scope which has already commenced in various States of this country, and among the nations of Europe. Their skepticism, like all skepticism, was chiefly iconoclastic. We need faith in an idea before we can build it up in actual life. Such a faith we shall see breaking out in our third and last epoch, and to this I now call your attention.

But before proceeding to speak of it, let me make two remarks, which I regret to feel compelled to submit, but for their truth I can vouch from my own professional experience of nearly half a century:—

First. The medical profession as a body has heretofore taken very little interest in the ideas underlying preventive medicine, and hygiene, private or public. This must be patent to every one of us. We all know how few sanitarians there have been among us until very recently, and how little they have been to our taste. I confess myself to have been a most decided sinner in this matter. It is proved likewise by the almost total neglect hitherto of physical culture in our schools and great seminaries of learning. I know of but one medical college in which there is a full professorship devoted to hygiene. The Harvard Medical School, which has made great advances in medical instruction within the past ten years, has only a lecturer, and as I learn from students now at the school, and who for some few years past have been there, the subject is scarcely known, as compared with other branches of medical learning.

Second. We owe to the laity, rather than to the profession, the first and strongest efforts in behalf of State Preventive Medicine.

Third Epoch; or, that in which the Medical Profession is aided by the Laity, and the idea of State Preventive Medicine fairly inaugurated, as marked by the Legal Establishment of the first State Board of Health.

(From 1869, and which will reach into the far-off future.)

The ruling idea of this epoch is still in its infancy, but it shows by what it has already accomplished, trivial though that performance may

seem, at first glance, to be, its inherent, great, and nascent power. Its objects are vastly wider than those of any preceding epoch. Its destiny is as fixed as that of the steam-engine, the telegraph wire, the locomotive, or the use of anæsthetics. I cannot foresee a time when the ideas underlying it will not be held in esteem and acted upon for the welfare of mankind.

Among the men who stand prominent in their relations to this epoch, and in connection with the English speaking race, are two laymen. One of them I fear may be but little known to most of my hearers, or even to the people of his native State: I allude to Lemuel Shattuck, of Boston. The other is Edwin Chadwick, a Barrister of London, for many years known and honored by every civilized nation as one of the ablest and most earnest of sanitarians.

Mr. Shattuck, in 1850, presented to the legislature of Massachusetts, a most exhaustive State Paper entitled, "Report of the Sanitary Commission of Massachusetts." In that document are laid down all the principal ideas and modes of action which underlie the present sanitary movements in Massachusetts, and I think I may also add in America. By this document were laid deep in the second epoch the fundamental ideas of the third. It was seed well sown, but destined to lie dormant for nearly a quarter of a century.

The report fell flat from the printer's hands. It remained almost unnoticed by the community or by the profession for many years, and its recommendations were ignored. Finally, in 1869, a State Board of Health of laymen and physicians, exactly as Mr. Shattuck recommended, was established by Massachusetts. Dr. Derby, its first secretary, looked to this admirable document as his inspiration and support. In giving this high honor to Mr. Shattuck, I do not wish to forget or to undervalue the many and persistent efforts made by a few physicians, among whom stands pre-eminent Dr. Edward Jarvis, and occasionally by the Massachusetts Medical Society, in urging the State authorities to inaugurate and to sustain the ideas avowed by Mr. Shattuck. But there is no doubt that he, as a layman, quietly working, did more towards bringing Massachusetts to correct views on this subject than all other agencies whatsoever. Of the second layman, Mr. Edwin Chadwick, I need say nothing. You all know him. Fortunately for himself, he has lived to see rich fruits from his labors. That was not granted to Mr. Shattuck.

But other influences have been at work tending to the same end. Europe has influenced us more than at any preceding time. France long since entered on a career of study and of publication on public health. The *Annales d'Hygiène Publique* are proof of this. The labors of Parent-Duchatelet are recognized by all. The zeal of Quetelet, of Belgium, has been like a household word everywhere among the laity and profession in every civilized State. Pettenkofer, of Munich, more recently has labored successfully in the same field; and, still nearer to the present, comes up that extraordinary genius, Virchow, of Berlin, great in every department of science or of State upon which he enters. But by far the greatest influence has been exerted upon us in America by England, who, by her unbounded pecuniary sacrifices and steady improvement in her legislation, and her able writers, has far outstripped any country in the world in the direction of State Preventive Medicine. I think I may, with perfect truth, say that the consummate skill in the discovery, removal, and prevention of whatever may be prejudicial to the public health, shown under the admirable direction of Mr. Simon, late Medical

Officer of England's Privy Council, by his corps of trained inspectors, is wholly unequalled at the present day, and unprecedented, I suspect, in all past time in any country on the globe.

In this country the papers published more recently by several of the State Boards of Health, have tended to interest the whole community on the subject of public hygiene. The number of single laborers in the same field is daily augmenting. The United States Government, by its Army and Navy publications, previous to, during, and since the late Civil War, has done a vast sanitary work. The library and museum in the Surgeon General's Department at Washington, commenced during the horrors of that contest, are also invaluable in the same direction. If they be hereafter thoroughly sustained by the government, as in the past, we cannot estimate the great advantage that will arise in all departments of medicine, and in none more than in that of public hygiene.

As the Crimean war taught the world very much in reference to this subject, so our war, dreadful as it was to both parties, was the means of bringing into life several most beneficent institutions which were never before thought of, or at least never carried out on so grand a scale. I allude to the Sanitary and Christian and Western Sanitary Commissions of the North; to the Wayside Hospitals, Christian Relief Associations, and State Hospitals at Richmond, etc., of the South. Through these, money and food were distributed without stint to the sick and wounded, often to foes as well as friends, wherever found, either on the battle-field or in hospital. We can never know how many human lives were saved, or how much torture prevented, by the agents of these noble institutions on both sides of the line of battle. But we can estimate how much the desire to save life, prevent disease, and promote health, and to provide some of the means for so doing, were, under their blessed influence, instilled into the minds and hearts of our people from Maine to Texas.

Great strides in Public Hygiene were also made during the war, by the fact that the people outside the military lines, and the officers and soldiers within them, very soon began to learn, by dire experience, or by the urgent appeals of experts in Sanitary matters, the all-important advantages of cleanliness, sobriety, and strict methods of action, as opposed to the distress consequent on filth, intemperance, and chaotic rule; and we cannot doubt that every surgeon, whether National or Confederate, who served during the war, became more practically versed in the advantages of prevention of disease than he was before. All that knowledge is now brought back to civil life.

The *National Quarantine Conventions*, so called, had also important influence. Four were held before the breaking out of the war, viz.: in Philadelphia, 1857; in Baltimore, 1858; in New York, 1859; and in Boston, 1860. The war closed their labours. From the eminent persons engaged in them, and attendants at the meetings, and the characters of the papers published, there can be no doubt that they contributed very much to lead the public mind to the ideas prevalent at the present time.

In April 18, 1872, was held the primary meeting whence has sprung the successor of the National Quarantine Conventions, above alluded to. I refer to the *American Public Health Association*, which has held meetings in New York, Philadelphia, and Cincinnati; and will this autumn hold its session at Boston. Able papers have been published by it on various Sanitary matters, on the transmission and prevention of disease, on quarantine, longevity, hospital hygiene, etc.

The *American Social Science Association* has, of late years, done most valuable work in the same direction.

Among the more strictly professional institutions that have helped forward the cause of Public Hygiene and Preventive Medicine, may be named the *American Medical Association.* In 1847, this Association was instituted, and almost from its inception, it has annually published papers relating to our subject, and still more recently very earnest efforts have been made by some of its members to obtain a definite National Health Organization from the United States Government. The frequent intermingling of men, brought about by this Association, from all parts of the Union, has contributed to the same end. It must be confessed, however, that, judging from the small number of members attending the Section on Hygiene, that subject seems to afford less interest than any other.

Finally I may assert that, during the past three or four years there has sprung up in many parts of the country, a wide-spread thoughtfulness about the necessity and value of hygienic measures. Almost all persons in their various spheres, have become more accessible to the protests of the sanitarian. Still further, it may be asserted that during this last epoch, short as it is, more practical work has been done among the people, tending to prevent and crush out disease, and more valuable papers written, illustrative of Public Hygiene, the world over, than since the Christian era began. In every State there are active and earnest laborers in this field, and their number is daily increasing. It would be impossible on this occasion, to name even a tithe of them. Some States in their corporate capacity, have sustained these workers for the public good. But I regret to say a large majority of the States and Territories of this Union are not yet sufficiently enlightened to appreciate the duty devolving on them to be careful of the health of their people.

With these general historical remarks on the gradual evolution of State Preventive Medicine and Public Hygiene, during the past centennial period, I now pass to the consideration of the actual condition of the Country at the present time with regard to these all-important topics.

THE PRESENT CONDITION OF STATE PREVENTIVE MEDICINE IN THE VARIOUS STATES, TERRITORIES, AND THE NATION.

Several months ago I issued a circular containing several questions bearing on this subject, and tending to elucidate it as far as I thought I could do so with the means at my command. Doubtless, to critical eyes, objections may be made to the small number and to the relevancy and character of the proposed questions. It appeared to me, however, that a few interrogations, all or most of them, answered, even monosyllabically, from all quarters of the Country would enable the inquirer to get a glance at the present condition of Public Hygiene and of State Preventive Medicine better than he could from any large number, very many of which would not be answered at all from some of the States. The paper was sent to two hundred and sixty-seven (267) medical men living in the thirty-eight States, nine Territories, and the District of Columbia,

making in all forty-eight governments. These embrace an area covering (25°) twenty-five degrees of latitude and (47°) forty-seven degrees of longitude, between the Atlantic and Pacific Oceans. In the South they lie almost in the tropics; at the North they reach close to the coldest inhabitable portion of the Globe; in all embracing an area of 3,603,884 square miles.

Of the character and ability of my correspondents, I would say that I tried to select the ablest men, and, as far as possible, those known or supposed to be interested in Public Hygiene; representative men, in fact, resident in the various regions. I may have failed to apply to all who perhaps would have willingly answered my questions. If, however, I have selected with any accuracy, I think you will admit that their various reports will together constitute as full and as accurate a statement of the whole matter, so far as my questions cover it, as it is possible for me, without governmental aid, to obtain. Replies have come from one hundred and sixty-seven (167) physicians, residents in every State and territory except the Indian Territory, which last is a reservation held still in an unorganized state as the residence of Indians, in various conditions from barbarism up to a low civilization, and where ideas of Public Hygiene are little thought of.

Analysis of Correspondents' Replies.

First Question.—*Does your State by its legislation show a due appreciation of the duty devolving upon a State to be careful of the health of its people?*

I considered this a fundamental question. The answers from the forty-eight States and Territories were as follows:—

Thirty-four,	(34)	No.[1]
Eight,	(8)	Yes.[1]
Four,	(4)	Indefinite.[2]
Two,	(2)	No Reply.

In other words, of the forty-eight governments in this Union, thirty-four (nearly three-quarters), by their legislative acts, have, according to correspondents resident therein, shown but little care for the health of their people. Among those thus failing in duty, appear the great States of New York and Pennsylvania; I cite these two, not in invidious distinction, but simply because they have been the chief States of our Union since Colonial times, and because they hold that proud position now. Moreover, it will be admitted by those States which have replied affirmatively (1) that it is only within six or eight years that they have shown any marked interest on the question of State Preventive Medicine, and (2) that the number of deaths from preventable diseases, in every State, is still frightfully great, which ought not to be the fact if these States had really shown, for any length of time, this due appreciation claimed for them by my correspondents. Let not us, however, who have traced the very gradual evolution of the idea of State Preventive Medicine during the past centenary, wonder at this apparent neglect of Public Hygiene, shown by the various States; let us rather rejoice at even this small awakening of the public interest upon these matters, wheresoever a European civilization has any foothold.

[1] Yes or No means that a unanimous opinion was returned by all correspondents in the State or Territory.

[2] This term is used when the opinions of correspondents were not unanimous on any question.

SECOND QUESTION.—*Is the State willing to expend money* (a) *to support State or Local Boards of Health?* (b) *to carry out Scientific Investigations as to the causes of Disease?* (c) *to repress noxious or offensive trades?* (d) *to prevent adulteration of food?* (e) *to prevent the Cattle Disease? or* (f) *to carry on any other investigations tending to promote Public Health, or to prevent ill Health?*

The touchstone which tests the earnestness of an individual or of a Nation in reference to any subject, is a willingness to spend money in furtherance of it. Tested by this talisman, how stands our Country in reference to Public Hygiene and State Preventive Medicine? Let us look at the answers to the above questions.

Is the State willing to spend money (a) *to support State or Local Boards of Health?* The following answers were received from correspondents.

Thirty-six,	(36)	No.
Ten,	(10)	Yes.
Two,	(2)	No Reply.

The fact here brought out is that three-quarters of all the governments of this country are unwilling to spend money to support a State Board of Health. Now I contend that the legal establishment of such a Board is the first step towards any definite sanitary organization in a State, and the fact that so many States have done nothing towards the formation of such Boards, or when legally established, have refused to pay money for their ample support, plainly shows the very small advance yet made by this country on these vital questions. Still, further, if we look at the States from which an affirmative reply has been received, we shall find that only very recently, since 1869, have any such Boards been established, and generally that the amount of money appropriated for their support is small, compared with what is spent in other departments of government vastly less important to human welfare.

Is the State willing to spend money (b) *to carry out Scientific Investigations as to the Causes of Disease?* The following are the replies:—

Thirty,	(30)	No.
Twelve,	(12)	Yes.
Five,	(5)	Indefinite Reply.
One,	(1)	No Reply.

Thus thirty governments out of the forty-eight are reported as being unwilling to spend money for the scientific study of the causes of disease. Twelve reply affirmatively. But from several of these we find some qualifications of answer, *e. g.*, "rather unwillingly," or "not much," or "when importuned," or "to a limited extent." Some of the foremost decline to make grants for special investigation. One of the most known and respected Sanitarians in the Union declares that his State "grants as much as is asked for," which fact, coupled with a second one, viz., that his State has not yet established a State Board of Health, seems to support the idea that he and his medical associates are at fault: Does the State even now need an efficient layman to stir it up to efficient action? Only during the past year, has Massachusetts done anything in this precise direction, viz., when it appropriated ten thousand dollars ($10,000) for the investigation of the question of the Pollution of Streams. As I had nothing to do with the making of that report, and as the majority of the committee having it in charge, was selected as the ablest men who could be found, though residents of other States, I feel at liberty to express the opinion that the sum expended was infinitely small in comparison with the very valuable and exhaustive report made;

a report which will be of importance to every other State undertaking the same or similar trains of investigations.

Is the State willing to spend money (c) *to repress noxious or offensive trades?* The following answers were made:—

Twenty-six,	(26)	No.
Fourteen,	(14)	Yes.
Seven,	(7)	Indefinite Reply.
One,	(1)	No Reply.

A priori, one would consider this a most legitimate and imperative duty of a State to so govern noxious and offensive trades that they should not bring ill health to the people, yet it appears that more than half of the States do nothing. Fourteen only seem attempting to do something. In this connection I cannot help referring to some results in Massachusetts.

The town of Brighton is beautifully situated in an undulating country, within easy access of Boston, and as such admirably fitted for residences of the business men of the city. This beautiful suburban district was, only a few years ago, a disgusting place, in consequence of about fifty vile smelling and filthy looking slaughter houses, which were dotted all over the township. No one could drive or go by rail through it without offence, and at times almost nausea. New slaughter houses were daily springing up. Under the influence of a law, empowering the State Board of Health to attend to the matter, and giving it very wide power, every private slaughter house has been swept away within the past eight years. A very large, cleanly conducted abattoir, which burns up in a great measure, if not wholly, its own noxious odors, and which is built a little distance from the village, now includes the whole of the individual slaughter houses above named. One can now drive in any direction without offence to eye or sense of smell. Meanwhile, the price of land has more than doubled in value. Before this was brought about, however, many hearings and personal examinations were made by the Board. Eminent lawyers appeared and protested against its proceeding under the extraordinary powers which the legislature had given it, and which those advocates said were unconstitutional, because, as they contended, contrary to the "Bill of Rights" of Massachusetts.

The Court, however, when appealed to, always sustained the decisions of the Board, and once it threatened a contumacious butcher, who refused to obey, with imprisonment for contempt. For these reasons and for others that might be named, I heartily commend the law, as it stands in Massachusetts, as worthy of consideration by those who wish to control noxious trades in other States.

Is the State willing to spend money (d) *to prevent the adulteration of food?*

Twenty-three,	(23)	No.
Sixteen,	(16)	Yes.
Seven,	(7)	Indefinite Reply.
Two,	(2)	No Reply.

Certainly an extraordinary result, but one which possibly might have been anticipated, when we remember the widespread system of sophistication and adulteration known to exist among us. Rapidity and quantity of work rather than excellence are sought for. When Public Hygiene is duly thought of, and State Preventive Medicine has full sway, the adulteration of the food of the people will be deemed one of the most heinous of crimes. Practically, now it is allowed to flourish unrestricted in the majority of the States, even in those which have laws intended

to prevent it. Europe is undoubtedly far in advance of America on this subject.

Is the State willing to spend money (e) *to prevent the cattle disease?* The following answers were received:—

Twenty-one,	(21)	No.
Ten,	(10)	Yes.
Sixteen,	(16)	Indefinite Reply.
One,	(1)	No Reply.

(For remarks on this subject see Question Sixth.)

Is the State willing to spend money (f) *to carry on any investigations tending to promote public health or to prevent ill-health?* The answers were these:—

Twenty-eight,	(28)	No.
Ten,	(10)	Yes.
Nine,	(9)	Indefinite Reply.
One,	(1)	No Reply.

That is, one-half the governments seem unwilling, and less than a quarter claim to be willing to spend money for this purpose. This seems a most natural consequence to all preceding answers.

THIRD QUESTION.—*Has your State established a State Board of Health? If so,* (a) *when was it established?* (b) *What amount of annual appropriation is made for its support?* (c) *Are any occasional grants made for specific investigations?* (d) *Has the Board any organized body of correspondents throughout the State?* (e) *What executive powers have been given to the Board in reference to local nuisances or noxious trades?*

Some of these questions have been fully or partially answered already. I shall in this place summarily include them in one answer as follows: Twelve only of these States have State Boards of Health. The first one was legally established in Massachusetts in 1869. It operates equally over every portion of the State. The amount of money paid, as I have already stated, is small considering the wealth and population of the State. For special purposes, Louisiana and the District of Columbia have at times spent large sums. Not a single State has as yet a perfect list of correspondents or inspectors. Massachusetts has done much in this direction, and is annually improving her position by selecting the ablest men, physicians or laymen, who can be found in the various towns.

The position of correspondent of this Board is frequently considered an honor, though there is no pay attached to the office, and often it is no sinecure. In reference to executive powers given to the Board, it cannot be said that any State Board has plenary powers, as town Boards have, in some States. It is, perhaps, well that their duties should generally be advisory merely, on certain occasions only having plenary powers conferred upon them, and with the right to appeal to the Courts to sustain their decrees, whereby the moral force of the Boards is materially augmented.

FOURTH QUESTION.—*Have County Boards of Health been established by law?*

The answers are—

Twenty-three,	(23)	No.
Four,	(4)	Yes.
Seven,	(7)	Indefinite Reply.
Four,	(4)	No Reply.

That is, only four of the States report affirmatively. Evidently the necessity of such Boards is not admitted. Yet I cannot but think that eventually, when Public Hygiene shall have its thorough organization, these County Boards will form an integral part of it; certainly in the Southern, Western, and sparsely settled States.

FIFTH QUESTION.—*Have any Town Boards of Health been established by law?*

The answers are—

Eight,	(8)	No.
Fourteen,	(14)	Yes.
Twenty-two,	(22)	Have them by local or municipal rather than state law.
One,	(1)	Indefinite Reply.
Three,	(3)	No Reply.

Which replies, as I understand them, intimate that in one-half of the governments many towns have established, by their inherent right of self-defence, local Boards of Health without first appealing to State Authority.

SIXTH QUESTION.—*Has the State passed any law leading to a thorough and definite improvement of the Public Health?* (a) *by a Sanitary Survey of the State?* (b) *by a law for the registration of Births, Deaths, and Marriages?* (c) *If so, how long has it been in operation?* (d) *Has the Registrar been able to draw from such records any law governing the Public Health?* (e) *Has any law been passed relating to drainage of land?* (f) *to irrigation?* (g) *to checking the influence of rivers by levees?* (h) *to the introduction of water into cities?* (i) *to the prevention of contagious diseases?* (α) *Smallpox?* (β) *Cholera?* (γ) *Yellow Fever?* (δ) *Cattle Plague? etc. etc.*

It is impossible on this occasion to consider all of these questions. I shall make selections from the answers which will best illustrate my main subject.

(a) *By a Sanitary Survey of the State?* The returns are as follows:—

Forty,	(40)	No.
None,	(0)	Yes.
One,	(1)	Indefinite Reply.
Seven,	(7)	No Reply.

One would suppose that this would be one of the first questions asked by the settlers of a new country, and yet, judging from our returns, not one of the States in this Union has ever made such a survey, save perhaps indirectly in connection with some matter of trade or a geological or other examination for the purposes of gain. Doubtless very grave mistakes have been made in consequence of this neglect. Considerations of trade and personal safety from external danger have too often decided the question of the site of a town. It is to be feared, moreover, that with the keenest examination of a locality, the emigrant, upon a new and virgin soil, would be unable to decide the question. For often, not until months after the plough has passed through it, and the colony has become firmly established, does the deadly malaria show itself. But when a State has arrived at sufficient strength and power to be able so to do, it would seem that a thorough Sanitary Survey should be considered the first of duties. And yet I doubt if any State Legislature of this Union, at the present time, would for a moment consider the question. It would be deemed a *useless expense!* The people will demand it ere long.

(b) *By a law for the registration of Births, Deaths, and Marriages?*

Sixteen,	(16)	No.
Twenty,	(20)	Yes.
Eight,	(8)	Indefinite Reply.
Four,	(4)	No Reply.

That is, one-third definitely declare that this fundamental law, upon which all vital statistics are founded, has never been passed. Twenty, less than half, have such a law. But eighteen out of the twenty use various qualifying phrases in regard to the character or enforcement of the law, *e. g.*, "imperfect," "not carried out," "defective," "no regular registration," "carried out only for certain great cities." In one it is "a dead letter," and in another "not enforced;" others have it "a farce," or that "it was enforced before the late war." Some have "only marriages," some "only deaths."

The sum total of the matter is that only two States have made affirmative replies without special qualifications. Knowing what I do of one of these, viz., Massachusetts, I cannot claim that to be perfect in many respects. If, turning from the States, we look at the United States censuses, we find the same short-comings. For further remarks on this topic I refer to Question Eighteenth; remarks by Messrs. Walker and Elliott.

No one can regret more than I do the necessity of stating these facts. May the sting of their severe truth, stimulate us and our children to better work in the coming centuries!

(c) *If so* (*that is, if a registration law has been enacted*), *how long has it been in operation?* After the facts stated in regard to the last query, the further question of time seems unimportant. It is evident, however, that for the last ten or twenty years only, have State Registrations been carried on in a very few States, although some of the larger cities have had imperfect returns for a much longer period. The United States Government has had decennial censuses since 1790. To judge of their value in certain respects, we have simply to look at the remarks made upon them by the superintendent of the last. (*Vide* Question Eighteenth.)

(d) *Has the Registrar been able to draw from such records any law governing the Public Health?* I shall make my remarks in connection with this subject, under the last question of the entire series.

(e) *Has any law been passed relating to the drainage of lands?*

Twenty-four,	(24)	No.
Seven,	(7)	Yes.
Six,	(6)	Indefinite Reply.
Eleven,	(11)	No Reply.

That is, one-half report that nothing has been done by the State. Only one-seventh report affirmatively. But I do not understand that in any case such a drainage was made for a sanitary object, but rather to improve the land for agricultural purposes. How much the Sanitary, Physical, Moral, or Intellectual wealth of a people is wrapped up in simple drainage of the land, we may judge from the glowing account given by my correspondent, Dr. Breed, at Princeton, Illinois. By that it appears that no less than 36,000 acres of land lying in one mass, and comprehending at least half of three counties in Illinois, were drained by the State, with the most gratifying results upon the health and character of the settlers thereupon. Moreover, a great sum accrued to the Common School Fund of the State, from the enhanced value of the land for all purposes. One cannot but recommend the example of Illinois to the consideration of the Legislatures of many of her sister States.

(f) *Has any law been passed relating to irrigation of land?* The answers are:—

Thirty-one,	(31)	No.
Five,	(5)	Yes.
Five,	(5)	Indefinite Reply.
Seven,	(7)	No Reply.

All the affirmative replies except one, viz., Utah, report that it was done for "Agricultural Purposes." Apparently no one thought of Health in connection with it. Even Utah probably was induced to irrigate more by its actual wants than by any thought of Sanitary improvement. Near Salt Lake, which itself is utterly impotable, pure water was of prime necessity for two imperative reasons, viz., for culinary purposes, and for agriculture. What the Moors did for Granada, the Mormons have, to a certain extent, done for our American Desert, by their "thousands of miles of ditch under a general law," as my correspondent graphically, if not scientifically, describes the immense work done by this people.

(g) *Has any law been passed relative to checking of rivers by Levees, etc?* The answers are:—

Twenty-eight,	(28)	No.
Four,	(4)	Yes.
Five,	(5)	Indefinite Reply.
Eleven,	(11)	No Reply.

In proposing the question, I had my mind chiefly on the great cities situated upon our mighty Mississippi, and its various tributaries. Eventually, it would apply to other rivers, like the Yellowstone and others. In various parts of the land, I regret that my correspondents enable me to state but little in reference to sanitary measures connected with the question. It is evident that New Orleans, *e.g.*, depends for its very existence upon them. Correspondents from Arkansas state that some levees have been made for agricultural purposes, and one adds significantly, "but it has improved the land and the health of the people living near the place."[1]

(h) *Has any law been passed for introducing water into cities?* The answers are as follows:—

Twenty-three,	(23)	No.
Fourteen,	(14)	Yes.
Four,	(4)	Indefinite Reply.
Seven,	(7)	No Reply.

Indiana and Pennsylvania have general laws upon the subject. Probably all the States have permitted, either by general or special statutes, this introduction of water, because of its prime necessity. More particularly within the last ten years have towns and cities waked up to the propriety of introducing fresh water from distant sources, in order to avoid the evil results of the possible contamination of wells from privies, etc., in the immediate neighborhood.

But it is a singular fact that while thus bringing in *new* water, cities

[1] In the very able and interesting report of the United States Bureau of Topographical Engineers (Philadelphia, 1861), prepared by Capt. A. A. Humphreys and Lieut. H. S. Abbott, U. S. A., on the Physics and Hydraulics of the Mississippi River, etc., it is suggested that, eventually, there should be a uniform and scientifically constructed system of levees for that river, from the Ohio to below New Orleans (p. 417, etc.). Surely such a plan would be worthy of the nation, and, under the idea of its value to commerce between the several States and foreign nations, would it not be legitimate national work under our Constitution?

have not often scrutinized its exact purity, nor have they sufficiently provided against its future impurities. Boston, Albany, and I fear Chicago and Philadelphia, and I doubt not many other cities in this Union, might be cited in proof of the truth of this proposition. Some of these cities have wholly ignored the warnings of sanitarians in this respect, listening to selfishness and political partisanship, rather than to the dictates of Hygienic Law. As they have sown the wind, so, at some future time, they will doubtless reap the whirlwind, if an epidemic like Cholera should sweep over the land.

(i) *Has any law been passed relative to the prevention of contagious diseases?* (α) *Smallpox.* The answers are:—

Sixteen,	(16)	No.
Twenty-one,	(21)	Yes.
Seven,	(7)	Indefinite Reply.
Four,	(4)	No Reply.

There must be some error here, for I cannot think that sixteen of the States have, as yet, passed no law relative to smallpox. And yet from the way in which the laws on this subject are neglected, or limited of operation even in those States which claim that they have proper laws; and from the fact that we have smallpox epidemics all over the country constantly prevailing, which could easily be prevented by vaccination; from all these facts, I feel sure that great ignorance and carelessness on the subject are to be found everywhere in this country. The recent events in Montreal, relative to vaccination, in which a mob of infuriated and ignorant men attacked and over-awed the Common Council of the city, show what might happen in these States.

A late city physician of Boston, used to declare that he could keep Boston free from smallpox if he could prevent the citizens from meeting immigrants from Maine, who flocked, most of them unvaccinated adults, into the city limits. This assertion, though not perhaps strictly true, I have no doubt is partially so, and it illustrates an important fact, viz.: that it is indeed impossible for one State to free itself from this scourge, until the United States shall, by a general law, require that every child born shall be vaccinated at a very early period, and afterwards occasionally revaccinated. Yet more, we shall never be free from it until every foreign nation shall adopt the same method, and an international code of health in this respect shall have been established. Though such a code may be in the far future, it must eventually come, not only in reference to smallpox, but in regard to every other pestilential scourge of our race.

(β) *Has any law been made relative to the prevention of Cholera?* The answers are:—

Twenty-one,	(21)	No.
Sixteen,	(16)	Yes.
Four,	(4)	Indefinite Reply.
Seven,	(7)	No Reply.

Entirely similar reports came in regard to this disease as in regard to smallpox; all indicate a "*laissez aller*" method in regard to it, so that if the cholera were to strike any State at the present time, there is probably not a town in the land which would not be liable to fatally suffer from its ravages in consequence of "filth"[1] within the town limits.

[1] Used in its broadest signification. Simon on "Filth Diseases and their Prevention." *Vide* Report of Medical Officer of the Privy Council and Local Government Board (Eng.), New Series, No. 2. Reprinted in Boston, Campbell, 1876, under the direction of the Massachusetts State Board of Health.

But the United States Government, through the War Department, has recently published an invaluable history of cholera in this country. By this volume alone we can judge of the immense importance to the whole country, a central power, such as I have suggested above, would speedily become. It would grasp the whole country in one mass, rather than as separate States, and immensely broader, almost cosmic views of disease would be thereby given instead of those derived from detached local observations. In order to gain this desirable end we need a National Health Council composed of representatives from each State, and a Secretary of Health, the peer of other Secretaries in the National Cabinet.

For further elucidation of my views on this subject, I beg leave to refer to my address delivered before the American Medical Association at its session in Louisville, in 1875.

(γ) *Has any law been passed relative to the prevention of Yellow Fever?* The answers are:—

Twenty,	(20)	No.
Twelve,	(12)	Yes.
Ten,	(10)	Indefinite Reply.
Six,	(6)	No Reply.

I suspect the above data and other replies of my correspondents represent in a general way the present uncertainty in the country about the prevalence and characteristics of this disease. There seem, however, to be some facts that are quite patent. Yellow fever is said (1) by some to exist as a permanent endemic disease in some of our southern cities (New Orleans, *e. g.*, according to Chaillé); "with waves of increase and decrease down almost to no prevalence at all." Some equally eminent (Herrick), declare that it cannot be called endemic in New Orleans, and that it might be extirpated but for importation. From both of these parties we may infer that it virtually has a permanent habitation there. But for an accurate and minute knowledge of the essential cause of the disease and means for its prevention we must look to the coming century.

It seems (2) equally plain that during the last centennial period, yellow fever has virtually wholly disappeared from Boston, New York, and Philadelphia, in which latter city it prevailed to a terrible degree in the latter part of the last and in the earlier part of this century.

May we not hope that as in the past, so in the future, there may be a gradual pushing back of this frightful disease until it shall be fairly excluded from our borders!

(δ) *Has any law been passed relative to the prevention of the Cattle Disease?* The answers are:—

Twenty,	(20)	No.
Eleven,	(11)	Yes.
Five,	(5)	Indefinite Reply.
Twelve,	(12)	No Reply.

As at one time pleuropneumonia among cattle threatened very seriously to interfere with the food of the people, I deemed this a proper question. It is evident from my correspondence that it has influenced very differently the different States. In those States where it occurred, as *e. g.* in Massachusetts and in some of the Western States, much money was expended, large meetings were held, and many cattle were slaughtered in order to check it. I may refer on this subject to my correspondent of Illinois (Dr. Breed) for an interesting notice of what was done at a Convention of Cattle Dealers of the Northern States and Canada; to the Report of the Metropolitan Board of Health in New York, and also to

the action of the Massachusetts Legislature. By strict quarantine and care in sanitary matters connected with the cattle, and absolute destruction of those earliest seized with the disease, and above all by a national co-operative action rather than by that of any single State, I believe we may keep that and all such pests at bay. But eternal vigilance will be needed on all the great ways for cattle traffic, so that the disease shall not get a foothold in the country, otherwise disaster may follow. In connection with this subject I would refer to the report made by the United States Commissioner of Agriculture on the Diseases of Cattle, containing investigations by Prof. Gamgee, and by Drs. Billings and Woodward of the United States Army.

(j) *Has any law been passed regulating Tenement Houses for the poor?* The answers are:—

Thirty-three,	(33)	No.
Four,	(4)	Yes.
Three,	(3)	Indefinite Reply.
Eight,	(8)	No Reply.

That is, on this most important measure, on which so much depend the health and morality, and intellectual progress of a people, only four, ($\frac{1}{16}$) one-sixteenth of the various governments, have, according to my correspondents, done anything, while ($\frac{15}{16}$) fifteen-sixteenths either reply negatively or indefinitely, or not at all.

In the light of what is doing in England by public law, and in New York and Philadelphia by less comprehensive, but most opportune private work, on the basis of pecuniary gain, combined it may be with philanthropy, this report seems meagre. In this connection, I beg leave to refer to interesting correspondence with Drs. Harris and Thayer of New York, and Dr. Ford and Mr. Blodget of Pennsylvania.

(k) *Has any law been passed relative to incorporating Building Companies for the improvement of the dwellings of the poor?* The answers are:—

Twenty-nine,	(29)	No.
Six,	(6)	Yes.
Four,	(4)	Indefinite Reply.
Ten,	(10)	No Reply.

This question, allied to but more comprehensive than the preceding, has, as we see, received as little attention. Yet, strictly speaking, I know of no two questions more important at the present hour than this and the previous one. The fact is patent to every one that there are in every city and township in this land, and in every other country, dwelling places in which the poor are *obliged* to live, which are a disgrace to modern civilization. Such abodes are moral pests in the community, and sources of unmitigated evil to the Body Politic. They are, in fact, places in which it is impossible for the majority of human beings to grow up, except to filth, crime, and disease. These I fear will always exist, unless the public authority takes charge of these tenements and either builds others under State laws, or authorizes associations, partly for pecuniary and partly for philanthropic and sanitary reasons, to erect them. Having erected them it will be necessary to faithfully supervise them at all times and seasons, otherwise filth, and consequently disease, will come as heretofore.

We have one house in Boston yclept in derision the "Crystal Palace," which is occupied, when full, by about three hundred people, and which has been for years the abode of drunkenness and crime. It was well known to the officers of the State's Prison as its chief primary school of Crime. I know it well. It is impossible to tell at this time the rich

though painful experience gained from it by the Boston Co-operative Building Company. This, as its name partially indicates, was incorporated by the State in order to enable philanthropy and capital to unite on an equitable basis, viz., in newly building or in renovating the houses of the poor, with the hope of receiving an adequate return for the capital expended. This corporation has been a success in building many houses, united in a block, and giving a perfect home to every family, only one, or at the most two, families being on one floor. But desirous of trying the plans followed so successfully, by Mrs. Octavia Hill, of London, in her renovation of some of the most wretched houses in that Metropolis, the Boston Association took possession of this "Crystal Palace." We forthwith drove tenants from the filthy cellars and opened windows into every bed-room, the large majority of them having previously had none. We shut *two* of the *dark passages leading as common corridors to all the tenements* in the building, but we could not shut up a *third*. At first our success seemed very certain, but the commercial panic came in 1873, and that drove away many, and paralyzed the labors of others of the tenantry. Finally the lease was given up for this reason chiefly, that we were losing money. But even if it had been a pecuniary success, it became the decided opinion of most of the Committee, who had charge of it, that morally it must, from the inherent vices of its construction, *remain vile.*

Whilst so many are congregated in one house, with long dark common corridors actually inviting vice and crime of the lowest kinds, it is impossible to check the tendency thereto. The committee saw young girls and boys grow up into vice (without a possibility of saving them), owing, as it appeared in a great measure, to the very structure of the tenement, and of its out-buildings.

Whilst taking this opposition to the idea of large buildings, with dark, easily accessible, common corridors, instead of rows of moderate-sized buildings, containing but few families, it was gratifying to the committee to find that the death-rate in the building was much lessened by the alterations made in it, and by constant vigilance in regard to sanitary rules, as far as it was possible to have any rules, in a place provided with a few common privies and slop receptacles, which were constantly, day and night, resorted to by people having small ideas even of the comfort of cleanliness, and totally ignorant of its immense value in a hygienic point of view.

I have cited this case simply because of my four and half years of personal experience upon it. But the same facts of wretchedly constructed abodes for the poor exist everywhere, and human nature is the same. Our experiment, I think, proves very conclusively the folly of expecting people to be temperate, or honest, or healthy in badly constructed houses. I commend this topic of providing *suitable homes*, not houses only, for the people, as one of the most important that can command the attention of Philanthropy and Capital during the coming Century.

SEVENTH QUESTION.—(a) *Are there well-attested facts proving that any disease formerly prevalent in your State has ceased to appear?*

The answers are:—

Twenty-five,	(25)	No.
Nine,	(9)	Yes.
Eight,	(8)	Indefinite Reply.
Six,	(6)	No Reply.

Of the nine affirmative replies not one can present positive proof of the statement, but some suggest a lessening of some diseases. This seems to be undoubtedly the fact in regard to malarial fevers, yellow fever, etc.

That intermittents have disappeared from some localities, which have been drained, seems to be true. Smallpox is wonderfully lessened and quite shorn of its terrors. A few special diseases, as, for example, the "milk sickness," and the "black tongue," are mentioned as having prevailed formerly, but not of late. These statements, though less reliable than accurate statistic proofs, give us hopes for the future when State Preventive Medicine shall have obtained its due influence.

(b) *Are there any well-attested facts proving that any disease formerly prevalent in the State has been crushed by State or Individual action?* The answers are:—

Thirty-seven,	(37)	No.
None,	(0)	Yes.
Four,	(4)	Indefinite Reply.
Seven,	(7)	No Reply.

The returns give a most decided negative, and not a correspondent takes the affirmative of the question.

Eighth Question.—*Are there any similar facts proving that any special disease has arisen or been generated, or has been introduced into the State during the past century, which did not exist in Colonial Times, and which now remains endemic?*

The answers are:—

Twenty-seven,	(27)	No.
Six,	(6)	Yes.
Two,	(2)	Indefinite Reply.
Thirteen,	(13)	No Reply.

The same indefiniteness of statement prevails on this question as on the preceding. In one of the six affirmative replies, Dr. Denison, of Denver, claims that perhaps phthisis has been introduced into Colorado. Of course it has been, as a great many go there now as a place most fitted for the cure for the disease. And future statistical mortuary data, unless care be taken, will lead to great errors of judgment in regard to the value of that climate, and its influence on phthisis. In some of the Northern States some physicians claim that cerebro-spinal meningitis, diphtheria, and even croup, have been introduced during the centenary. There are grave doubts about the truth of all such statements. Others claim that yellow fever and cholera are such diseases; "yellow chills" are named by one, "trichiniasis" by another. I feel that there is no proof that any new disease has been introduced, and become endemic here.

Ninth Question.—*If there be any such new disease, has it been investigated by the State or by individuals?*

The answers are:—

Twenty-seven,	(27)	No.
Six,	(6)	Yes.
Two,	(2)	Indefinite Reply.
Thirteen,	(13)	No Reply.

My correspondents cite investigations by a commission of the city of New Orleans in 1853, relative to typhoid fever; by Dr. Kedzie, of

Michigan, on cerebro-spinal meningitis; on diphtheria, by the N. Y. Metropolitan Board of Health; on "yellow chills" by various persons in Alabama.

Of course this meagre statement falls far short of the truth in regard to the number of investigations undertaken by individuals, corporations, and commissioners, on the various diseases of the country, some of which have no more right to be classified as new diseases, than typhoid fever and diphtheria; and none of them can be certainly said to be diseases never seen before the past century.

TENTH QUESTION.—*Has the town or city in which you reside taken any measures for the improvement of the Public Health?*

(a) *By health laws?*

The answers are:—

Nine,	(9)	No.
Twenty-five,	(25)	Yes.
Nine,	(9)	Indefinite Reply.
Five,	(5)	No Reply.

From the above, it seems that the majority of the towns have done what mere self-defence would inculcate. But of the twenty-five affirmative replies very many report either inefficient enforcement or complete abeyance of such laws. Many towns have special laws for drainage, and other objects seem partially or wholly ignored. For my own State of Massachusetts, I think I can say that in not a single town has there ever been a complete system of health laws enacted and thoroughly enforced. But, notwithstanding this unsatisfactory statement, I also believe that I may with equal truth assert that so recently has the idea of State Preventive Medicine begun to take hold of our people, that our advance has been quite as much as could be expected. Very many more years will be needed before this country or Europe will even approximate to that point which, at present, only an ideal sanitary legislation would require, but which the coming century will bring forth into actual operation.

(b) *By special action in specific cases?*

Three,	(3)	No.
Twenty-five,	(25)	Yes.
Eleven,	(11)	Indefinite Reply.
Nine,	(9)	No Reply.

The remarks made on the previous question apply to this.

ELEVENTH QUESTION.—*Does your town use well-water for culinary uses?*

TWELFTH QUESTION.—*Is care taken to prevent pollution?*

THIRTEENTH QUESTION.—*Do you have a water supply from distant lake or river?*

FOURTEENTH QUESTION.—*Is care taken to prevent pollution?*

Including all these questions under one category; of 143 towns 82 use wells, 61 use rivers or lakes. In regard to the care taken to keep either class of waters pure, I find as follows:—

49 or 34.26 per cent. try to keep it pure;
69 or 48.25 per cent. make no attempt to do so;
25 or 17.48 per cent. make no, or an indefinite reply.

In other words, taking my returns as furnishing at least an approximation to the general state of the country in this respect, 34.26 per cent. only of the towns make any claims, even the most trivial, to be careful of the purity of their water; while 65.73 per cent., or nearly twice as

many, are totally neglectful. That is to say, nearly two-thirds of the people of this Union are living in a senseless disregard as to whether they are drinking pure water or water which, though "sparkling and tasting pleasant," may be loaded with every species of filth!

FIFTEENTH QUESTION.—*Have you sewers to carry off such water supply?* The answers are:—

Eleven,	(11)	No.
Thirteen,	(13)	Yes.
Twenty,	(20)	Indefinite Reply.
Four,	(4)	No Reply.

These replies coincide with all the preceding, and intimate very fully the primitive ideas held by the people of this country in regard to the necessity of carrying off water once made impure. What happens in Boston I fear happens elsewhere, viz.: that abundant supplies of water have been introduced into certain parts of the city without any immediate provision being made for its removal by sewers. The consequence has been that the cesspools have been overflowed, and that the land has been soaked with water, perchance containing the elements of disease, and certainly tending to develop phthisis, from the soil moisture induced by such overflow. Of course, evil must result.

I might refer to other particular cities and places, now the common summer resorts for the most wealthy people, where a most scandalous condition exists in this respect, making it at times more dangerous to visit the "health resort," so called, in country and sea-side, than it would be to remain at home, even in a poorly-sewered, offensive-smelling city. But we have no need to visit health resorts to find great imperfections of sewerage. That of Boston, which I have already quoted in reference to its suburbs, has been, even within its city limits, notoriously bad, notwithstanding it has been claimed to be nearly as perfect as could be. Some of the main sewers of that city are even now nearly filled with refuse, more or less solid. One of the main conduits is occupied to a great depth by so-called "mud," composed of dirt from the open street gutters and of human excreta. Others are more or less partially obstructed. The whole congeries of them form one vast network of cesspools, belching out at times filthy odors from "manholes" along the streets. The mouths of the sewers, which open all around the city limits, and in the closest proximity to it, pour forth volumes of similar foul exhalations while the tide is low, and the quantity of refuse issuing from them has been declared, under oath, to interfere very much in certain places with trade, by preventing vessels which were formerly able to approach certain wharves, from so doing at the present time.[1] I summon thus significantly my own city to the bar of public sanitary rebuke, not because I think her really a worse sinner in these respects than many other and some greater cities, but for two reasons: (1) that I know whereof I speak, and can vouch for the truth of the assertions, extraordinary as they may seem to some of you, and (2) that I rejoice to say that, in this centennial year, Boston is seemingly repentant, and is prepared to amend her system at a large expense. Thank God! Her citizens are beginning to feel that the *worst prodigality is the waste of human life*, and that *mere money is as nothing before the idea of perfect human health!*

[1] *Vide* Tyler et al. *v.* Squire et al., before the State Board of Health of Massachusetts, 1873.

SIXTEENTH QUESTION.—*How far are the sewer outlets from the source of water supply?*

Upon this question it may be generally stated that these distances in different cities of the Union vary from what may be practically called zero to 20 or even 30 miles. It would seem that, where it is convenient so to do, a distance more or less great is always chosen; but strange as it may seem, some of our great cities have been wholly thoughtless. If my correspondent be correct, even in this great city of Philadelphia in which we meet, although the majority of the sewerage is carried far below the water supply, we cannot claim the same for all of it. And not a few cities which take water from rivers and lakes are practically regardless of the quantity of impurities that enter the stream or lake at some distance, it may be, but nevertheless *above* the part whence the city gets its own water supply. I might cite Albany as a great and most glaring example of this latter statement.

SEVENTEENTH QUESTION.—*What is your method of disposing of sewerage? House offal? Slops or filth liable to accumulate about homesteads?*

I have received answers from eighty-four towns, and the summary of them is, that only in about one-fifth (sixteen) are there any proper and efficient means for the removal of these various sources of contamination. A great many have still most primitive ways of disposing of them; "they are thrown away carelessly." Many have "surface drainage." In some they are "thrown into open lots," or "to the hogs;" and in some "each inhabitant does as his own will dictates." Nothing can be more chaotic than the present method pursued in this country, considered as one great whole.

EIGHTEENTH QUESTION.—*Have any State, County, or City reports of Health or Deaths been published?*

The answers are:—

Thirty,	(30)	No.
Nine,	(9)	Yes.
Two,	(2)	Indefinite Reply.
Seven,	(7)	No Reply.

That is, 62.50 per cent., or more than half of the correspondents, say there is no registration in their States; 18.75 per cent. have had some registration, most of them only for a few years past; and 18.75 per cent. make indefinite or negative replies. Adding together the first and last items as being essentially the same, we have, as the final probable result, that 81.25 per cent., or more than three-fourths of these States, have virtually ignored vital statistics during the whole centennial period. Add to this that among those which claim to have had registration, it is likewise stated by my correspondents that in not a few the registration is very imperfect.

The United States commenced its national censuses in 1790, and has continued them decennially ever since, but never with the accuracy which is desirable. I judge this from the opinion of experts whose opinion I have asked of even the latest, that of 1870. The following remarks by General Walker and Mr. Elliot seem also to indicate that my inference is true.

General Walker says: "The gross incompleteness of the returns of deaths in the census of the United States is shown by Mr. Elliott's Approximate Life Table. What, it may be asked, is the value of statistics

confessedly so imperfect?"[1] He elsewhere admits that the number of deaths falls short 40 per cent. of what must actually be the fact.

Mr. Elliot, who calculates life tables from these returns, admits that he is obliged to "resort to the somewhat arbitrary assumption of 40 per cent. limited, however, by an investigation of the rates of mortality relative to population which obtain in other communities, so far as accessible, and in portions of our own country."[2]

NINETEENTH QUESTION.—*How many years, approximately or definitely, have these registration reports been published?*

I regret that, owing to imperfect returns, I cannot give any but general statements.

But when I think of the comparatively brief period of time since the registration has been commenced in the different States where any is made, and knowing as I do, the imperfections of those which may be supposed to be the best, I feel sure I am within the limits of truth, when declaring that not a State in this Union has a registration which has been in progress a sufficient number of years, or which has been conducted with sufficient accuracy, to be fairly comparable with that registration which will be eventually demanded by an enlightened State Preventive Medicine, after that idea has been firmly grasped and acted upon for some years by the intellect and conscience of our people.

In confirmation of this assertion, I state the following fact. I was desirous of learning definitely, not from theorizing on the influence of civilization, not from the records of foreign countries, but from accessible statistics collected and printed in this country, whether man lives longer now than at the commencement of the centennial period. I consulted several eminent American experts in statistical data. I asked them to point out to me any such data. Not one of them could do so. One who stands pre-eminent, and whose word is listened to by Europe, advised me as follows: "You may consult bills of mortality; grave-yard records; family Bibles; traditions of the former and later times, and they will show whether we have gained or lost in longevity." I have no doubt that, provided we could find those records, we should perhaps partially gain the end proposed. My question was whether he could point out to me any printed document, derived from accurate records made in the United States, answering my question. He could not do so.

TWENTIETH QUESTION.—*Has any law of development, or of partial development, of any disease been discovered by individual or State action, by attention to which in coming centuries we may hope to greatly lessen or destroy such disease?*

I proposed this question of fact as the test question, so to speak, of the Centennial Period. It was the culmination of all these questions hitherto proposed.

The answers are:—

Thirty-nine,	(39)	No.
Six,	(6)	Yes.
None,	(0)	Indefinite Reply.
Three,	(3)	No Reply.

The returns indicate that my correspondents have a more decided opinion upon this question than upon any other of the series. There are

[1] Statistical Atlas of the United States. By F. A. Walker.
[2] Ibid. Approximate Life Table for the United States. By E. B. Elliott.

only three who fail to make any answer, and not a single State has replied indefinitely. Again, thirty-nine out of forty-eight States, or 81.25 per cent., reply in the negative; six of the forty-eight, or 12.50 per cent., reply affirmatively.

But let us look carefully at the affirmative replies, and interpret more closely their exact meaning. The replies I think may be justly arranged in two categories:—

First. Those in which it appears that by close attention to sanitary arrangements, and their rigid enforcement, certain diseases are checked. One correspondent remarks, "all the acquired knowledge concerning all and each of the typho-contagia, and the factors of their propagation is equivalent to discovery." (Dr. Harris, of New York.) From other States we are informed that "the laws of development of typhoid fever are known better." (Drs. Plummer, of Oregon, and Butler, of Vermont.) Drs. Stuart and Toner, of Washington, D. C., say they are "able to control and prevent zymotics by sanitary enforcement." Dr. Baker, of Lansing, the self-sacrificing, efficient Secretary of the State Board of Health of Michigan, writes as follows: "It seems proper to modify very much the 'Yes,' which I have placed opposite the question; if we substitute for 'law of development,' or 'partial development of,' the words 'general truth respecting,' I think the answer will be truthfully 'Yes.'" He then cites scarlet fever "which will be lessened in the near future, because of the accumulated evidence of the great mortality caused by it." He also thinks "croup and pneumonia will be eventually greatly lessened from the fact that a cold and dry atmosphere causes the greatest mortality from both. And by securing such favorable conditions in our houses, and particularly in our sleeping rooms, it is reasonable he thinks, to hope that many deaths from pneumonia and croup may in the future be prevented." These may be considered as one category.

Second. Let us grant that all this is true, nevertheless I do not see that any "law" of development of disease, similar in degree at least, if in character, to that of "soil moisture" as a very prominent cause of phthisis, has been presented by any of the above respected correspondents. Massachusetts, through the agency of the Massachusetts Medical Society,[1] proved many years ago by data received from her three hundred and twenty-five towns, and where consumption had been previously considered everywhere equally endemic, that there were dry spots where it was very rare, and wet where it was very rife. By accurate statistical data, laboriously gathered, it was further proved that of two families growing up, one on a wet soil and the other on a dry soil, the one resident on the wet by that fact became twice, if not three times, as liable to phthisis as the other, resident on a dry spot. That law was first discovered and announced in New England, and subsequently by ample statistical data proved to exist in Old England. I think it may now be said to be a Cosmic Law, and as such it seems to me I am justified in placing it alone in the second category under this final question.

In order to meet the numerous similar questions which will inevitably arise in the future, we need, as Mr. Elliot suggests, a National System for the registration of all the Births, Marriages, and Deaths taking place in the country. Moreover, the Registrar should, for the whole country, learn the exact character of the localities in which the deaths take place, if we would know exactly and broadly the endemic influences of any place.

[1] Topographical Distribution of Consumption. Annual Address before Mass. Med. Society, 1867.

SUMMARY.

Gentlemen: My work is done. I have endeavored to place before you the exact truth in these matters so far as I could obtain it. No one can feel more keenly than I do that it has at times been most unpalatable, and that it seems to be but little creditable to the country. Nevertheless, looking at the very recent growth of a regard for Public Hygiene, and at the certain though small advances made in various parts of the country in State Preventive Medicine, I have the strongest hopes for the future of our Country and the world in this regard. We could not, with the influences hitherto pressing upon us, have advanced further. System making, the outgrowth of many previous centuries, prevented all progress in this direction during our first epoch. The overthrow of these theories and theory makers, and the bringing of medical inquiries into line with the methods pursued by modern science, was the great fact gained during the second. Although this has been followed by a sweeping and widespread skepticism in our own art, we must remember that this same skepticism is found everywhere within the domains of modern thought. Good will ultimately result from it. But these facts have not allowed of the growth of Preventive Medicine until within a very recent period of time.

We stand now at the very dawn of the grandest epoch yet seen in the history of medicine. While philosophically, accurately, and with the most minute skill, studying by means of physiology, pathological anatomy, chemistry, the microscope, and, above all, by careful clinical observation, the natural history of disease and the effects of remedies, our art, at the present time, looks still higher, viz.: to the *prevention of* as well as to the *cure of* disease. And this is to be done by sanitary organizations throughout each State and the Nation, the laity and the profession heartily joining hands in this most noble cause.

Our Present Duty.

Our present duty is organization, National, State, Municipal, and Village. From the highest place in the National Council down to the smallest village Board of Health, we need organization. With these organizations we can study and often prevent disease.

This great and beneficent object of the prevention of disease appeals to all. The aged may give counsel. To the young of this and of future generations belongs the solid work which is to bless the coming centuries. I appeal therefore with all the earnestness at my command to the young men of the present hour. Can there be anything more inspiring to a generous hearted, intelligent youth than the thought that, by laborious research into the causes of disease, by the discovery of means for its prevention, and by the teaching of these various causes and means to the people, he may help to save even a few of the more than two hundred thousand human beings now annually slaughtered in this country by preventable disease?

Our Hopes for the Future.

I trust that this discussion will have given rise in your minds to some of the bright hopes for the future which have arisen in my own. Although Public Hygiene has made but few advances hitherto, it is nevertheless founded upon Natural law. It has been legitimately and healthfully growing during a portion of the past quarter of a century. Modern Science greets it and brings it within its domain as one of its most precious objects for thorough investigation. I hope therefore that, standing as we now are near to the close of this fair Centennial Birth-year of our Nation, up to whose festival all nations of the earth have been invited, I may be allowed to appeal to all, whether young or old, American or collaborating friends from other lands, to join with me in a *Cordial All Hail to Coming Centuries*, not only in America but in every civilized spot of God's earth; because everywhere, and to all coming time, human life will be lengthened, made more healthy, and consequently more truly happy, by the potent influence of

State Preventive Medicine.

ADDRESS ON MEDICAL CHEMISTRY AND TOXICOLOGY.

BY

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When it is remembered that nearly all human thought and labor are devoted to the promotion of the welfare of the human body, and that agriculture, manufactures, and commerce have this as their chief end, it will not be deemed strange that a science so comprehensive and withal so flexible in its applications as Chemistry, should be laid under contribution to preserve that body in health, or to restore that health when impaired. Before chemistry had become a true science, its aid was invoked by the old alchemists in their search for the "Elixir of Life." A universal remedy was the dream of many a toiler in secret places, whose occult science had for him a strange fascination. There were among the Arabians many who, in their rude way, applied such chemical knowledge as they possessed to the discovery of new medicines. The same was true of the earlier physicians of Europe. There was everywhere an effort to make nature reveal some secret which might be turned to account in the healing art. This, more than any other, was deemed the true function of Chemistry.

It was not until Bacon announced the true method of scientific investigation, and declared that the Kingdom of Science, like the Kingdom of Heaven, was to be entered as a child, rather than with the spirit of a dictator, that the mists of earlier times were gradually swept away, and the path prepared for the appearance of Science with exact methods and with exact results.

It is now only about a century since Chemistry took shape as a Science. The labors of Black, Priestley, Scheele, Lavoisier, and others showed that a vast field of research lay before them. For a century chemists have been exploring this field, and have taken possession of many a spot, and secured permanent title-deeds for posterity; but, it must be confessed, that a century is far too short for the survey of the whole domain, and our descendants will have ample scope and incentive for the profoundest labor and research. Only two years since, the chemists of Great Britain and America celebrated the centennial of Chemistry, dating the birth of the science from the discovery of Oxygen by the distinguished Priestley, August 1, 1774. The commemoration in this country took place at Northumberland, Pa., the American home of Dr. Priestley. The general progress of the century was there ably set forth by men of note in their several departments of investigation. The very valuable address of Prof. Benjamin Silliman, on this occasion, on *American Contributions to Chemistry* (a complete compendium to which we are greatly indebted) shows that in this country we have not been idle in advancing this department of science.

After the discovery of oxygen, nitrous oxide, and other gases, by

Priestley and others, Sir Humphry Davy, then a young man with majority unreached, was employed at Bristol, through the generosity of Messrs. Watt and Wedgwood, to undertake a careful series of experiments with gases as remedial agents. So important and intimate is the connection between chemistry and medicine, that we should naturally look to those who feel the imperative need of this science in its sanitary applications, for the best labors in the science. There could be no other incentive more potent. The air and vapors we breathe, the food we eat, the fluids we drink, the medicines we take, and the poisons we seek to avoid, all come within the range of the analyst, and his words of counsel and caution are to be heeded. In this field of labor many eminent chemists have labored with exclusive devotion, while others, as Davy, Scheele, and Liebig, commenced their distinguished careers as pharmacists, and may be said to have emerged from the apothecary's shop. Indeed, we may not overlook the close connection between chemistry and pharmacy. As we trace the history of medical chemistry in the United States, it will be seen that the contributions of the pharmacists have been so many and various, that the historian must give them a high and honorable place in the annals of American progress.

One hundred years ago we find that the only medical college in the land, with an occupied chair of chemistry, was the Philadelphia Medical College, soon after (1791) merged into the University of Pennsylvania, the institution in whose halls we are now assembled, an institution not only venerable in years, but renowned for the great and noble work it has accomplished. The occupant of this chair, elected August 1, 1769, was none other than Dr. Benjamin Rush, distinguished alike for his professional labors, which brought honor upon medical science, and for his devoted patriotism in the dark days when our national independence was won. He graduated at Princeton, and afterwards studied medicine in Philadelphia, Edinburgh, London, and Paris. While in Edinburgh he studied chemistry with the eminent Dr. Joseph Black. He was one of the signers of the Declaration of Independence, which this centennial year commemorates. In 1777 he was made surgeon-general of the army in the middle department. Ever a distinguished physician and medical professor, ever active in all public and philanthropic movements, his long life was one of singular usefulness and honor, and no name in our annals should be mentioned with more reverence and respect than that of Benjamin Rush, the earliest American professor of chemistry. The only other chair of chemistry in the colonial days was that established in William and Mary College, Virginia, in 1774, and filled by the Rev. James Madison.

In 1779 a professorship of chemistry and natural philosophy was established in the literary department of the University of Pennsylvania. Thus two of the first three chemical professorships in America were established in Philadelphia. In the medical department of Harvard University, Aaron Dexter was elected Professor of Chemistry and Materia Medica in 1783; and in 1792 Dr. Samuel Latham Mitchell was elected Professor of Chemistry in Columbia College, New York. Dr. Mitchell graduated in medicine in Edinburgh in 1786. Two years after entering upon his professorship of Chemistry, he published an essay on the *Nomenclature of the New Chemistry*. In 1797 he, with others, established the *Medical Repository*, the first scientific periodical ever published in the United States. He is now widely known as an early student of

natural history, and his publications on zoological subjects are yet prized.

In 1795 Professor John McLean, a young chemist of Scotland, was elected Professor of Chemistry in Nassau Hall, Princeton. In 1803 Professor Benjamin Silliman, the first professor of chemistry in Yale College, entered upon his duties. He became widely known as an able and eloquent expounder of chemical science. In 1818 he established *The American Journal of Science and Arts*, which became, more than any American journal, the means of disseminating discoveries in chemistry and in cognate sciences. His name will ever remain an honored one in the history of American science.

The work done in chemistry in the medical schools has not always been marked by scientific thoroughness; indeed, in too many cases, very little practical benefit has resulted from the lectures, often only heard to be forgotten. But a change is now being effected in this respect, and we note with satisfaction that a two years' course of systematic laboratory practice is made part of the regular curriculum of study in the Medical Department of Harvard University.

The earlier professors of chemistry in the Medical Schools did not contribute largely to the advancement of the science in the way of original research. The science was in its infancy, and the circumstances surrounding them were not generally such as to awaken that measure of enthusiasm which is necessary to scientific discoveries. A marked exception, however, is found in the case of Dr. Robert Hare, who became Professor of Chemistry in the University of Pennsylvania in 1818. In his youth he was a member of the Chemical Society of Philadelphia, to which belonged Priestley, Seybert, and others. At the early age of twenty, he announced the discovery of the oxy-hydrogen blowpipe, which at once gave him distinction in both hemispheres. This became a valuable instrument in certain departments of chemical investigations, and in the arts. Dr. Hare had great mechanical skill, and with his ingenious apparatus was enabled to perform experiments with great brilliancy and success. In addition to many valuable contributions, he published, in 1828, a *Compendium of Chemistry*, a valuable and useful work at the time. In this connection it should be mentioned that in 1819 Dr. John Gorham, Professor of Chemistry in the Medical Department of Harvard University, published the first original systematic treatise on chemistry ever issued in this country, entitled the *Elements of Chemical Science*, a work in two volumes, which reflected great credit upon its author. It should be stated, however, that there had been, before this, American republications of foreign chemical works.

We now come, in the order of history, to the name of Prof. John Redman Coxe, who deserves mention for the part he played in inaugurating a movement, which, as we shall see, has contributed, perhaps, more than any other to the progress of Medical Chemistry in the United States. A part of his medical education was obtained in the University of Edinburgh, but he graduated in the University of Pennsylvania in 1794. He afterwards spent considerable time in London, Edinburgh, and Paris. In 1809, he was elected by his Alma Mater Professor of Chemistry, and was transferred to the chair of Materia Medica in 1818. While in this latter chair he was the "leading spirit" in a movement to provide a special course of lectures in Pharmacy for the young druggists and druggists' clerks of Philadelphia. This was early in 1821. This movement arrested the attention of the leading druggists of the city, and led

them at once to establish a new and independent institution—first called the College of Apothecaries, but changed the next year to the Philadelphia College of Pharmacy—to secure the same results, but upon a more liberal plan.

The founders of this college were wise men, and secured for their new institution professors of high scientific character. The first course of lectures was given in 1821–2. Dr. Samuel Jackson, a medical graduate of the University of Pennsylvania, was the first Professor of Materia Medica. In 1827 he was elected to a professorship in his Alma Mater. His career as a lecturer, as an author, and as a practitioner, was in all respects distinguished. Dr. Gerard Troost, afterwards eminent in other departments of science, was the first Professor of Chemistry, but was succeeded within a year by Dr. George B. Wood, happily still living to enjoy the rewards of a life well spent in the service of science and of humanity. His published works, at once so complete and scholarly, have honored the profession of which he has so long been a distinguished member.

In 1831, Dr. Wood was transferred to the department of Materia Medica. His successor in the chair of Chemistry was Dr. Franklin Bache, a worthy descendant of the eminent statesman and philosopher whose name he bore. In 1833, Professors Wood and Bache published the first edition of the *United States Dispensatory*, a work which has passed through thirteen (13) editions, and is still the indispensable handbook of the physician and pharmacist. These eminent professors impressed upon the College of Pharmacy a character for thorough science which it has never lost. Our brief sketch would be imperfect should we here fail to mention the name of the late Prof. William Procter, Jr., who became the Professor of Pharmacy in this college in 1846, and who by his scientific labors, prosecuted through many years, contributed more to the special department of Medical and Pharmaceutical Chemistry than, perhaps, any other American. It should here be mentioned that, four years after the establishment of the Philadelphia College of Pharmacy, its friends founded the *American Journal of Pharmacy*, a journal still issued, and which has contributed very greatly to the dissemination of a knowledge of chemistry in its relations to medicine.

The second College of Pharmacy in this country was established in New York in 1829, through the instrumentality of George D. Coggeshall, a graduate of the Philadelphia College. The first Professor of Chemistry was the late Dr. John Torrey, whose labors in science, and especially in American botany, have given him a name which will ever be honored.

MEDICAL CHEMISTRY.

Having thus glanced at the history of the introduction of chemistry into our schools, in passing on to discuss the theme assigned us, MEDICAL CHEMISTRY, we find ourselves entering upon a broad domain of investigation. We might enlarge upon the many and wide bearings of chemistry upon medical practice, and show how indispensable it is to the best success of the practitioner; point out its grand achievements in the departments of physiology and pathology, and show how inseparable is its connection with the progress of medical science in all its departments;

but, perhaps, as more consonant with the occasion and with the limitations of the hour, we may restrict ourselves to a brief consideration of the more prominent of the *American Chemical Contributions to the Medical Progress of the Century.* In this review, we find that these contributions relate largely to the examination of the chemical and therapeutic properties of our indigenous plants.

One of the first of these plants examined was *Sanguinaria Canadensis*, or Blood-root. Attention was first called to this plant in 1803, by Dr. Wm. Downey, of Maryland, as a powerful emetic; and in 1819, Dr. Wm. Tully, of New Haven, investigated with care its therapeutic properties. In 1828, Dr. J. F. Dana, of Dartmouth, first chemically examined the plant, and succeeded in isolating a basic or alkaloidal principle which he called *Sanguinarine* (N.Y. Lyc. Nat. Hist., ii. 245, 1828). The identity in ultimate composition of this alkaloid with the alkaloid of *Chelidonium majus*, subsequently discovered by Probst and called *chelerythine*, was fully established by Dr. Joseph Schiel, of St. Louis, in 1865 (Amer. Journ. Sci. (2), xx. 220). A second principle was extracted from Sanguinaria Canadensis by Riegel, which he considered identical with Merck's porphyroxine, from opium; but this identity was subsequently denied by Dr. G. D. Gibb. A third proximate principle derived from this plant was described by Prof. E. S. Wayne, of Cincinnati (Amer. Journ. Pharm., 1856, 521); and for it Dr. Gibb proposed the name *puccine.* Prof. Wayne has also recognized in blood-root the presence of *chelidonic acid*, first found in the plant Chelidonium majus.

In 1819, Dr. John Torrey, while investigating a curious underground fungus called Tuckahoe or Indian-bread (*Scleroticum gigantium*), discovered a new principle which he termed *Sclerotine*, an account of which was read before the Lyceum of Natural History, New York, Nov. 15, 1819, and published in the N. Y. Med. Repository, Dec. 1820. In 1824 Braconnot described pectic acid, a principle now obtained from many succulent plants. In 1827 Dr. Torrey republished his former paper with additions, and showed the identity of the two substances (Amer. Journ. Sci. (2), xxvii. 439).

Another interesting native plant that early received attention is *Lobelia inflata*, known as Indian tobacco, and used as a medicine by the aborigines. The attention of the medical profession was first called to this plant by Rev. Manasseh Cutler, of Ipswich, Mass., eminent as a botanist in the latter part of the last century. Lobelia was first examined chemically in 1833 by Dr. S. Colhoun, Professor of Materia Medica in Jefferson Medical College, Philadelphia, who announced the existence of a basic principle capable of forming salts with acids, but failed to isolate the principle (Amer. Journ. Pharm., v. 300, 1833). Wm. Procter, Jr., who has already been referred to, in his Inaugural Thesis, in 1837, on Lobelia inflata, demonstrated that the active principle of the plant is a liquid alkaloid, which he succeeded in isolating. He named this principle *lobalina*, and he described the salt formed by it with the leading acids. His very elaborate thesis was published in the American Journal of Pharmacy for July, 1837. In November, 1850, Wm. Bastick read a paper before the Pharmaceutical Society of Great Britain on Lobelia inflata, in which he described the same alkaloid, and his name is often mentioned in foreign publications as its first discoverer. A number of cases of poisoning have occurred in this country from the use of this plant, chiefly as administered by empirics.

The next indigenous plant to be noticed is *Veratrum viride*, or Ameri-

can hellebore. This plant was known to the Indians chiefly as a poison. According to Joselin, an early visitor to this country, it was used by them in the selection of their chiefs, the individual found least susceptible to its effects being regarded as the "strongest of the party, and entitled to command the rest" (Amer. Journ. Pharm., vii. 203). It was brought to the attention of the medical profession about the year 1830, by Drs. Tully and Ives, of New Haven. Its therapeutic properties were more fully investigated by Dr. Charles Osgood, of Providence, in 1835 (Amer. Journ. Med. Sci., xvi. 296). In 1851 Dr. W. C. Norwood, of South Carolina, again brought it before the profession in a series of papers highly extolling its medicinal virtues.

The chemical history of this plant presents some features of peculiar interest. Very contradictory conclusions have been reached by different investigators in regard to the exact chemical nature of its active principle or principles. The first chemical examination of this plant was made in 1835 by Dr. Charles Osgood, but he failed to obtain satisfactory results (Amer. Journ. Pharm., vii. 202). It was again examined by Dr. Thos. R. Mitchell in 1837, who also failed to determine the true nature of its active principle (Ibid., ix. 181). After a somewhat extended examination, Henry W. Worthington, in 1838, concluded that the plant contained an alkaloid identical with *veratria*, previously obtained from veratrum album, its European congener (Ibid., x. 89). In 1857, Dr. Jos. G. Richardson, after a very elaborate investigation and an extended series of parallel chemical experiments, with the alkaloids of both these plants, came to the same conclusion (Ibid., 1857, 204). In 1862 Mr. G. J. Scattergood announced that in addition to the alkaloid *veratria*, the plant contained a resin, to which he ascribed the sedative action of the drug, and he also obtained some evidence of the probable existence of the alkaloid *jervia* in the plant (Proc. Amer. Pharm. Assoc., 1862). Mr. Charles Bullock, of Philadelphia, in 1865, claimed that the alkaloid in question was not identical with *veratria*, and that the resin of Scattergood owed its activity to the presence of another alkaloid. These principles were afterwards named respectively *veratroida* and *viridia* (Amer. Journ. Pharm., 1865, 321). In 1872 Dr. Eugene Peugnet, of New York, also assented to the want of identity between veratroida and veratria, and he announced the identity of the viridia of Bullock with the alkaloid jervia, first discovered in veratrum album by Simon (Med. Record, May, 1872). These views of Dr. Peugnet were endorsed by Mr. C. L. Mitchell, of Philadelphia, after an elaborate investigation in 1874 (Proc. Amer. Pharm. Assoc., 1874, 436). Lastly, in this singular history, in a paper in the January number of the American Journal of Pharmacy for the current year, it is again claimed that the alkaloid, other than jervia, of this plant *is* identical with veratria, it fully responding to all the known tests for this alkaloid. The paper just cited, by Dr. T. G. Wormley, also points out the behavior of jervia with reagents, and cites cases in which both alkaloids were recovered from the blood of animals poisoned with veratrum viride.

In 1875 Mr. Charles Bullock contributed a valuable paper on the methods of preparation and properties of jervia (Amer. Journ. Pharm., 1875, 449). In this connection we should not fail to mention the interesting and valuable series of experiments upon the physiological action of the alkaloids of this plant by Prof. Horatio C. Wood, Jr., of Philadelphia, published in the American Journal of the Medical Sciences, January, 1870, and the Philadelphia Medical Times, vols. ii. and iii.

Leaving this protracted notice of the American hellebore, let us turn to a plant of milder name and properties, the Wax Myrtle or Bayberry, the *Myrica cerifera* of the botanists. In 1819 the berries of this plant were examined chemically by Dr. J. F. Dana, then chemical assistant in Harvard University (Amer. Journ. Sci., i. 294). Mr. G. E. Moore, in 1862, made a complete analysis of the Wax of the Bayberry, and found it to consist of *palmitin* and *palmitic acid*, with a small quantity of *lauric acid* (Ibid. (2), xxxiii. 313). In 1863 the bark of the root of this plant was examined by Mr. Geo. M. Hambright, of Philadelphia, who found that it contained a new crystallizable principle, to which he gave the name *myricinic* acid (Amer. Journ. Pharm., 1863, 193). The bark of this plant, as a remedial agent, was first brought to the attention of the profession in 1822, by Dr. W. M. Fahnestock, of Harrisburg, Penn.

According to Rev. F. Heckewelder, the Indians of this country employed *Podophyllum peltatum*, or May apple, as a poison to destroy themselves (Eberle's Therapeutics, i. 205). This plant was long used as a popular cathartic, and was brought to the attention of physicians, by Dr. Bigelow, about 1818.

The first chemical examination of this plant was made in 1831, by Wm. Hodgson, who obtained a crystallizable bitter principle, which he believed to be the active principle of the plant, and which he named *podophyllin* (Amer. Journ. Pharm., iii. 273); but Prof. T. F. Mayer was led to believe that the principle obtained by Hodgson was the alkaloid *berberina* (Ibid., 1863, 98); and this fact was fully established by Prof. J. M. Maisch (Ibid., 303). After an elaborate examination of the plant by John R. Lewis, the results of which he presented as an Inaugural Essay, he concludes that the active properties of the root are due to the presence of two peculiar resinous principles, the one soluble, the other insoluble, in ether (Ibid., 1847, 165). According to Prof. Mayer, the root contains, besides berberina, a colorless alkaloid, a neutral volatilizable principle, and saponine (Ibid., 1863, 98).

A native plant that has received considerable attention at the hands of the physician and chemist is *Hydrastis Canadensis*, or Yellow puccoon. It was known both as a medicine and as a dye to the aborigines of this country, and employed by irregular practitioners before being used by the regular profession. The first chemical examination of this plant was made in 1851, by Alfred A. B. Durand, of Philadelphia (Am. Journ. Phar., 1851, 112). He obtained a colorless crystallizable base, which he named *hydrastia*, but he failed to obtain it in a pure state. In 1862, Mr. J. D. Perrins, of Worcester, England, obtained it pure and determined some of its properties. He read his paper before the London Pharmaceutical Society, April 2, 1862, an abstract appearing in the Chemical News (v. 204), and the paper in full in the London Pharmaceutical Journal for May, 1862. In 1863, F. Mahla, of Chicago, Illinois, in a very valuable paper published in Silliman's Journal (op. cit. (2), xxxvi. 57) very fully describes the properties of hydrastia and gives its ultimate composition. We have given the history of this alkaloid somewhat in detail, since there have been different claimants for the honor of its discovery.

Mr. F. Mahla, in 1861, fully established the presence of *berberina* in the hydrastis Canadensis (Sill. Journ. (2), xxxiii. 43). This was the first instance in which this alkaloid—now known to be so widely distributed in plants—was found in a plant belonging to the Ranunculaceæ, and it thus established the fact that the same alkaloid may occur in plants belonging to different families. The chemical history of the alkaloid

berberina is somewhat remarkable. In 1826, Chevallier and Pelletan discovered it in the bark of *Xanthoxylum clava Herculis*, and named it *xanthopicrite.* In 1835, it was found as *berberina*, by Buchner, in *Berberis vulgaris.* These observers erroneously regarded it as a weak acid. Fleitman established its basic character; and Perrins, in 1862, showed the identity of xànthopicrite with berberina, and established its formula. The probable existence of a third alkaloid in hydrastis canadensis, "more resembling berberina than hydrastia, but decidedly different from the former," was announced in 1873, by A. K. Hall, of Ann Arbor, Mich. (Am. Journ. Phar., 1873, 247). This third principle was more fully examined in 1875, by John C. Burt, of the University of Michigan (Ibid., 1875, 481).

Cimicifuga racemosa, or Black snakeroot, a native of the United States, was first introduced to the attention of the regular profession by Dr. Thos. J. Garden, of Charlotte, Va., in 1823. This plant was examined chemically in 1834, by John H. Tilghman (Amer. Journ. Phar., vi. 14); in 1843, by J. S. Jones (Ibid., xv. 1); and in 1861, by Geo. H. Davis (Ibid., 1861, 391); but neither of these gentlemen succeeded in obtaining the active principle of the plant. At a meeting of the N. Y. Academy of Medicine, April 3, 1871, Dr. S. R. Percy exhibited a sample of a new alkaloid obtained by him from the cimicifuga racemosa, which he named *cimicifugia* (Amer. Med. Times, N. Y., April, 1861).

The poisonous nature of the leaves of *Prunus Virginiana*, or Wild cherry, has long beeń known. As early as 1806, Dr. Coxe, of Philadelphia, conjectured that the poisonous properties of water distilled from the leaves of this plant were due to the presence of prussic acid (see Am. Journ. Pharm., 1859, 433). In 1834, Stephen Procter determined the proximate composition of the bark of the tree, and established the presence of hydrocyanic acid and a volatile oil, as products of distillation with water (Amer. Journ. Pharm., vi. 8). In 1838, Prof. Wm. Procter, Jr., established the existence of *amygdaline* and of a decomposing agent, identical with *emulsine*, in the bark, leaves, and kernels of the wild cherry, and showed that these principles (as had before been shown by Liebig and Woehler in the case of bitter almonds) were the source of the volatile oil and hydrocyanic acid obtained by distillation (Ibid., x. 197).

In 1856, Prof. E. S. Wayne, of Cincinnati, obtained from the root of *Leptandra Virginica*, or Black root, a peculiar, bitter, crystallizable principle, which possessed the properties of the plant, and which has been named *Leptandria* (Amer. Journ. Pharm., 1856, 510). Prof. Wayne also found that the root contained mannite (Ibid., 1859, 557). In 1863, Prof. F. F. Mayer announced the presence in the plant, of a glucoside closely resembling senegin in its properties, but more readily decomposed by alkalies (Ibid., 1863, 298).

We may next notice *Rhus toxicodendron*, or Poison oak, a native plant well known from its peculiar action upon the skin of some individuals. It was used by the Indians, both as a poison and as a medicinal agent. The first chemical examination of this plant seems to have been made in 1857, by Dr. Joseph Khittel, of Munich, who determined its principal proximate constituents, among which he claimed to have obtained a volatile alkaloid upon which the poisonous properties of the plant depended; but of this he failed to give any proof. In 1865, Prof. John M. Maisch, of Philadelphia, obtained from the fresh leaves of the plant, a new volatile, organic acid, which he named *Toxicodendric acid*, and fully

established that it was to the presence of this principle the plant owed its peculiar poisonous effects. Prof. Maish failed to obtain any evidence of the presence of a volatile alkaloid (Proc. Amer. Pharm. Assoc., 1865, 166). Several cases of poisoning of children who have eaten the fruit of this plant, have been reported.

The beautiful climbing plant *Gelsemium sempervirens*, or Yellow jasmine, a native of the South, was brought to the notice of the profession by an accident, in which a decoction of the root was administered for that of another plant. Its claims as a therapeutic agent were strongly urged upon the profession by Prof. Wm. Procter, in 1852 (Amer. Journ. Pharm., 1852, 307). This plant is now not only used quite generally by the profession of this country, but has found favor with the profession of Europe, having been first employed there more especially as a remedy for neuralgia. Its physiological effects were very carefully studied by Prof. Bartholow, of Cincinnati, in 1870 (London Practitioner, Oct. 1870); and very recently by Drs. Ringer and Murrell, of University College, England (Lancet, March and April, 1876). The first attempt to determine the chemical composition of the root of this plant was made in 1854, by M. H. Kollock (Amer. Journ. Pharm., 1855, 197). It was again examined in 1868, by C. L. Eberle (Ibid., 1869, 35). Both these gentlemen, however, failed to isolate the active principle. In 1869, Dr. T. G. Wormley obtained from the plant a new alkaloid, *gelseminia*, and an organic acid, *gelseminic acid*, the properties of both of which he fully described in the American Journal of Pharmacy for January, 1870. This paper also contains a report of a case of poisoning by the fluid extract of the plant, in which the alkaloid was recovered some months after death. On physiological grounds, Drs. Ringer and Murrell are inclined to the view that the plant contains two active principles, one capable of paralyzing, the other of exciting, the spinal cord.

An indigenous plant, used more especially as a domestic remedy, that early received chemical attention was the Prickly ash, *Xanthoxylum fraxineum*, which was examined in 1829, by Dr. Edward Staples, who found that it owed its active properties to the presence of a peculiar crystallizable principle which he named *xanthoxyline*.(Amer. Journ. Pharm., i. 163). After the discovery of berberina and its identity with xanthopicrite had been shown, it was generally believed that Dr. Staples's xanthoxyline was identical with berberina, especially when the wide distribution of this alkaloid became known; but Mr. Perrins, who first pointed out the identity of the two former alkaloids, states that xanthoxyline is an entirely distinct principle (Amer. Journ. Pharm., 1863, 459, note).

In connection with this early contribution of Dr. Staples may be mentioned that of Dr. J. F. Dana, who in 1819 discovered the existence of the animal alkaloid *cantharidine* in the Potato fly, *Lytta vittata*, the alkaloid having been first obtained in 1812 by Robiquet from the Spanish fly, *L. vesicatoria*. Other American contributions to the chemistry of cantharidine have been made by Prof. Procter, in 1852; by Wm. B. Warner, of Maryland, in 1856; and by Prof. Maisch, in 1872.

A stately American tree, named from its large and beautiful flower, the Tulip tree or *Liriodendron tulipifera*, affords a bark which was recommended to the profession by Dr. Young, of Philadelphia, in 1792, as a substitute for Peruvian bark in intermittent fevers, and in diseases requiring a gentle stimulant and tonic. In 1831, Prof. Emmet, of the University of Virginia, obtained from this bark a neutral, bitter principle

which he named *liriodendrin*, and which he described as a white crystallizable solid (Amer. Journ. Pharm., iii. 5).

Apocynum cannabinum, or Indian hemp, long known as a powerful emetic and cathartic, was examined chemically by Dr. Knopp, who found it to contain, among other proximate constituents, a peculiar, bitter, active principle to which he gave the name *apocynin*. This plant was subsequently examined by Dr. Griscom (Amer. Journ. Med. Sci., May, 1836). This plant is different from *Cannabis indica*, also popularly known as Indian hemp, which was examined chemically in 1865, by Prof. Wm. Procter (Amer. Journ. Pharm., 1865, 23).

The American water hemlock, *Cicuta maculata*, found abundantly throughout the United States, was examined in 1855, by Jos. E. Young, who found it to contain a peculiar acid (probably the *coniic* acid of Perschier), and the volatile alkaloid *conine*, first obtained from conium maculatum, by Geiger, in 1831.

Wahoo, *Euonymus atropurpureus*, so well known throughout the Western States, and sometimes called "burning bush," from the brilliancy of its crimson fruit, was made the subject of a very elaborate chemical examination by Wm. F. Wenzell, in 1862, who found it to contain a peculiar basic principle, which he named *Euonymine*, and a new organic acid, *euonic acid* (Amer. Journ. Pharm., xxxiv. 387).

Many other American plants have contributed to the wants of the physician, and have been the subjects of chemical examination, but special reference to them is necessarily omitted.

We now come to consider the history of the discovery of a compound which is not only interesting in itself, but is more especially so on account of its subsequent applications and the important position it now holds in the medical world.

In 1831, Dr. Samuel Guthrie, of Sackett's Harbor, N. Y., a man of unusual chemical knowledge for his time, discovered a substance which he prepared by distilling a mixture of chloride of lime and alcohol, and an account of which he published in the American Journal of Sciences, Oct. 1831, under the title "A new mode of preparing a spirituous solution of chloric ether." In this paper Dr. Guthrie states that in order to ascertain the effect of this substance in full doses on the healthy subject, and thus discover its probable value as a medicine, he had, during the last six months, administered it to a great number of persons, not only very freely, but frequently to the point of intoxication, and that the effects were grateful both to the palate and the stomach, and he concluded that "it would seem to promise much as a remedy in cases requiring a safe, quick, energetic, and palatable stimulant." His attention was, as he states, first directed to this subject by a statement in Prof. Silliman's Elements of Chemistry (vol. ii. p. 20), then just published, to the effect that an alcoholic solution of chloric ether, when diluted, was a grateful diffusible stimulant and might probably be introduced in medicine.

Prof. Silliman distributed samples of this substance to Dr. Eli Ives, of the Medical Department of Yale College, and to others, to be used in their practice. Under date of January 2, 1832, Dr. Ives reported that he had administered it in a number of cases with marked advantage, and had also used it by inhalation with good results (Amer. Journ. Sci., xxi. 406). The product thus produced by Dr. Guthrie was regarded by him at the time as the substance then known as "chloric ether" or "Dutch liquid," whereas it was an entirely new and distinct substanee, that now

known as *chloroform*. Chloroform was also discovered by Soubeiran, of France, and an account of it published in February, 1831, under the name of *Ether bichlorique*. So, also, Liebig, in 1832, made another independent discovery of the same substance, naming it *Chloride of carbon*. It was named chloroform by Dumas, who discovered its true chemical constitution in 1834. A committee of the Medico-Chirurgical Society of Edinburgh awarded Dr. Guthrie the credit of having first published an account of its therapeutical effects as a diffusible stimulant in 1832 (Amer. Journ. Sci. (2) vii. 143).

As this substance afterwards became a prominent agent in producing anæsthesia, it leads us naturally, but very briefly, to notice the use of this and other substances inhaled for anæsthetic purposes, and the part which Americans have taken in a discovery which has proved an inestimable blessing to mankind.

In this memorial year, we naturally follow the years back to the beginning of the century we commemorate, and find that in 1776, or just one hundred years ago, Dr. Priestley, who had previously made the brilliant discovery of oxygen, discovered *Nitrous Oxide*, which he called *Dephlogisticated Nitrous Air*. In 1779, Sir Humphry Davy, then a very young man, entered upon a series of experiments in Bristol with this nitrous air, which he named nitrous oxide, and on the 11th of April of that year, made the first inhalation of the gas, and recorded its effects upon himself. By his investigations he ascertained its exhilarating effect, and discovered its power, when inhaled, to remove intense physical pain. His remarkable generalization from his experiments was expressed in the following oft-quoted words: "As nitrous oxide in its extensive application appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place." (Researches, etc., concerning Nitrous Oxide and its Respiration, Bristol, June, 1800.) While this suggestion appears to have been forgotten, yet nitrous oxide became widely known and used as a stimulant, and its administration as "laughing gas" was a matter of popular exhibition. In like manner, ether, the date of the first inhalation of which is in some obscurity, was used for the same purpose in the United States, as mentioned by the earlier writers on chemistry. In the experiments with both nitrous oxide and ether, there were cases in which these agents produced perfect insensibility, but the anæsthetic condition was not specially observed.

In the fall of 1844, Horace Wells, a dentist of Hartford, having witnessed an exhibition of the inhalation of nitrous oxide, and observed that a person under the influence of the gas was injured without being conscious of suffering, boldly inhaled the gas, and had a tooth removed without sense of pain, and gave it to others for the same purpose, with similar results. Thus the suggestion of Sir Humphry Davy in Bristol, England, published in 1800, was realized by Horace Wells at Hartford, Conn., in 1844.

In 1846, Wm. T. G. Morton, a dentist of Boston, a former student of Wells, and aware of his labors with nitrous oxide, commenced experiments with ether for the same end. The administration, in the Massachusetts Hospital, of ether by inhalation to a patient upon whom the eminent Dr. John C. Warren performed a surgical operation, authenticated it as an anæsthetic agent to the surgical and medical profession, and from that time the knowledge and practice of anæsthesia extended rapidly over the world.

As already shown, chloroform was employed by inhalation by Dr. Ives, of New Haven, in his practice in the winter of 1831–32 with marked benefit. In March, 1847, M. Flourens experimented with this agent upon lower animals, and in November of the same year the distinguished Prof. James Y. Simpson, of the University of Edinburgh, employed it as an anæsthetic agent with success, and introduced it into surgical and obstetrical practice.

Thus, in summary, we find that anæsthesia, first suggested by Sir Humphry Davy, in 1800, was demonstrated with nitrous oxide as an agent, by Horace Wells, in 1844; with ether as an agent, by Wm. T. G. Morton, in 1846; and with chloroform, by Sir James Y. Simpson, in 1847.

The next American chemical contribution to medicine that we may mention, is that on the "Origin of Urea in the Body," by Prof. John C. Draper, of the University of the City of New York (New York Journal of Medicine, Feb. 1856). In this paper the author shows the error in the statement of Liebig that muscular action increases the amount of urea secreted. The process employed was the decomposition of urea by nitroso-nitric acid, and weighing the carbonic acid produced as carbonate of baryta. The results obtained by Prof. Draper have since been verified by Fick and Wislicenus, but the credit of the first disproval of the statement of Liebig belongs to Dr. Draper.

Dr. Draper has also contributed papers, which may be mentioned in this connection, on "Products of Respiration," New York Med. Times, July, 1856; "Insensible Perspiration," Proceedings of the New York Academy of Medicine, May, 1864; "The Heat produced in the Body, and the Effects of Exposure to Cold," American Journal Science and Arts, Dec. 1872.

The last contribution we shall mention in this connection, is a valuable paper in the department of Physiological Chemistry, entitled "Experimental Researches into a new Excretory Function of the Liver," by Professor Austin Flint, Jr., widely known in the field of experimental physiology. This paper was published in the American Journal of the Medical Sciences for October, 1862; and afterwards presented to the French Academy of Sciences, from which it received in 1869 honorable mention and an award of 1500 francs. The important discoveries put forth in this memoir were the production of cholesterine in the physiological wear of the brain and nervous tissue, and its elimination by the liver, and discharge from the body in the form of stercorine. Dr. Flint's researches and interesting conclusions upon this subject have lately been confirmed in Germany, by experiments in which cholesteræmia has been produced in animals by injection of cholesterine into the blood.

TOXICOLOGY.

We now enter upon the other and not less important branch of the subject assigned us, namely that of TOXICOLOGY. This science is one of the highest moment to the race. While health and life are assailed by various diseases, by the ill-adjustment of outward circumstances, such as climate, locality and the like, by the unsuitableness of food and drink, or by an excessive use of these, and by a thousand other harmful conditions, these

assaults are generally made by slow and noticeable approaches, and are within the ken of the physician. But there have always lurked in the mineral and organic kingdoms, foes which may be likened to assassins who spring upon the unwary and destroy life as by a secret blow. These assailants are the poisons, the very Thugs of nature.

Before the present century little was known of poisons beyond their deadly effects and the attending physiological circumstances. Indeed, any true knowledge of poisons was unattainable until the advent of Chemistry as an exact science. Poisons are naturally grouped into two kinds—inorganic and organic.

In noticing the inorganic or mineral poisons, that one which holds the highest position in historical toxicology, and the one which until recently has been used as a toxic agent very much more than any other, is *Arsenic*. Although known in its metallic state, and as arsenious acid, as early as the eighth century, to Geber, the Arabian, and although it had been used as a poison for centuries, yet so little progress had been made in regard to its chemical detection under these circumstances, that Sir R. Christison, as late as 1824, declared that the tests for arsenic at that time in general use were so fallacious, when applied to complex mixtures, as to be unfit for medico-legal investigations. He then proposed a process for the detection of this metal which, as modified by Fresenius and Babo, continues in use to the present day. At the time of introducing this method, Prof. Christison remarked that by it he was able to detect the one-sixteenth of a grain of the poison. This, at the time, was regarded as a great triumph of analytical skill. Not long before this (1795) Dr. Black stated that one grain was the least he could detect; and at a somewhat earlier period, yet less than a century since (1786), Hahnemann stated that ten grains was the least quantity he could recover.

In 1836, Mr. Marsh, of Woolwich, England, proposed for the detection of this metal, a method which has since been known by his name, and which has proved of the highest value in chemico-legal practice. As originally employed by Mr. Marsh, this test was restricted to obtaining deposits of the metal from the ignited jet of the arsenide of hydrogen; but in 1841 Berzelius proposed the reduction of the arsenical gas by heat applied to the exit tube of the apparatus, and it has been found that this method is even more delicate than that proposed by Marsh. In 1841 Dr. Hugo Reinsch proposed, for the detection of this metal, his copper test, which has proved a most admirable method for the recovery of the poison, especially from complex organic mixtures. In point of delicacy, as now employed, this test is not inferior to the process of Marsh, and it has in certain cases even the advantage over the latter in simplicity of application. The two tests now mentioned, both in regard to simplicity of application and delicacy of reaction, leave little or nothing more to be desired in regard to the recovery and identification of this poison.

One of the first American contributions relating to the detection of arsenic was by Prof. James F. Dana, a graduate of Harvard University, who studied chemistry in London, and was afterwards Professor of Chemistry in Dartmouth College and in the College of Physicians and Surgeons, New York. As early as 1816, he prepared a paper on the "Tests for Arsenic," for which he received the award of the Boylston prize. In 1818, Dr. Thomas Cooper announced the use of chromate of potash as a test for arsenic; but it did not prove to be a distinctive test. In the same year, he read a paper on the subject before the American Philosophical Society (Amer. Journ. Sci., iv. 16). In 1830, Prof. John

P. Emmet, of the University of Virginia, proposed iodide of potassium as a test for this metal. In the next year (1831) Dr. Lewis Feuchtwanger published a paper entitled "Remarks on Arsenic, with drawings of the color of its precipitates," in which he gave a full account of the processes then known for the detection of this poison (Amer. Journ. Sci., xix. 339). An important and original contribution on this subject was made in 1832, by Prof. J. K. Mitchell, of Philadelphia. In this paper, "On the Properties of Arsenic," Dr. Mitchell determined the specific gravity of the different varieties of arsenious acid; its solubility under various conditions; and the vaporizing temperature of metallic arsenic and of arsenious acid (Amer. Journ. Med. Sci., x. 126).

The next contribution to be mentioned is that "On the Detection of Arsenic in Medico-Legal Investigations," by Dr. Wm. R. Fisher, of the University of Maryland, in a paper read in 1836 before the Maryland Academy of Science and Literature, and published in their Transactions (vol. i. p. 117). In 1838, Dr. Robert B. Hall published a Summary of Experiments upon dogs, with hydrated peroxide of iron as an antidote in arsenical poisoning (Amer. Journ. Pharm., x. 263). And in 1840, Dr. Fisher, already mentioned, contributed a paper on the same subject (Ibid., xii. 1). In 1841, Prof. J. Lawrence Smith, of Louisville (to whom science is indebted for many original researches, and whose special investigations of meteoric bodies have given him a wide and lasting reputation), when a student in Paris, prepared a valuable paper On the Tests and Antidotes for Arsenic (Am. Journ. Sci., 40, 278). Two years later (1843), Dr. D. P. Gardner, Professor of Chemistry in Hampden-Sidney College, Va., contributed an elaborate paper On the Application of Reinsch's test for detecting Arsenic in Medico-Legal Investigations (Amer. Journ. Sci., 44, 240). In 1862, Charles H. Porter, Professor of Chemistry and Medical Jurisprudence, Albany Medical College, presented to the Medical Society of New York an excellent paper entitled Medico-Legal Contributions on Arsenic, which contains reports of a number of cases of arsenical poisoning, together with an account of the methods employed in their chemical examination.

We next notice a very valuable contribution by Prof. John C. Draper, of the University of the City of New York, On the Quantitative Determination of Arsenic, published in the American Chemist for June, 1872. Professor Draper suggests a modification of Marsh's apparatus, and the use of a ribbon of magnesium for decomposing the arsenical solution; and, also, the use of a fagot of platinum wire on which the arsenic forms a coating, the weight of which can be determined. By re-heating the fagot in dry oxygen, the arsenic is obtained in the form of oxide, which may be dissolved in water and submitted to the usual tests. In July of the same year, Dr. Draper published in the same journal a paper On the Distribution of Arsenic in the Body. Another contribution to this subject that deserves mention, is a record of The Chemical Testimony in the Sherman Poisoning Case, by Prof. George F. Barker, of the University of Pennsylvania (Amer. Chemist, June, 1872). This paper contains a detailed account of the methods pursued and results obtained in the examination of the different tissues submitted for analysis. In this case, a quantity of arsenic, corresponding to about five grains for the entire organ, was obtained from a portion of the liver. The bodies of four (4) of the eleven (11) persons supposed to have been poisoned by Mrs. Sherman were examined by Prof. Barker, and arsenic found in each.

Having thus noticed the principal American contributions relative to arsenic, it is proper in this connection to refer to some of the more important American contributions relating to other poisons.

In 1827, Prof. Robert Hare proposed a method for determining the presence of opium by the reaction of one of its constituents, meconic acid, in striking a red color with a persalt of iron. He asserted that this test would show the presence of laudanum when only ten (10) drops were contained in one-half gallon of water (Amer. Journ. Sci., xii. 290). With but little modification this test is the one employed at the present time for the detection of opium.

In 1856, Dr. Lewis H. Steiner, of Baltimore, made a report to the American Medical Association, On Strychnia, in which, in addition to a discussion of the chemistry of the subject, he gives a detailed history of the noted case of Dr. Gardner, of Washington, who destroyed himself with that poison. In a paper On the Chemical Reactions of Strychnia, published by Dr. T. G. Wormley, in 1859, in the American Journal of Science, the relative delicacy of the various tests for this poison is pointed out. The fact that the color-reaction of strychnine is interfered with by the presence of morphine, having first been noticed by A. W. Brieger in 1850, this interference is fully considered in this paper, and the conclusion reached, that the interference increases both with the relative proportion and absolute quantity of morphine in the mixture submitted to the test. Another paper to be mentioned in this immediate connection, is that of Prof. John J. Reese, On the Detection of Strychnia as a Poison, and the Influence of Morphia in Disguising the usual Color-test, published in the American Journal of the Medical Sciences for October, 1861. So also in this connection may be mentioned a valuable contribution On the Behavior of the Color-test for Strychnine, with other Vegetable and Animal Proximate Principles, by Dr. Thomas E. Jenkins, of Louisville, Ky. (Semi-monthly Med. News, April, 1859). The behavior of this test with a number of proximate principles other than strychnine, was first examined in 1856, by M. Eboli, Professor of Chemistry at the Academy of Medicine, Lima. Dr. Jenkins so extended this examination as to include about fifty of these principles; and Prof. Wm. A. Guy, of London, in 1861 investigated some sixteen additional substances.

Another contribution to this subject is a Report of a Trial for Poisoning by Strychnia, by Prof. Geo. F. Barker, recorded in the American Journal of the Medical Sciences for October, 1864. In this paper Prof. Barker gives a very complete history of a case remarkable in that violent symptoms were present in about three minutes after the poison had been taken by an adult, the effects thus manifesting themselves perhaps more rapidly than in any other instance yet reported of poisoning by this substance.

An elaborate paper entitled Experiments of Poisoning with Vegetable Alkaloids, by Dr. J. H. Salisbury, was published in the American Journal of the Medical Sciences for 1862. In regard to the limit of the reactions of poisons and other principles, with chemical reagents, Dr. T. G. Wormley has contributed papers on the reactions of the following: Atropine, Chemical News, London, June, 1860; Brucine, Ibid., July, 1860; Morphine, Ibid., Sept. 1860; Narcotine and Meconic Acid, Ibid., Sept. 1860; Corrosive Sublimate, Ibid., Sept. 1860; Veratrine, Ohio Med. and Surg. Journ., vol. xii., No. 6; Nicotine and Daturine, Ibid., vol. xiii., No. 1; Solanine, Ibid., vol. xiii., No. 2; Codeine, Meconine,

Narceine, and Aconitine, Ibid., vol. xiii., No. 4; Conine, Ibid., vol. xiv., No. 1; Oxalic Acid, Ibid., vol. xiv., No. 5.

Mention may next be made of a curious plant, *Erythroxylon coca*, the leaves of which have arrested considerable attention because of their use by the Indians of Peru for the purpose of producing excitement, and giving strength for prolonged physical exertion. Dr. Samuel R. Percy, of New York, presented a paper on this subject before the N. Y. Academy of Medicine, Dec. 2, 1857, and exhibited fine colorless crystals of the alkaloid of the plant, for which he proposed the name *Erythroxyline* (Amer. Med. Times, Nov. 1860). In 1860 Prof. Woehler and Dr. Nieman, of Gottingen, obtained the same alkaloid, giving it the name *cocaine*.

In 1866, Dr. Percy, of New York, presented a paper to the American Medical Association, On Digitaline, its Chemical, Physiological, and Therapeutic Action, for which the Association awarded a prize; and, in 1868, the same gentleman received a prize from the Alumni Association of the Medical Department of Columbia College for a paper On Atropia, its Chemical, Physiological, and Therapeutic Action, with Experiments instituted to ascertain its Toxicological Properties. In January, 1871, Prof. John J. Reese, of Philadelphia, published a communication On the Antagonism of Poisons, in the American Journal of the Medical Sciences, in which he examined the antagonism of morphia with hydrocyanic acid, atropia, strychnia, aconitia, and arsenic, and also of strychnia with tobacco, aconite, iodine, and Calabar bean. For the destruction of the organic tissues, in searching for poisons in medico-legal investigations, Prof. Henry Wurtz, in 1851, proposed the use of bromine as an agent possessing certain advantages over the ordinary agents used for this purpose (Silliman's Journal (2), xi. 405).

We now notice a very interesting and valuable paper on Experimental Researches relative to Corroval and Vao, two new varieties of Woorara, the South American Arrow Poison; by Drs. Wm. A. Hammond and S. Weir Mitchell, published in the American Journal of the Medical Sciences, July, 1859. This is an original and exhaustive examination of the physiological effects, and the chemical properties and physical characters, of these varieties of Woorara. The authors extracted from both varieties an alkaloid, for which they proposed the name *corrovalia*, obtaining it in larger quantity from the variety corroval. This substance was not obtained in a crystalline state, nor were any of its salts. One of the authors of the last-named paper, Dr. S. Weir Mitchell, published in the Smithsonian Contributions, vol. xii. (1860), a very elaborate and complete monograph, entitled, Researches upon the Venom of the Rattlesnake. This contribution is marked by the same patient and thorough research which characterizes the other scientific labors of this original investigator. It considers the anatomy of the venom apparatus, the physiological mechanism of the bite, the toxicological action of the venom on warm-blooded animals, and the various antidotes for the poison. His chemical investigations show the toxicological element of the venom to be a peculiar albuminoid principle, which he names *crotaline*.

Again, in 1868, Dr. Mitchell instituted a series of experiments on the Insusceptibility of Pigeons to the Toxic Action of Opium, in which he showed that this bird was almost wholly insusceptible to the action of that drug. The same immunity was observed whether the drug itself, or morphia, was employed, and also whether it was administered internally or employed hypodermically. In his final experiment, Dr. Mitchell

administered to a large pigeon twenty-one grains of powdered opium, without producing any evidence of opium poisoning (Amer. Journ. of the Med. Sci., Jan. 1869). Dr. B. W. Richardson, of London, on repeating Dr. Mitchell's experiments, fully confirmed his results. As a comparison of the relative susceptibility of the pigeon and the child, Dr. Richardson states that a pigeon can receive at once as much opium as would produce dangerous or even fatal symptoms, if divided equally and administered to twenty children under one year old, the weight of the bird being two hundred times less than the combined weight of the children (Brit. and For. Med. Chir. Rev., April, 1869). Continuing his researches, Dr. Mitchell extended his experiments to the physiological effects of the derivative alkaloids of opium, including morphia, narcotina, thebaia, meconine, codeia, narceia, and cryptopia, employed both internally and subcutaneously. In these experiments, Dr. Mitchell obtained some very interesting and novel results. Thus, he found that while opium, which is a poison to man, is with difficulty hurtful to the pigeon; on the other hand, narcotina, which is nearly inert in man, is fatal to birds (Amer. Journ. of the Med. Sci., Jan. 1870).

We next notice some very original and interesting investigations of Dr. James Blake, of San Francisco, who has been engaged for many years in examining the connection between Isomorphism, Molecular Weight, and Physiological Action (Amer. Journ. Sci. (2) viii. p. 193). Dr. Blake has investigated the effects of compounds or salts of forty (40) of the elements, including substances contained in all the more important isomorphous groups, and in all of these, with the exception of the compounds of potassium and ammonia, finds that when introduced directly into the blood of living animals, the substances in the same isomorphous group exert analogous effects; and also that amongst the more purely metallic elements, the intensity of physiological action is in proportion to the atomic weight of the substance compared with other substances in the same isomorphous group. From experiments made by others, Dr. Blake believes that the same law will apply to organic or carbon compounds, and that living matter will be found to be a valuable reagent for elucidating their molecular relations. His latest researches on Lanthanium and Didymium place the former evidently in the magnesium group and the latter with aluminium. These researches, with those of Drs. Crum Brown and Fraser, and with those of Dr. Richardson, are in a new field of inquiry, and promise very interesting and important results to science.

Having thus glanced at the more important American contributions to toxicology, the way is prepared to take a broader view of this science, and observe the progress of its development, especially in its chemical features. This progress will be seen to be greater in regard to the organic poisons, the marked advance in methods of chemical analysis having made such progress possible.

At the opening of the present century, the fact that the toxic effect of certain vegetable poisons was due to the agency of a peculiar active principle, resident in the plant, had not been determined. The vegetable substance in which the existence of a principle of this kind was first observed was opium, in which Sertürner and also perhaps Seguin in 1804 recognized the presence of a new substance; but it was not until 1817 that Sertürner obtained it in a pure state, and examined its properties. He found that the alcoholic solution of this new principle acted upon

vegetable colors like solutions of the mineral alkalies, and that the substance combined directly with acids, forming neutral salts which were soluble in water, and that it was precipitated from solutions of its salts by the mineral alkalies. To this substance Sertürner gave the name *morphine*, and from the properties just mentioned he regarded it as a kind of alkali; hence the name vegeto-alkali or alkaloid for principles of this kind.

Very soon after the discovery of morphine it was found that nux vomica, white hellebore, cinchona bark, belladonna, tobacco, and certain other vegetable products which had been long known as capable of exerting marked physiological effects, owed their activity to the presence of similar alkaline principles. The number of such natural principles now known is quite considerable. Since 1848 a great number of organic alkalies have been obtained artificially, some of which rival potash and soda in the degree of their alkalinity. The study of these artificial alkaloids has thrown much light upon the constitution of the natural alkaloids, which is, as yet, imperfectly understood.

The discovery of the natural alkaloids has been of great service to the physician, giving him a more concentrated and manageable form of medicine; but to the toxicological chemist this discovery has been of the very highest value, enabling him in cases of poisoning by crude vegetable substances, in just so far as he can recover the alkaloid or active principle from complex mixtures (such as the contents of the stomach), and can separate and determine it by special chemical properties, to identify the toxic substance employed. There are, however, many vegetable and animal poisons, the exact nature of the active principle of which has not yet been determined.

Formerly, about the only method of detecting vegetable poisons was by means of the physical and botanical characters of the subtance taken, such as the leaves, seeds, etc., and this method is yet of service in furnishing corroborative evidence in cases even in which the nature of the peculiar principle is known. It is obvious that in poisoning by crude vegetable and animal substances, we can determine the presence of the poison by chemical methods only, first, when we know that the substance contains an alkaloid or other principle peculiar to itself; second, when we are able to recover this principle from complex mixtures; and third, when we are acquainted with one or more chemical reactions characteristic of this principle. Thus, in poisoning by opium, we cannot prove the existence of opium as a whole, but only inferentially, by proving the presence of a principle peculiar to this drug. Our success in the discovery of organic poisons, where the existence of a peculiar principle is already known, depends largely on our ability to recover such principle from complex mixtures in a state sufficiently pure for the application of special tests.

It was not until 1851 that any real progress was made in this direction, when Prof. Stas, of Brussels, introduced a systematic method for such recovery. This method has proved of the highest importance, and marks an era in toxicological science. It is based upon the fact that the salts of the alkaloids, as a class, are soluble in water and alcohol, but are insoluble in ether; and that these salts when in solution are readily decomposed by the mineral alkalies with the elimination of the alkaloids, which in their free and uncombined state are more or less readily soluble in ether. In any given case therefore, in so far as the active principle or alkaloid

is soluble in ether, and this liquid fails to extract foreign animal and vegetable matter, in just so far can we obtain the alkaloid in its pure state. Professor Stas successfully applied his method for the recovery of the principal alkaloids likely to be met with in chemico-legal investigations, employing for this purpose the usual extracts of the plants in which they are found. Professor Otto, of Brunswick, proposed to modify the method of Stas, in the case of the fixed alkaloids, by first washing the acid aqueous solution of the alkaloidal salt with ether as long as this fluid was colored; the solution is then rendered alkaline and the liberated alkaloid extracted with ether as before proposed. Professor Otto claimed, and justly, that in this manner the alkaloid might be obtained in a state of greater purity, and almost invariably in a crystallized condition.

In 1853, Professors Graham and Hoffman, of London, proposed a method for the recovery of strychnine from organic mixtures, which was afterwards extended to other alkaloids; it is based upon the fact that when a solution of strychnine in its free state is agitated with charcoal, the charcoal absorbs the poison and yields it up to boiling alcohol (Quart. Journ. Chem. Soc., 1853). In practice, in chemico-legal investigations, this method is inferior to that of Stas. In 1856, Messrs. Rogers and Girdwood, of London, proposed the use of chloroform instead of ether, as advised by Stas, for the extraction of the alkaloids. They also proposed to destroy the last traces of foreign matter by charring the chloroform residue with concentrated sulphuric acid over a water-bath (Lancet, London, June, 1856). The use of chloroform as advised by these gentlemen has proved of value in researches of this kind, since it is a better solvent of most of the alkaloids than ether, and has therefore an advantage in certain cases; but, on the other hand, it has sometimes the disadvantage of taking up more freely than ether foreign animal and vegetable matters. Another method for the extraction of the alkaloids from complex mixtures is that of Usler and Erdmann, who proposed the use of amylic alcohol, the principle of the process being much the same as in the method of Stas. Amylic alcohol is especially adapted to the recovery of morphine, solanine, and certain other alkaloids, which are more or less freely soluble in this liquid, whereas they are but very sparingly soluble both in ether and chloroform, and therefore are but imperfectly extracted by these liquids.

In 1862, Professor Thomas Graham, of London, in his very important contribution on liquid diffusion, showed that moist organic membranes have the remarkable property of separating crystallizable substances when in solution from such as are uncrystallizable, the former readily passing through such membranes when surrounded by a liquid, whereas the latter fail to pass or pass only very slowly. These classes of substances he named respectively *crystalloids* and *colloids*, and to this method of separation he applied the term *dialysis*. This method is applicable to both inorganic and organic bodies, and is often found very useful in toxicological investigations.

The latest contribution to this department of research is that of Prof. Dragendorff, of the University of Dorpat, who, in 1868, proposed a very elaborate and exhaustive method for the extraction of the alkaloids and similar principles, and for their separation from each other, based upon their behavior with petroleum-naphtha, benzole, chloroform, and amylic alcohol, dividing them into several groups according as they are affected by these different liquids under certain specified conditions.

From this brief historical sketch, we learn that very considerable attention has been directed to this important and essential part of toxicological chemistry; and, it may be added, the results have been such as to leave little to be desired in regard to the recovery of a large number of organic poisons. We are now prepared to take a rapid survey of the progress made in regard to the special identification of poisons by chemical and other tests. Even less than a third of a century ago, there were but few of the organic poisons for which special and unerring tests were known, especially when present in the minute quantity usually met with in chemico-legal investigations.

In September, 1843, E. Marchand made the important discovery that if strychnine be treated with concentrated sulphuric acid containing a little nitric acid and peroxide of lead, it dissolves with the production of a series of colors peculiar to that alkaloid. The tests employed for the detection of this poison prior to the discovery of this color reaction, which has since been known as the color-test, gave reactions more or less common to substances other than strychnine. In fact, this color-test is the only one yet known the reaction of which, taken alone, is peculiar to strychnine. It was considered a marvel of delicacy of reaction when Marchand announced that this test would reveal the presence of a quantity of strychnine not exceeding the one-thousandth of a grain in weight. This test has since been modified by Mack, Otto, and others, by substituting for the nitric acid and lead compound other oxidizing compounds; and it will now, in the hands of a skilful manipulator, show the presence of strychnine when in quantity not exceeding, perhaps, the one-millionth of a grain.

The discovery of this test did much to stimulate chemists to search for special or characteristic tests for other alkaloids and vegetable principles likely to be encountered in chemico-legal investigations, and in many instances with success. Much remains, however, to be done in this direction, even in regard to some very well known poisons of this class. There are some poisons, the presence of which may be determined by what is known as a combination of tests, no one of which is in itself peculiar or exclusive; but there are others for which we have as yet neither peculiar and distinctive tests, nor any peculiar combination of tests for their detection, especially when present in minute quantity.

It is less than a quarter of a century since the aid of the *microscope* was, to any marked extent, invoked in this department of research, yet within this comparatively brief period its use has very greatly extended our means of determining the specific nature of the results of chemical tests. This instrument not only often enables us to determine the true nature of a precipitate or sublimate by its crystalline or other form, the character of which would otherwise be doubtful, but it enables us to recognize and identify certain substances when present in quantities so minute as to be far beyond the reach of the ordinary methods of examination. Thus by micro-chemical analysis we are able to identify with absolute certainty the reaction of the one hundred thousandth part of a grain of hydrocyanic acid, and of certain other poisons in quantity equally minute. Even a sublimate, consisting of a few crystals, each of which does not exceed the one hundred millionth of a grain in weight, may, under certain conditions, serve to characterize the presence of arsenic beyond a question of doubt. The chief contributors to the micro-chemistry of poisons have been Professors Guy and Taylor, the eminent

toxicologists of England; Drs. Helwig and Earhard, of Germany; and Professor Wormley, of the United States.

For the final purification and subsequent examination by means of the microscope either directly or under the action of reagents, the process of *sublimation* has been proposed, as applicable to both inorganic and organic substances. In 1858, Professor Wm. A. Guy, of London, called special attention to the process of sublimation for the detection of arsenic, corrosive sublimate, and certain other inorganic poisons; and in 1865, Dr. Helwig, of Mayence, announced that in like manner certain alkaloids and vegetable principles furnished sublimates, some of which were characteristic in their microscopic forms and in their behavior with reagents, and he gave photographic illustrations of many of the forms thus obtained. In 1867, Dr. Guy modified somewhat Helwig's method, extended it to a much larger class of objects, and proposed to add to the diagnostic value of the method by a simple arrangement for noting the temperature at which changes in form and color, and sublimation itself, occur.

The brilliant discoveries of Bunsen and Kirchhoff, in 1859, which enable us by means of *spectrum analysis* to recognize and identify with unerring certainty the elementary forms of matter and certain compounds, even when present in quantities so minute as to be far beyond the reach of the ordinary methods of analysis, have not been without some fruit in the department of chemico-legal investigations. Thus Dr. Bence Jones found by this method thallium in the liver, when the ordinary chemical tests failed to reveal its presence.

It is a proof of the great activity and sagacity displayed in modern science that the spectroscope with its amazing capabilities should so soon be combined with the microscope, an instrument of scarcely less capabilities, thus forming the *micro-spectroscope*, by which we are enabled to make new discoveries of the highest value in Medico-legal Chemistry. For this combination of the two instruments we are chiefly indebted to Mr. H. C. Sorby, who has with its aid determined the spectra of a large number of organic coloring principles when in solutions. One of the most important applications of this instrument thus far made in legal medicine, is that for the detection of blood, its delicacy being such as, according to Mr. Sorby, and as confirmed by our own observations, to reveal the presence of a single corpuscle, or less than the one thirteen millionth of a milligramme, or one five hundred millionth of a grain, of that fluid. Although this method will serve to discriminate the coloring matter of the blood from other coloring principles, yet it does not enable us to distinguish the blood of any one animal, including man, from that of another. This, however, can be done within certain limits, but only within certain limits, by means of the microscope; this instrument enables us to specify with great certainty certain animals, even of the same class, from which the blood was *not* derived, but it does not enable us to *positively individualize* the animal from which it *was* derived.

Our discussion of toxicology would be very incomplete without some notice of the subject of the *absorption* of poisons. Prior to the important discoveries of Magendie on venous absorption, in 1809, it was believed that all poisons produced their effects by impressions transmitted through the nerves. The chief argument in support of this view, was that the rapidity of action of certain poisons was incompatible with any other mode of transmission. Dr. Blake successfully met this argument, and showed from experiments that sufficient time elapsed for absorption even

in the case of those substances the most rapid in their action, such as hydrocyanic acid and conine. Through the labors and experiments of Dr. Blake, Sir Benj. Brodie, Sir Robert Christison, M. Bernard, and other physiologists, the views of Magendie were confirmed, and it was accepted that the entrance of the poison into the blood was a condition essential to its action.

That certain substances were absorbed and widely distributed, was early observed from the color imparted to various tissues of the body, and to the urine, by certain coloring principles, as indigo, picric acid, and the like; and by the characteristic odor perceived throughout the body in poisoning by prussic acid and other odorous substances. When we consider how minute is the quantity of certain poisons required to destroy life, the imperfect character of the methods of analysis early employed, and the fact that certain poisons may soon pass beyond recovery, it is not surprising that the progress in recovering poisons from the blood and tissues was somewhat slow. We find, however, that in 1821, Tiedeman and Gmelin found lead, mercury, and barium in the blood of animals poisoned by salts of these metals; that, in 1827, Wœhler recovered certain administered poisons from the urine; and that, in 1829, Wibmer satisfactorily proved the presence of lead in the tissues of animals poisoned by that metal. It was reserved, however, for Professor Orfila, whose original and profound labors in this department have brought honor upon science, to show the absorption and distribution of arsenic throughout the animal body. In January, 1839, this distinguished investigator announced to the Parisian Academy of Medicine, that in poisoning by this substance, the arsenic diffuses itself in such quantity that it may be recovered by chemical methods from the blood, tissues, and various secretions of the body. In 1840, he proved these facts to the satisfaction of a committee of the Academy. This discovery inaugurated a new era in medical jurisprudence. Subsequently, Orfila proved that the same wide distribution of poison throughout the body held in regard to mercury, antimony, lead, and other metallic poisons.

It has generally been believed that in the absorption of metallic poisons, and in their deposition in the soft tissues of the body, a larger proportion is to be found in the liver than in any other part, the least proportion being found in the muscles; but, according to the quite recent researches of M. Scolosubroff, absorbed arsenic, both in acute and chronic poisoning, is deposited specially in the nervous system. Thus, if the quantity found in the fresh muscle be taken as 1, that in the liver is 10.8; that in the brain 36.5; and that in the spinal marrow 37.3 (Ann. d'Hygiène, Jan. 1876). Under the present improved methods of analysis, it now rarely happens, in cases of poisoning by mineral substances, that the poison cannot readily be detected in the various organs and tissues of the body; yet it is well known that even in this kind of poisoning the toxic agent may be entirely removed from the body prior to death.

Our experience in regard to the recovery of the vegetable poisons has not been so satisfactory. While there are some of this class which may be readily detected in the blood and tissues, and others that have been found only rarely, there is still a large number that have never as yet been found in the absorbed state.

That all the organic poisons enter the blood and are diffused throughout the body is no longer doubted. The first instance of the recovery of a vegetable alkaloid, after it had undergone absorption, was announced by Stas, as late as in 1845, and less than a third of a century ago; in this

case he recovered morphine from the viscera of a body that had been buried for thirteen months. Many instances have occurred since, however, in which, even under more favorable conditions, there has been a total failure to recover this alkaloid from any part of the body. So, also, in 1851, Stas detected nicotine in the blood of a dog poisoned by that substance, employing his ether process for the extraction of the alkaloid.

Perhaps no one circumstance did so much to call the attention of toxicologists to the subject of the detection of the vegetable poisons in the blood and tissues, as the remarkable trial, in London, of Palmer for the poisoning of Cook by strychnine, in 1856. Prior to May, 1856, according to Prof. A. S. Taylor, in no instance had strychnine been recovered in an absorbed state from the blood, tissues, or soft organs of the body. This trial directed special attention to this poison, and led to its more careful investigation, and from that time instances of its recovery after absorption have become quite frequent. In fact, under the recent improved methods of analysis, a failure to recover this poison in its absorbed state, should rarely happen. Since this time, chemists have been gradually adding to their triumphs in this important department of research, and now authentic instances have been recorded of the recovery and identification by chemical tests, of at least the following alkaloids: morphine, nicotine, strychnine, brucine, atropine, daturine, veratrine, jervine, and conine. It may be added, that considerable difference exists in regard to the readiness with which the different poisons just mentioned may be recovered when in the absorbed state.

That all poisons, except those endowed with corrosive action, have to enter the circulation in certain quantity before producing their peculiar effects, is now universally conceded. We may now inquire what is known of the mode or manner in which when diffused in the blood they destroy life. It is well known that some poisons produce an appreciable change in the physical and chemical properties of the blood, as shown in the difference of color and consistency of that fluid. These changes are well marked in the cases of sulphuretted hydrogen, prussic acid, carbonic acid, chloroform, and conine, under the action of which the blood becomes more liquid and of a darker color. In most of these cases the poisoned blood, when examined by the spectroscope, gives the spectrum of deoxidized blood.

It was long since observed that certain poisons on being introduced into the blood were either wholly, or in great part, decomposed. Thus, Sir R. Christison failed to obtain any chemical evidence of the presence of oxalic acid in the blood of the vena cava of a dog, killed in thirty seconds by the injection of eight grains and a half of that poison into the femoral vein. Even with the improved methods of analysis, chemists have never yet been able to recover this poison, as such, from the blood. In some cases we can very clearly trace the changes which take place in the substance absorbed by the blood. Thus Erdmann and Marchand, in 1842, found that cinnamic acid was converted during its progress through the animal system into hippuric acid; and in 1862, Dr. Letheby found that under like conditions nitro-benzole was changed into aniline. In like manner it has been found that hydride of benzole is changed into benzoic acid, which in its turn becomes converted into hippuric acid. In 1832, Liebig showed that hydrate of chloral under the action of an alkali, was changed into chloroform and formic acid; and in 1868, Liebrich showed that the same change took place when this substance

entered the blood by absorption, and to this change is attributed the peculiar sedative effects of chloral.

Even in regard to the action of strychnine, brucine, morphine, veratrine, nicotine, and certain other organic poisons, which may be recovered from the blood in their unchanged state, it has been claimed by some that they are changed in part at least before they produce their toxic effect, but of this there is no direct proof. When we consider the extremely minute quantity of substances of this kind which may destroy life (a quantity sometimes certainly not exceeding in weight the one three hundred thousandth part of the blood), and the ready demonstration of their presence in the blood after death, it would appear that in certain instances at least, the fatal issue is not dependent upon the decomposition of any part of the toxic agent.

Although the various facts that have been presented are of great value and interest, yet it must be admitted that the whole subject of the final action of poisons is left in great obscurity. While it is believed by all that poisons are absorbed and taken into the circulation, it is found that only some produce any marked changes, physical or chemical, in the blood; and on the other hand, while some poisons are themselves decomposed in that liquid, yet others equally fatal, appear to suffer little or no change. We may trace the destroyer in all its subtle insinuations through the blood and tissues, and see life quickly driven from the body, but we know not *where* nor *how* the fatal stroke is given. These are questions which modern science, with all its refined methods of research, has left unanswered.

In conclusion, it may be said that the field in which the toxicological chemist labors, is one of the highest responsibility. He must not only be consecrated to his science, know all its best methods, and be able to practise them in all their most delicate and skilful manipulations; but he should have a higher consecration to *truth*, ever remembering that in toxicological jurisprudence his words should be weighed in balances even more delicate and exact than those in which he weighs his material substances. Human life on the one hand, and the security of society and the vindication of law on the other, may hang upon the slender thread of a chemical reaction, or tremble in the final adjustment of a most delicate instrument. The highest skill, and a regard for truth that knows no shadow of turning, can alone fit the toxicological chemist to meet the grave responsibilities of his profession.

ADDRESS ON SURGERY.

BY

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GENTLEMEN OF THE INTERNATIONAL MEDICAL CONGRESS:

THE language of the Aborigines of America has ever been admired for its simplicity and significance. In the State of Mississippi, itself implying the father of all waters, is a County called Itawambi, derived from the title given an Indian Chief, upon whom all possible honors had been conferred, so that nothing more could be done for him. Standing here this day, the representative of the most demonstrative and efficient department of the healing art; the recipient too of many unmerited favors; fully conscious of the inability to do justice to the subject proposed, or satisfactorily improve the illustrious occasion—an event without precedent, and which no living man will ever see again—language fails to convey or words express what is now felt. With all the simplicity of the Indian and the sincerity of the Christian, I would say to you, my professional brethren, who have done so much for me, *Itawambi*.

Precisely fifty years ago, he who is now to address you, landed on Chestnut Street wharf of this city, enquiring for the office of the late distinguished accoucheur, Charles D. Meigs, to become his private pupil; fifty years before that, his father, born in Philadelphia, had been the associate of Rush, Shippen, and James; and now, at the end of those hundred years, the son is invited back to declare before this International Medical Congress, and at the celebration of the first Centennial of these United States, what his countrymen have done for Surgery. Here too in the Medical Department of the University of Pennsylvania, the first chartered Medical College on this Continent, where so many have been taught by Rush and Chapman, Hare and Jackson, Physick and Gibson. To me it is but the coming back to my dear old Alma Mater. Surely "There's a divinity that shapes our ends, rough hew them how we will."

"God moves in a mysterious way,
His wonders to perform;
He plants his footsteps in the seas,
And rides upon the storm."

A mere enumeration of what we have contributed to Surgery, even were that possible during the hour assigned to the subject, might not prove the most profitable occupation of it; and moreover, while this may be the Centennial of our National existence, it is not that of our profession; for, as is well known, the War of Independence left us an impoverished people, almost destitute as regards the sciences, certainly without any peculiar medical literature. Even the reputed father of American Surgery was not admitted a house-surgeon in St. George's Hospital, London, though a private pupil of the celebrated John Hunter, until

1790; nor was he elected Professor of Surgery in the University of Pennsylvania until 1805. It was as late, too, as 1820, forty-four years after the Declaration of Independence, that the taunt was uttered, with probably as much truth as sarcasm, "what does the world yet owe to an American Physician or Surgeon!" Then again, all our early teachers had of necessity to be educated abroad, so that in the very nature of things no national characteristics could have been given to medicine until years after the colonies became the United States. Notwithstanding all this, in a little over fifty years, the sarcasm of Sydney Smith was answered by a professor of Surgery in the city of Paris, M. Chassaignac, remarking as is said to an American medical student, "Sir, you ought to be proud of your country, for at this moment she holds the sceptre of Surgery in the world."

He whom we have ever regarded as the father of American Surgery, Philip Syng Physick, was born in Philadelphia in 1768, and was therefore only eight years old at the declaration of our Independence; he graduated in the literary department of the University of Pennsylvania, received his diploma at the Royal College of Surgeons, London, and the degree of doctor of medicine in Edinburgh, in 1792. Returning home soon after, the appearance of yellow fever, for the first time in Philadelphia, brought him prominently before the public. For services rendered the city hospital in 1794, Dr. Physick was presented with silver plate valued at one thousand dollars.

As early as 1794, he recommended curved forceps holding a needle to secure deep seated arteries. In 1802, he proposed the seton for ununited fractures. In 1836, Mr. Liston gave him credit for this valuable suggestion; and in the Contributions to Practical Surgery, published by Dr. Norris, formerly Surgeon to the Pennsylvania Hospital, in 1873, the seton and its modifications are declared to be safer, speedier and more successful than resection or the caustic.

In 1809, Dr. Physick by a suture removed the projecting duplicated portion of the intestine in Artificial Anus, and thus re-established the integrity of the alimentary canal. Dupuytren's enterotome has since somewhat supplanted the seton.

Physick improved Desault's splint by extending it higher up on the fractured side, thus making extension and counter-extension more in the axis of the broken limb. He was among the first to apply animal ligatures; employing buckskin for this purpose, while his nephew and favorite pupil, Prof. Dorsey, preferred French kid deprived of its tanned cuticle, a substance with which he successfully ligated the femoral artery. Deer sinew torn into shreds of suitable size for the vessel to be ligated, is preferred by others, though silk is generally employed. Animal ligatures certainly irritate less, are readily absorbed, and thereby promote reunion of wounds. In the use of them for some forty years, though not exclusively, the knot of a deer sinew has never been seen, while the metallic ligature or suture ever remains a foreign body in the flesh.

Dr. Physick was the first to employ blisters in gangrene threatening mortification, as in anthrax, or over an inflamed vein The late Prof. Chapman, long his colleague in the University of Pennsylvania, published a case in which he attributed the life of the patient to this suggestion.

He improved the gorget, an instrument employed in his day in lithotomy.

He invented the tonsillotome, in 1848, by which a hazardous operation

has been greatly simplified and made safe. Formerly, attempts were made to cut out the entire tonsils at the imminent risk of fatal hemorrhage, or they were painfully strangulated with a double canula. Fortunately the day has passed for the total excision of these bodies, except when malignant. Intra or extra-oral this is a dangerous proceeding. In a recent attempt twelve arteries had to be tied; now we are content to remove the projecting portion beyond the lateral half arches of the fauces. When reminded that it is the internal carotid which is exposed in these operations, the value of Dr. Physick's method must be greatly enhanced. Of course the recent modifications of the tonsillotome, some of them quite ingenious, are due to the original forceps containing his concealed blades.

Dr. Physick early advocated putting inflamed joints and diseased bones, as in cases of coxalgia, caries of the spine, etc., in perfect repose, as if in splints.

To him also Monsieur Reybard, then surgeon to the hospital of Lyons, France, declares that the profession is indebted for the first accurate account of cystic or sacculated rectum, which he described in the early part of the present century; Reybard moreover asserts that to Dr. Physick was due the starting point of all improvement which has been made in the treatment of abnormal anus.

It fell to his lot to remove successfully, in one case, a greater number of urinary calculi than has ever been done before or since in Surgery. They amounted to upwards of a thousand, varying in size from a small bean to a bird shot. The patient, who was quite aged, held at the time the office of Chief Justice of the United States, and subsequently resumed his seat on the bench.

A striking proof of Dr. Physick's great influence even in Europe was the fact that, when his nephew, then the Professor of Anatomy in the University of Pennsylvania, published his Elements of Surgery, in two volumes, containing, as was well known, the peculiar views and practice of his uncle, that work, the first of the kind ever issued from the American press, actually became the text-book of Surgery in the University of Edinburgh; which was certainly a high compliment to our distinguished countryman.

Physick's name has ever been the most familiar and favorably known of all American surgeons in Europe; and, in 1825, he was elected a member of the Royal Academy of Medicine of France. He was also an early advocate of conservative Surgery; was a favorite pupil of the renowned John Hunter, the great anatomist, physiologist, and surgeon. As he grew older, Dr. Physick became more chary with instruments, and trusted more to the recuperative powers of nature. Of him, it has been said, that he never spilt a drop of blood uselessly, nor wasted a word in his lectures. With the simplicity of a child, the modesty of a maiden, the industry and integrity of a martyr, he quietly worked out a surgical character which will endure so long as his loved science shall be cultivated.

Intimately connected with the origin, rise, and progress of Surgery in America, were four other professional gentlemen, the contemporaries of Dr. Physick, who were long spared beyond the allotted period of man on earth, as if to illustrate in their lives and characters the benefits of temperance, industry, and benevolence. These were Warren, Mott, Dudley, and Gibson, whose average age was eighty-three years.

There were three distinguished Drs. Warren, of Boston, Mass.; Joseph, the early martyr of American Independence, was the senior, the most talented, most patriotic, most heroic. He it was who fell—musket in hand, having declined the position of commander-in-chief—at Bunker's Hill. Dr. John Warren, his brother, studied medicine under him, and graduated in letters in 1771. He gave the first course of dissections in Boston, which, no doubt, led to the organization of the Medical Department of the well-known Harvard University, in which he was the Professor of Anatomy and Surgery for a quarter of a century. His son, the third of the name, JOHN COLLINS WARREN, was born in 1778, and died in 1856. After the advantages of a thorough education in this country and Europe, he succeeded his father in 1815. He was a liberal contributor to our literature, and author of a work on Tumors, which is said to have shed lustre on our science. His last contribution was published when in his seventy-seventh year. He was the first to operate on a patient put in the anæsthetic condition from ether, and was thereby connected with one of the greatest professional events in the world. Of him Velpeau declared that he first performed the operation for dropsy of the pericardium, for he declared that of all the cases then published, his alone was worthy of full credence; and well he might, for the Warrens were ever the soul of integrity. Mr. George Pollock, in the Medico-Chirurgical Transactions of London for 1838, gives credit to Dr. Warren for first closing the fissure in the vault of the mouth. Of him and his son, J. Mason Warren, the worthy representative of the family, and, we deeply regret now to add, deceased, Sir Wm. Fergusson, the prominent worthy representative of British Surgery, recently declared that in regard to the claim of priority in operations for cleft palate, "the first idea I had of this portion of the operation was obtained from Mason Warren. I deem it but fair," he continues, "to the reputation of that distinguished surgeon to state that I know of no originality before his, and that I look upon all modern claims to such originality as arising either from ignorance or the desire to rob the fair reputation of a name, which in son as in father, will stand for generations among the brightest in Surgery." It is certainly a nice point to decide which is the most honored, he who utters such magnanimous sentiments, or the recipient of so disinterested a compliment.

VALENTINE MOTT, a native of Long Island, near the city of New York, was born in 1785, graduated in 1806, and went immediately to Europe, where he remained pursuing his professional studies three years. On his return home he was elected in rapid succession to the chairs of Surgery in Columbia University, Rutgers Medical College, and the College of Physicians and Surgeons, all in the city of New York. When only twelve years in practice he performed one of the boldest, most original, and difficult operations in Surgery, one too, never attempted before, and yet, to the wonder and astonishment of the profession, the patient survived to the twenty-sixth day, and then died, in our opinion, from imprudence in being permitted to exercise in the hospital grounds. Dr. Mott first tied successfully the common iliac artery, doing this in 1827. He first exsected the clavicle, tying, as he said, over forty arteries, besides the external jugular vein twice, and dissecting out the thoracic duct from the diseased mass. This, which he always considered his most formidable operation, required four hours for its performance. Dr. Gross adds that, as a brilliant and daring operation, it is without a parallel in the

annals of Surgery. Dr. Mott was the first to exsect the inferior maxilla, except its articulations, doing this successfully in 1833. He is said to have tied the carotid fifty-one times, and to have amputated about a thousand limbs. Dr. Gross declares that no surgeon, living or dead, ever tied so many vessels or so successfully, whether for the cure of aneurism, the relief of injury, or arrest of morbid growth. And of him, England's greatest Surgeon, Sir Astley Cooper, generously confessed that Dr. Mott had performed more of the great operations than any man that ever lived. His axiom too, through life, was the liberal one of operating in every case which allowed a rational hope of success, either to improve the patient's appearance or to preserve life. No one probably ever handled an instrument more gracefully, was more nimble fingered, or more skilful than the late Valentine Mott. As an operator, he was probably never surpassed.

Benjamin Winslow Dudley, the great Surgeon of the West, was a native of Virginia, born in 1785, the same year, and graduated the same time that Dr. Mott did, viz., in 1806; he, however, in Philadelphia and the latter in New York. Dudley also spent years in Europe, chiefly under Cline, Abernethy, and Cooper in London, and Larrey, Dubois, and Boyer in Paris. Soon after returning home, he organized the Medical Department of the Transylvania University, at Lexington, Kentucky, in which he occupied the chairs of Anatomy and Surgery, and was ever its ruling spirit. Dr. Dudley was the strenuous advocate of thoroughly preparing for all operations; and taught that if the chylopoetic viscera were properly cared for they would provide for the rest of the system. He gave little medicine, but insisted upon the observance of hygiene, diet, rest, travelling, and even sea-voyages. An Englishman, alluding to Dr. Dudley, is said to have remarked that it had been reserved for a backwoodsman of America to teach how to prepare the patient for a capital operation. He justly claims to have first cured a case—and it was a formidable one too—of intracranial aneurism by due preparation and ligature of the carotid. No one ever did so much with the roller—relying upon it even in fractures—of which he was emphatically the master. He was best known, however, as the lithotomist of the West, if not of America. We unfortunately have no statistics of his operations. If a record of his cases operated upon was ever kept, it must have been lost during the late war. We know that he used the gorget, and he is said to have cut two hundred and twenty-five patients, only six of whom are known to have died. He was truly a surgeon of the conservative school, and was for years the surgical patriarch of the West.

William Gibson was born in Baltimore, Maryland, in 1784, and after receiving a classical education went abroad. He took his medical degree in Edinburgh, and through the friendship of Sir Charles Bell obtained extraordinary advantages. He was at Corunna, Spain, when Sir John Moore fell, and at Waterloo, where he himself was slightly wounded. It was he too who extracted the ball from Gen. Scott, long after the battle at Lundy's Lane. He had met Sir Astley Cooper, Sir Henry Halford, Abernethy, Baron Dupuytren, Napoleon Bonaparte, Velpeau, Lord Byron, and other celebrities. Such was his influence in the profession, that, upon the death of Dr. Dorsey in 1818, Dr. Gibson, though Professor of Surgery in the University of Maryland, actually secured that professorship in the University of Pennsylvania, which was then occupied by the father of

American Surgery, Dr. Physick consenting to take the chair of Anatomy with an adjunct. Dr. Gibson was probably the best lecturer on his branch in the United States; in addition to oral instruction, he illustrated his course by splendid collections of paintings, models, and other appliances. His memory was so retentive that he had been known to repeat three hundred lines of Virgil. He was also a bold and successful operator; he was the first to tie the common iliac artery, for a gunshot wound in the streets of Baltimore, the patient surviving thirteen days. He also performed the Cæsarean operation twice upon the same patient, saving each time the mother and child; an operation since repeated by Drs. William Byrd Page, and John Neill, with like success, so that the good city of Philadelphia has been peculiarly favored in this respect. Much has recently been said—this being the Centennial year—of those who first rocked the cradle of American Liberty; but not a word in commendation of those, who resorted to such extraordinary measures, to keep in motion the little one called into requisition at each accession to her infant population. Alas! for the glory of poor doctors.

In the brief biographical sketches of those to whom we are so much indebted for the present position of Surgery in our country, reference has been made to what they did on the kindred subjects of Aneurism and the Ligation of Large Arteries; and while we may not yet report a successful tying of the aorta, notwithstanding the encouragement implied by finding that vessel occasionally obliterated, and the success by compression reported by Dr. Murray, of Newcastle-upon-Tyne, England, we can, nevertheless, quote what Mr. Erichsen, the well-known author and surgeon, has recently published—"the bold and difficult operation of arresting the circulation through the brachio-cephalic, is one of the most remarkable, and the only one found recorded in the annals of Surgery" —referring to what was done by Dr. Andrew Woods Smyth, of Charity Hospital, New Orleans, Louisiana, the 15th of May, 1864. The patient was a mulatto, aged 22, a steward on a steamer; the tumor, which was of the size of an orange, was developed in four months, after a collision at sea, to which it was attributed. On the 15th of May, 1864, a ligature was applied on the innominate artery, a quarter of an inch below its bifurcation; and one also on the right carotid, an inch above its origin. Pulsation returned at the wrist in forty-eight hours. On the 28th, the ligature came away from the carotid, and the next day, the fourteenth after the operation, hemorrhage occurred to syncope. This recurring the two following days, the wound was filled with small shot. On June 1, the detached ligature was removed from the brachio-cephalic. Hemorrhages still continuing at intervals, on July 9, the right vertebral was tied; being the fifty-third day after the original operation; and to this, the success in the case is fairly attributable. The shot remained in the wound thirty-eight days. The patient now moved to Galveston, Texas, but returned in 1874, ten years afterwards, to New Orleans, with another tumor in the neck causing great pain. Dr. Smyth now detected an aneurism larger than the former one, which the patient said had been developing for about a year. On October 5, 1874, he tied the internal mammary artery in the third intercostal space. Abscesses now began to form under the clavicle and were cautiously opened. The patient again returned to Galveston, and then once more to New Orleans, where he lingered until the 6th of April, 1874. A post-mortem examination

revealed the facts, confirming the history above given of this deeply interesting case.

The patient lived within a few days of ten years after the innominate was tied; enjoyed the best of health, and had gained twenty pounds. In this case Dr. Smyth tied not only the innominate, but the common carotid, vertebral, and internal mammary arteries, and did more to relieve the affection than was ever attempted before in Surgery. This patient lived in a hot, moist climate, and was probably purpuric.

Attempts to ligate the aorta successfully thus far have failed. In this country, Prof. Hunter McGuire, of Richmond, Va., tied it unsuccessfully in 1868; and the next year, Dr. Stokes, of Dublin, Ireland, met with a similar result, though he reached the vessel by an almost bloodless incision through the lumbar region.

Maisonneuve is reported to have tied the vertebral artery. We know that Prof. Willard Parker, of New York, did this in 1863, and as has been noticed, Dr. Smyth, of New Orleans, tied it in 1864. Prof. Warren Stone, of New Orleans, is reported, in a case of gunshot wound of the vertebral artery, to have arrested the hemorrhage by compression with pledgets of lint.

The common carotid, Dr. Gross claims was first secured by Dr. Cogswell, of Hartford, Conn., who in exsecting a scirrhous tumor from the neck in 1803, applied two ligatures to that vessel. Heretofore Sir Astley Cooper's case in 1805, had been considered the first one. In 1875, a surgeon recently from Europe, now Prof. Frothingham, of Ann Arbor, Michigan, boldly laid open a traumatic aneurism of the common carotid, turned out the clots, with a piece of glass, tied both ends of the wounded vessel, as also the internal jugular and lingual veins, and saved his patient. This was certainly a very creditable operation. Dr. Mussey, in 1829, tied both carotids successfully after an interval of twelve days. In Dr. Mott's case the simultaneous application of ligatures to these vessels proved fatal.

Of the branches of the common trunk, the internal and external carotids, the latter is the one which is most frequently tied. Indeed Mr. Bryant in his Practical Surgery, in 1872, declares that, owing to the difficulty of finding the bleeding point, the ligature to the internal branch has been abandoned. Nevertheless it has occasionally been tied by some of our practitioners. In 1845, Dr. J. H. B. McClellan, son of the late Prof. George B. McClellan, founder of the Jefferson Medical College, in removing a portion of the parotid gland found it necessary to ligate this artery; so did Prof. Gurdon Buck, of the New York University, for a deep wound in the parotid region, tie successfully the common and internal carotids; Dr. Stephen Smith, in 1864, secured this vessel after pressure had failed, and he performed a similar operation for sloughing of the tongue; finally, my colleague in the chair of Surgery in the University of Nashville, Dr. William T. Briggs, has the credit of first applying two ligatures, one on each side of a wound which resulted in an aneurism, tying also the common carotid at the same time. This was done in February, 1871. Then again Dr. Sands, Professor of Anatomy in the College of Physicians and Surgeons of New York, without knowing what Dr. Briggs had done, did the same thing in October, 1872, besides securing a lateral wound made in the internal jugular vein.

The method adopted in these latter cases was carrying out the principle of Mr. Guthrie—the application of two ligatures to a wounded artery in contradistinction to the one only insisted upon by John Hunter. This

then is an American illustration of a new principle in tying wounded arteries, which fortunately has occurred in the practice of two of our best anatomists.

The late Prof. Wright Post, of New York, succeeded first with ligature to the left subclavian artery in 1817, a feat which made him quite famous, since some of the most distinguished surgeons of Europe had failed to do this. In 1866, Dr. T. G. Morton also tied the left subclavian, when the upper extremity of that side becoming gangrenous, he amputated it at the shoulder-joint and saved the patient. In two cases of subclavian aneurism, Dr. J. Mason Warren succeeded with ice and direct compression, when the disease was too far advanced for the ligature.

To Dr. Warren Stone, in 1850, is given the credit of first applying a metallic ligature to a human artery, and in 1866, Dr. Claudius H. Mastin, of Mobile, Alabama, applied one successfully to the external iliac. Dr S. W. Gross, son of the professor, has recently proven by numerous experiments how innocuous are ligatures to veins; and the late Dr. Levert, at the suggestion of Dr. Physick, published his thesis, in 1829, proving thus early, the advantage of metallic sutures.

It is difficult to decide who first suggested the ligature to arteries supplying the extremities for affections threatening mortification. Our English friends have generally given us the credit, but it does seem that tying the carotid for internal aneurism, as was done by Dudley, or the femoral for elephantiasis, as was done by Carnochan; the use of pressure to check the redundant supply of blood to a vascular tumor; or even elevating the inflamed part—all apparently illustrate one and the same principle of cutting off the redundant supply of blood.

Arterial compression as an antiphlogistic has long been recognized in the profession. Dr. March published a brochure on the value of compressing the brachial artery in cases of whitlow, a notice of which may be seen in the American Journal of the Medical Sciences, Vol. 18, for 1849. It is there stated that this seemed to be more efficacious than the plan proposed by Mons. Gerdy, of keeping the limb elevated.

Sir James Paget, moreover, declares that the proposition to treat cases of active inflammation threatening gangrene by putting a ligature on the main arterial supply, emanated from the American School of Surgery. In the Lettsomian Lectures on Surgery, published in the London Lancet, Mr. C. H. Maunder, Surgeon to the London Hospital, expressly declares that the operation originated in America; and in Holmes's System of Surgery, Mr. Simon mentions that Henry Campbell, of Augusta, Georgia, then Professor of Anatomy in the New Orleans School of Medicine, and who was an army surgeon during the late war, had even raised the question whether the hand or foot, wrist or ankle, forearm or leg, should ever be amputated for destructive inflammation, specially resulting from traumatic causes, whenever the state of the patient admitted this to be done, without first resorting to previous experimental ligation of the artery supplying the affected region. Dr. Druitt, who contributed the article on Inflammation to the last edition of Cooper's well-known Surgical Dictionary, in 1872, refers, moreover, to Dr. Campbell as the introducer of what he terms the Hunterian ligation of arteries to prevent or relieve destructive inflammation. The operation, first practised by Onderdonk, in 1813, and D. L. Rogers, in 1824, has been revived and systematized by Prof. Campbell, who recommends it as a definite mode of treatment.

Prof. Campbell claims that (1) Compression prevents drainage; liga-

tion prevents blood going to a part. (2) Ligation to prevent inflammation he repudiates as unjustifiable and dangerous, and even liable to produce mortification; while ligation to cure inflammation already existing, he says is invariably safe, and will prevent mortification, or even stop it after it has begun. These distinctions he declares were never brought previously before the profession. He believes that no one before himself ever tied an artery with the distinct view of curing inflammation and preventing gangrene.

One of the most remarkable cases connected with the subject of aneurism, whether considered in reference to its origin, the organ affected, or the extent of the disease and the result of treatment, was reported to the American Medical Association by the late Prof. Mussey, of Cincinnati, when chairman of the Committee on Surgery in 1850. It was that of a mechanic of St. Louis, Missouri, aged 25 years, whose right eye protruded and pulsated; the transverse suture near the external angle of the eyed admitted the tip of a finger, and the functions of both ear and eye of the affected side were no longer performed. After thorough preparation by Dr. Dudley, and ligation of the right common carotid, the patient so completely recovered that he actually resumed the labor of a blacksmith.

Mr. Erichsen acknowledged, in 1851, that to Prof. Carnochan, of New York, was due the credit of recommending a ligature to the femoral artery to cure elephantiasis; when this gave way from the extension of the disease, he tied the external iliac, and finally had to resort to compression on the distal side of the bleeding orifice. Sixteen months after the first operation, the patient was in robust health.

American surgeons have not failed to try compression for the cure of true aneurisms. The late Prof. Jonathan Knight, of New Haven, Connecticut, than whom few ever had a better record, has the credit of first relieving a case of popliteal aneurism by digital compression, the case being published in the Transactions of the American Medical Association for 1848. In the Philadelphia Medical Times for 1875, Dr. R. J. Levis, one of the Surgeons of the Pennsylvania Hospital, published a very creditable case of successful compression for aneurism of the internal iliac. This was effected by means of a simple steel band, having a hollow pad for counter pressure and a convex one with a screw upon the artery. In five and a half hours the current of blood was totally arrested through the tumor; there was some œdema and discoloration, yet the patient fully recovered in thirty-seven days.

Of twenty-three cases of digital compression, comprising a successful one of his own, collated by Dr. S. W. Gross, of Philadelphia, fifteen were reported as cured. In two cases of inguinal aneurism, compression failed.

Within a few days a pamphlet has been received from Walter Reid, M.D., Staff Surgeon Royal Navy, Plymouth, England, in which the author proposes to cure external aneurism by compression in a few hours. Concluding from Cohnheim's experiments that in warm-blooded animals a stasis of circulation may be safely continued for from six to eight hours, he made a successful experiment in a case of popliteal aneurism. Esmarch's elastic bandage was alone applied with the usual pressure of a roller above and below, while the pressure was diminished upon the tumor itself. The proposition certainly deserves further consideration.

In 1860, Dr. John H. Brinton, of Philadelphia, published in the American Journal of the Medical Sciences, an accurate description, with illustrations, of the valve at the termination of the right spermatic vein in the

vena cava ascendens, and its absence on the left side, where the corresponding vein terminates in the renal. Here then is an obvious and satisfactory reason, though it may not be the only one, of the frequency of varicocele on the left side as compared with the right. A suggestion for the treatment of varicocele, originating, if I mistake not, with myself, is the application of animal ligatures to the spermatic cord, excluding, of course, the vas deferens; then the ablation of some four inches square of the scrotum; and the closure of the wound with pin sutures. By this operation, both pathological conditions, the varicose veins and redundant scrotum, are removed.

In the treatment of affections of Bones and Joints, we claim to have made valuable suggestions, if not important additions. An unusual recovery from undoubted fracture at the base of the cranium, which recently occurred on one of our national ships, may deserve brief notice. The sailor was seen to fall from a spar to the main deck, and was taken up insensible; he had stertorous breathing, followed by a copious flow of colored serum from the right ear, estimated at fifty or sixty ounces; and became partially comatose. Brain substance was also detected in the discharges. He, however, gradually improved under treatment, and was returned to duty a little over a month after his fall, though slightly epileptic, and with some little discharge still from the ear.

We find in an early number of the New England Journal of Medicine and Surgery, that Dr. Luke Howe treated a fracture of the inferior extremity by the means now employed, excepting the omission of the sand bags to prevent the rolling of the injured limb; and believed it to be original. His case is reported in 1823, more than half a century ago. In the American Journal of the Medical Sciences for August, 1829, Dr. William C. Daniel, then an eminent practitioner of Savannah, Georgia, described how he applied a silk handkerchief around the ankle and tied it at the sole of the foot, a weight being then attached and passed over a roller for extension. That veteran of American Medical Journalism, Dr. Isaac Hays, says that Dr. Daniel has precedence of all modern claimants for introducing the weight and pulley as a means of extension in treating fractures; for he himself had published two cases thus successfully treated, one in 1819 and the other in 1824. Dr. Hartshorne also, formerly one of the surgeons to the Pennsylvania Hospital, declared in 1869, in the same journal, that the credit of priority in introducing the weight extension in practice was justly due to this gentleman.

To Dr. Gurdon Buck, of New York, credit is given for introducing adhesive plaster to secure extension in the treatment of fractures of the inferior extremity. Prof. Gross, however, in his account of American Surgery, claims this for his preceptor, Dr. Joseph K. Swift, of Easton, Pennsylvania, who employed it as early as 1829.

Dr. William Gibson, of St. Louis, Missouri, devised in 1867, a ring for fractures of the patella; and Dr. P. S. O'Reilly, of the same city, in 1871, described what he calls a new splint for fractures and dislocations of this bone. This consists in an open ring, with a thumb-screw, that it may fit the more accurately; to which a strap extending under the sole of the foot may be added, and also a posterior padded splint for the thigh and leg. This seems the perfection of an apparatus for these injuries.

On the subject of deformities after fractures of the thigh, two of our most distinguished Professors and Surgeons in the Jefferson Medical College hold the position which we also sustain; that a fractured

femur cannot be fully extended to its normal length, nor could it be so retained without impairing its nutrition. They declare that a certain degree of shortening, and perhaps deformity, cannot be prevented, whatever may be the treatment. And it may be added that no measurement whatever can detect the difference in some cases, though one to a limited extent actually exists, as may be verified in a post-mortem examination.

The method of reducing dislocations by manipulation was first systematized and proposed as such by the late Dr. William R. Reid, of Rochester, New York, and published in 1859; though probably first suggested by the late Nathan Smith, of Connecticut. This seems to have created a new era on the subject, notwithstanding that it had occasionally occurred to some practitioners how easily reduction was effected, sometimes even during the examination of the case. Prof. Spence, of Edinburgh, in his Lectures published in 1875–76, calls this the rotatory or circumduction method, and adds, "we generally hear it spoken of as the American method." He also admits that it is undoubtedly to the writings of Drs. Reid and Bigelow, of the United States, that we owe the more general use of the practice. He notices too, specially, the ilio-femoral, or inverted Y ligament, and the different views entertained by Mons. Chassaignac, of Paris, from those expressed by Dr. Bigelow, who was the first to note minutely its functions. Dr. Reid's conclusions regarding dislocations are: (1) That the impediment to reduction is due to the stretch of the muscles, by the malposition of the dislocated bone, and not to their contraction; (2) That in reducing luxations these muscles should be relaxed; (3) That dislocation on the dorsum of the ilium is reduced simply by flexing the leg upon the thigh, and carrying the thigh over the sound one, upwards over the pelvis as high as the umbilicus, and then rotating it.

As is well known to the profession, Prof. Nathan Smith, of New Haven, had taught and published similar views to those of Dr. Reid. The late Prof. Brainard, of Chicago, Illinois, reported, in 1852, the reduction of four cases of dislocation of the head of the femur into the thyroid foramen, with the aid of a billet of wood four inches in diameter, well padded, placed in the perineum as a fulcrum, the displaced limb being then carried as a lever across the sound one.

The substitution of manipulation to reduce dislocation at the hip, is claimed therefore to be American, and in connection with anæsthesia, the great boon to suffering humanity, also introduced by us, to be inestimable.

Old as the world may be, it has been reserved for an American to point out a pathognomonic sign by which a fractured clavicle can always be distinguished from a dislocation at the shoulder joint. Prof. L. A. Dugas, of the Medical College of Augusta, Georgia, asserts that if the fingers of the injured limb can be placed by the patient or surgeon upon the sound shoulder, while the elbow touches the thorax, there can be no dislocation; in other words, he asserts that it is physically impossible to bring the elbow in contact with the sternum, or front of the thorax, if there be a dislocation, and that no other injury of the shoulder-joint can induce this inability. Ample experience has shown this rule to be axiomatic.

The late Prof. Crosby, of New Hampshire, in dislocations at the metacarpo-phalangeal articulation pushed the phalanx backward until it assumed a position perpendicular to the metacarpus; then pressing forward the base of the dislocated finger, reduction was easily effected. Dr. Levis, Surgeon to the Pennsylvania Hospital, suggested a small splint,

having four large gimlet holes bored in it through which tapes are passed to secure a luxated finger or thumb, by which means extension can be effectually made.

As a substitute for the pulley, Dr. Fahnestock, of Pennsylvania, suggested a rope doubled several times upon itself, which by a stick—a walking cane—placed in its centre could be twisted gradually so as to produce extension in dislocations.

In reference to resections and exsections of the bones and joints, our surgeons have been active and ingenious. As early as November, 1826, Dr. John Rhea Barton, then one of the surgeons of the Pennsylvania Hospital, sawed through the femur just between the trochanters, and then straightened the limb, which became so useful that the patient went out gunning and actually brought home his own game; owing, however, to his habits, those of a common sailor, he did not derive the full benefit expected from the operation; but a similar one, in which, however, a disk of bone was removed, performed by Dr. J. Kearney Rogers, of New York, in 1830, resulted by gentle manipulation in the formation of an artificial joint, and in an excellent recovery: good success has attended other cases similarly treated by our practitioners. Mr. Erichsen declares that the first operation for osseous angular anchylosis was performed by Dr. Barton. He also asserts that, in 1837, Dr. Mussey excised the whole of the scapula and clavicle for an enormous osteo-sarcoma, the patient being in good health fifteen years afterwards. These successes no doubt induced others to enter the new field of investigation, and resulted in Dr. Brainard, then Professor in the Rush Medical College, Chicago, Illinois, submitting, in 1854, to the American Medical Association, an essay on a new method of treating ununited fractures, and certain deformities of the osseous system, which secured the prize for that year. The main feature in Dr. Brainard's method was that by making perforations in the osseous structure with drills, and then waiting for the softening process of inflammation, almost any bone, or anchylosed joint, might be straightened, broken up, or the deformity relieved. Barton, Brainard, and Buck, were undoubtedly the pioneers in correcting deformity and restoring to usefulness, limbs, which before their day were considered hopelessly useless, or were sacrificed by amputation.

The first resection of the inferior maxilla was performed in what was then known as the back-woods of America, and this too without known precedence, or even professional counsel or assistance. The case is admitted by Mr. South, who translated Chelius's Surgery, Drs. Mott, Gibson, Gross, and others. The operator was William H. Deadrick, then of Rogersville, East Tennessee, who in February, 1810, exsected about three inches of the lower jaw, including the whole of its circumference and one of its angles, from a boy, fourteen years old, who recovered and was alive years afterwards. Dr. Carnochan, of New York, is believed to have first disarticulated this bone in its entirety in the Emigrants' Hospital, of New York, in 1851; and this was again done the next year, by Prof. Wedderburn, then in the Medical College at New Orleans.

The late Prof. Horatio G. Jameson, of Baltimore, claimed for his country the honor of having led the way in resections of the superior maxilla. Dr. Gross admits that the first severe and difficult operation of this kind was performed by him successfully in 1829, by removing nearly the entire bone on one side.

The removal of the clavicle, or a portion of it, was probably first attempted by Dr. Charles McCreary, of Hartford, Kentucky, and was

reported by James H. Johnson, in the Medical Journal then published in New Orleans, in 1850. In his System of Surgery, Dr. Gross mentions that the operation was performed in 1815, on a boy who survived long afterwards.

As early as 1853, Dr. Compton, of New Orleans, exsected the entire ulna and all of the radius, except its lower extremity, in a patient fifteen years old, who made an excellent recovery.

In 1843 Josiah C. Nott, then Professor of Surgery in the Mobile Medical College, excised two segments of the coccyx, for neuralgia, in a lady after twenty-five years of great suffering. The case, first published in the New Orleans Medical Journal for 1844, was copied into the American Journal of the Medical Sciences, and Prof. T. Gaillard Thomas has added that this was fifteen years before a similar operation was performed by the late Sir James Y. Simpson.

Prof. Lewis A. Sayre, of New York, reports that, up to January, 1876, he had exsected the head of the femur fifty-one times, with a mortality of only twenty; eight of these fatal cases too had recovered from the operation. He removed also portions of the acetabulum in some of these instances. At the late meeting of the American Medical Association in Philadelphia, he introduced a little patient, suspending him by the shoulders and head; plaster of Paris bandages were then applied, and in a few minutes the little boy walked about the room, though he had not done so for months before.

In connection with affections of bones and joints, did time permit, there might be noticed what has been done here in Kyllosis and Orthopædy. Dr. James A. Dickson, of North Carolina, is believed to have first performed tenotomy by dividing the tendo Achillis in 1835, followed by Dr. D. L. Rogers, of New York, in 1845. Then came Dr. Detmold, a pupil of Stromeyer, who in 1837 settled in New York, where he introduced myotomy. Dr. Mott, however, founded the first Orthopædic Institution in America. In 1844, Dr. Henry J. Bigelow, of Boston, Massachusetts, obtained the Boylston prize for that year for his work on Orthopædy. Dr. Sayre, however, has done more on this subject than any other American. His work just published is undoubtedly the best ever issued from the press. He has called particular attention to genital irritation, even in very young children, in connection with deformity. He gives special credit to Mr. Barwell, of London, and Dr. H. G. Davis, of New York, for devising apparatus for extension while the little patients exercise in the open air; and has himself introduced a new method of doing this by plasters, elastic cords, or artificial muscles, as they are called. For contracted limbs and congenital deformity much is being done here by early attention, so as to prevent development in unnatural positions, by gradual or forced flexion and extension under ether, electricity, etc., before resorting to tenotomy or myotomy.

In the Compendium of Practical Surgery, published in 1851 by Bérard, Denonvilliers, and Gosselin, of Paris, in referring to a paper written by Dr. M. Smith, son of the celebrated Dr. Nathan Smith, commenting on incisions of the periosteum and trephining of bones in suppurative inflammation, is found a compliment to the heroic measures proposed by the Americans.

In reference to the Genito-urinary Organs, we refer first to the case of Dr. Charles L. Stoddard, of Wisconsin, who is reported to have exsected the kidney successfully in 1861, eight years previous to the operation of

Prof. Simon, of Heidelberg, who operated in 1869. In 1870 Prof. Gilmore, of the Mobile Medical College, successfully cut out an atrophied kidney from a colored woman, then five months pregnant, who, nevertheless, went to her full term, and was safely delivered of a living child.

Our early surgeons performed almost exclusively the lateral operation for urinary calculus, and almost as exclusively employed the gorget—an instrument of which it can happily be said *requiescat in pace.*

Ribes, of Paris, first described accurately the bilateral operation for stone in the bladder. Dupuytren, however, gave it character by proving its advantages to be easy and more direct access to the bladder, and that the prostate is cut in its central portion, and in double the extent of the section in the lateral operation, without endangering nerves, vessels, or other important structures. Dr. Ashmead, of Philadelphia, in 1834, first performed the bilateral operation in the United States.

Mr. Erichsen asserts that Civiale recommended a combination of the median and bilateral operations, for in this way, he declares, the chief objections to both are removed. The medio-bilateral is gaining ground as an operation for urinary calculus.

As is well known, when Mr. Erichsen, among others, denied the possibility of the intromission of foreign bodies below the vocal cords, Dr. Horace Green passed a tube through the rima glottidis some inches into the trachea, and then withdrawing the stilet, directed the patient to close his lips and blow through the canula. This he did several times, even so forcibly as to extinguish a candle. Sponging below the vocal cords was first done in October, 1854, by the same gentleman.

The case reported by Profs. Crosby and Peaslee, of New Hampshire, of a sponge one inch and three-quarters in breadth and fifteen-sixteenths of an inch in thickness, passing the rima glottidis, in a patient aged forty-five, on July 23, 1850, and who lived fifty-four hours, ought surely to have convinced every one of the possibility of at least introducing sponge-probangs into the larynx.

In 1844, Dr. Charles Hall, of Burlington, Connecticut, removed, after tracheotomy, a piece of pipe-stem by means of a probe, which fortunately entered the hollow or canal of the pipe-stem and stuck fast. This certainly was a fortunate hit. Mr. Liston once extracted a piece of sponge from the right bronchus of an adult; and Mr. Dickin, in 1832, a bell button from the right bronchus of a child eight years old.

Mr. Spence, Professor of Surgery in the University of Edinburgh, succeeded, in June, 1862, in extracting, by tracheotomy, the inner portion of a tracheal tube which had lodged in the bronchus, and concluded that it was the first and only instance of the kind thus relieved. A child, however, was brought from Alabama in May, 1853, to Augusta, Georgia, where, after the trachea had been opened, a four-penny nail was removed from the left bronchus with a pair of dressing forceps lengthened in the blades.

American surgeons present a creditable report on amputations. Statistics on the subject, whether derived from hospital records or army reports, or more especially from private practice, sustain this position. Much, no doubt, is due to our open climate, sunny country, cleanly habits, good nourishment, and free exercise. Even in regard to the dreadful operation of amputation at the hip-joint, it is probable that the second successful case, if not the very first ever performed, was that of Dr. Walter

Brashear, of Bardstown, Kentucky. This was done in August, 1806, on a youth of seventeen years, who survived long after it. His method was the circular, as if for the upper third of the thigh; and, after securing the arteries, then the head of the bone was disarticulated through a long incision over the trochanter major. May not this prove to be the best method for this amputation? The case of Perault, of St. Maure, France, in 1773, was only the semblance of an amputation, since the limb had been destroyed by mortification up to the hip.

Mr. Erichsen, Baron Larrey the second, and M. Legouest, of the French army, declared most emphatically that there had not been a case of recovery from primary hip-joint amputation recorded in military surgery previous to our late war. In the Schleswig-Holstein, French, German, and Italian wars, not a case survived, whether operated upon by German, English, French, Italian, or Russian surgeons; whereas, during the late war between the States, some three or four were reported as successful, though subsequently this number was reduced, because, on account of the difficulty in the condition of the country, after our gigantic internecine war, these cases could not be accurately determined. We may judge, however, of the impression of our success in operations at the hip-joint in Europe, when the Surgeon-General of the Prussian army, the distinguished Dr. Von Langenbeck, declared that, but for these operations, it might be questioned if immediate amputations and resections at the hip should not be hereafter prohibited.

In the New York Journal of Medicine, for 1852, it is stated that up to that date there had been on the Continent of Europe thirty-five amputations at the hip, with fourteen recoveries, or forty per cent.; in England twenty-five, with eleven recoveries, or forty-four per cent.; and in the United States eleven, with eight recoveries, or seventy-three per cent.

In civil practice, we have the recent remarkable case of Dr. Jerome Sands, of Portchester, New York, who, without waiting for reaction after the injury by a passing train, performed hip-joint amputation by antero-posterior flaps, on a boy eleven years old. The administration of ether, performing operation, and dressing wound, all consumed only three hours. Prof. Spence, of Edinburgh, under similar circumstances, waited several hours before he operated; his case too, was a success, and it may be the first and only one of the kind in Europe. In Philadelphia, where surgery has been so successfully practised, two of her Professors in one College, Drs. Gross and Pancoast, have each performed two amputations at this articulation, and saved the four patients. Yet this is the operation, it will be remembered, of which the great English military surgeon Hennen, asserted, only fifty years ago, that there was not one in a thousand who would not prefer instant death to the attempt.

No one can be expected to do justice to so prolific a subject as the history of American Surgery, in the brief space allowed the occasion. An enumeration of our contributions to this department of medicine would alone require a volume. Time will permit now only a brief summary of some suggestions or improvements not yet noticed.

The extensive use of silver wire in surgery and gynæcology is undoubtedly due to Dr. J. Marion Sims. This is acknowledged in Holmes's Surgery. To him also is given the credit of publishing the first work ever issued on Uterine Surgery in the English language.

Paracentesis thoracis was made a safe operation by Drs. Bowditch

and Wyman, of Boston, before the introduction of Dieulafoy's aspirateur.

Dr. A. L. Pierson, of Salem, Massachusetts, was the first to operate for hare-lip with scissors and sutures, within six to twelve hours after birth.

Prof. Alex. H. Stevens first suggested the important principles to be observed in exsecting tumors, viz., by the first incision to expose fairly the morbid growth, and secondly to remove it and nothing more.

The œsophagus forceps of Dr. Bond, of Philadelphia, are so constructed that, by narrow bevelled edges, a pin or fish-bone can be removed without pinching the mucous coat.

Of Dr. Sayre's vertebrated probe, Mr. Erichsen declares that it is a most ingenious and valuable one, by accommodating itself to the tortuosities of a canal or sinus.

Dr. Levis, of the Pennsylvania Hospital, recommended pure carbolic acid liquefied, as a local anæsthetic, before using the actual cautery, and the late Prof. Josiah C. Nott, of Mobile, declared that this same agent in carbuncle was antiseptic, escharotic and anæsthetic.

Prof. Joseph Jones, of the University of Louisiana, New Orleans, one of the most indefatigable laborers in the profession, after a most thorough examination of the important subject of traumatic tetanus, concludes that the remedial agents most to be relied upon, are chloral hydrate, Calabar bean, chloroform, sulphuric ether, cannabis Indica, tobacco, bromide of potassium, and opium. He also suggests that important advances will be made in the therapeutics of this affection, if the attention of the entire profession can be concentrated on a few remedial agents, as chloral hydrate, Calabar bean, and bromide of potassium. In regard to the prevalence of this terrible affection, the census of the United States for 1850 gave six cases for the six New England States, while for Louisiana alone there had been two hundred and fifteen.

Prof. F. H. Hamilton, of Bellevue Hospital, New York, is the author of a well-known Treatise on Fractures and Dislocations, which has reached its fifth edition. He has recently issued a System of General Surgery, of about one thousand pages, with illustrations; and has also published a Treatise on Military Surgery and Hygiene. During the war, he held a high rank, and has long been known as one of our ablest surgeons.

The Principles and Practice of Surgery, by John Ashhurst, Jr., M.D., issued by Henry C. Lea, Philadelphia, 1871, a volume of a thousand pages, containing five hundred and thirty illustrations, has probably done more than any other publication to announce what Americans have done for surgery.

The founder of the Jefferson Medical College, Prof. George McClellan, reported, long ago, eleven excisions of the parotid gland with only one fatal case.

Prof. Van Buren, M.D., of Bellevue Hospital, recommended the French method of relieving anal fissure, by sudden dilatation from within outward with the thumbs, so as to rupture or tear open the sphincter muscles.

The British and Foreign Medico-Chirurgical Review stated in 1870 that, in plastic operations on the face, and in closing of wounds and unnatural openings in the urinary organs and parts of generation, Americans must have the position of precedence.

Another surgeon, Professor Gouley, of New York, has distinguished himself in external urethrotomy without a guide, and in relieving strictures.

Dr. John T. Hodgen, Professor of Surgery in the St. Louis Medical

College, proposed a few years ago the removal of calculi lodged in the urethra, by passing a double wire concealed in a catheter, and expanding it after its introduction into that canal.

Prof. Henry J. Bigelow has recently published an article on "The true Neck of the Femur," another on "Exstrophy of the Bladder," and a third on "Turbinated Corpora Cavernosa."

Dr. Addinell Hewson, Surgeon to the Pennsylvania Hospital, found a mortality, after amputations performed with an ascending barometer, of about eleven per cent.; when it was stationary, some twenty; and when descending, amounting to twenty-eight per cent. This certainly deserves investigation.

In proof of the industry exhibited, and of the contributions made by Americans to Medical Science, a long catalogue of periodicals, numerous monographs, and extensive systems in the three practical departments of the profession might be enumerated. One of these, on Surgery alone, has reached the fifth edition, contains fourteen hundred engravings, and requires a text of more than two thousand pages in small type; being equivalent to more than half a dozen volumes of ordinary size. Then there are some forty Journals devoted to medical science, from the weekly to the quarterly, containing reports of cases, lectures, movements in the profession, etc.

We can point, too, to the admirable publications issued from the office of the Surgeon-General U. S. A., pronounced to be incomparably the most minute, comprehensive, and valuable ever issued from the press. And, as evidence of the industry and efficiency of our corps of Surgeons, it is said in the official reports of the late war between the States, that the mortality in the medical staff was greater than in any other.

The commendations from foreign sources respecting these publications, have been most gratifying to our national pride. The Medical Inspector of the French army, and director of the school for the Medical Staff of that army, Mons. Sédillot, wrote, in 1866, that American Surgery had proved itself equal to cope with the exigencies of a war of incomparable magnitude; surgeons everywhere, said he, were proud of the marvels accomplished in science and self-devotedness.

In Circular No. 6, are classified 87,822 wounds and injuries, and 17,125 operations. These reports have been translated into the German, French, and Italian languages. The Director-General of the British Medical Department returned thanks for the gift of a work so replete with interest of the highest professional value. Sir Wm. Fergusson said in the fifth edition of his standard work on Surgery, that modern surgery owed much to our American brethren—referring to conservatism, which he says was signally displayed both in the United States Army, and by their gallant contemporaries in the Confederate service. Baron Von Haurowt, chief Surgeon of the Russian Marine, in a report for 1867, after an official inspection of the medical institutions of this country, speaks of the Army Medical Museum, at Washington, as the most valuable collection of the kind in existence.

In an address to the students of St. Thomas's Hospital, London, Oct. 1874, Mr. McCormac said, no European nation, probably, can point to such a monument of industry, or so great a mass of valuable material, carefully preserved and digested, as the records of the Medical Department of the U. S. Army. It is, he continues, an undertaking simply gigantic, reflecting credit on the government that originated, and the men who carried it out. Such records, he says, are of the utmost value

both to the military and civil service. No less than fourteen foreign medical journals noticed our Army medical reports.

In 1875, Dr. Crothers, of Albany, New York, stated that Dr. Swinburne's attachments for extension and counter-extension were being introduced into the French army, with the American pattern and minutiæ for the battle-field. Dr. Swinburne, himself, formerly Port Physician of New York, while travelling in Europe in 1870–71, by request took charge of a hospital, and he and Dr. Evans, Dentist to the French Emperor, received the Cross of Honor for services rendered. Dr. Monsereau declared before the Faculty of Paris, that the American Ambulance was the best of all.

In the Prussian Service, our ambulance was also adopted. So again, Mons. Sarcey, who wrote the "Siege of Paris," announced the superiority of this method of treating the wounded on the battle-field. It was freely confessed that American Surgeons had done more for Military Surgery than the ordinary experience of a century, and that never before was the business of life-saving done more thoroughly and successfully than during our great war.

The discovery of the circulation of the blood in 1628, and the introduction of vaccination in 1796, were the significant events of the past two centuries in our profession. That of the present one will, in all probability, prove to be the crowning glory of American Surgery. For it is that which Poets and Philosophers, Philanthropists and Physicians, and the Humane of all ages, had sought for in vain for six thousand years. The search had been for that agent which could suspend consciousness and obtund sensibility, so that the most painful manipulations might be made, or capital operations performed, with little or no risk to life. It has been reserved for American Surgery to teach the world how to prevent or relieve human suffering. God in his mercy has condescended to teach us how to relieve pain. We can now say to the agonized mother, Awake! behold your babe; to the tetanic limb, Peace! be still.

Well has it been said that no imagination can conceive and no tongue express, the accumulated woe that anæsthesia has assuaged, and is yet destined to prevent or relieve.

Fifty years ago, there were probably not a half-dozen Americans known abroad as Surgeons. As early, however, as 1830, Dr. Reese, in preparing an American Edition of Cooper's Surgical Dictionary, introduced no less than one hundred and nine American contributions to it; and Mr. Cooper, himself, soon after, publicly expressed his regret for not having noticed what we had done in this department of medicine; admitting, too, that we had performed operations never attempted in Europe, and had led the way by our splendid achievements in operative Surgery, and demonstrated their practicability and success. The hour assigned me for presenting what we have done in Surgery, during the past century, especially its latter half, has expired; the result, however imperfect, is before you. Time, if nothing else, has prevented even the naming of all that is worthy of special mention. I trust, however, that quite enough has been shown to warrant us in committing the interests of our time-honored calling, the noblest of earth, to our successors of a second centennial, in the full persuasion that all its interests, its usefulness, its honor, and its dignity, will be fully sustained.

ADDRESS ON MEDICAL BIOGRAPHY.

BY

J. M. TONER, M.D.,

OF WASHINGTON, D. C.

GENTLEMEN OF THE CENTENNIAL INTERNATIONAL MEDICAL CONGRESS:

I APPEAR before you to discharge the duty assigned me of preparing a biographical retrospect of the medical profession of the United States during the centennial period just past. Though apparently an easy task, I cannot approach it without hesitation. Apart from the feelings of diffidence, which under any circumstance this occasion and this audience must inspire, I am fully aware of the caution with which the office of the biographer should be assumed, and that his best efforts are never above criticism.

It is admitted that the measure and the character of the renown which attaches to the hero is, in a great degree, qualified by the fitness of the chronicler who undertakes to record and perpetuate his achievements. With what intimate knowledge and forecast, then, should even the most ready pen essay the work of writing the life of the good and great; when, through natural inability for the task, or want of care, the noblest actions may be placed in a false light, obscuring what should be displayed, or obtruding defects that the mantle of prudence and charity should cover! Conscious of the many difficulties and the very brief time allotted to the reading of this discourse, I shall confine myself to the narration of a few simple facts in the lives of the more illustrious, and to a record of the names of physicians who attained distinction during the century, and I trust that this limitation may be some excuse for the dryness which must necessarily characterize such details.

In glancing over the period to be embraced in this retrospect, I am struck by the paucity of really striking events which have influenced the practice of medicine, and which have left special marks at the end of the first century of our national existence.

Wars have generally been promotive of medical science, and our profession was no doubt much benefited by the contest for Independence. For the first quarter of a century after this armed struggle, the leading physicians and surgeons were those who had served in the army.[1] The most notable event of this period was the occurrence of epidemics of yellow fever, which appeared in the summers of 1793 and 1798 in nearly all our Atlantic cities. This disease tested the courage and taxed the energies and best skill of the profession, and prompted the more eminent

[1] The following are the names of the leading practitioners in each State who served during the Revolutionary War: Connecticut, Aneas Munson; Delaware, James Tilton; Georgia, Lyman Hall; Maryland, John Archer; Massachusetts, John Warren; New Hampshire, Josiah Bartlett; New Jersey, William Burnett; New York, Charles McKnight; North Carolina, Charles Harris; Pennsylvania. Benjamin Rush; Rhode Island, Isaac Senter; South Carolina, Peter Fayssoux; Virginia, James McClurg.

to reduce their observations to writing, and to have them published, either in defence of their practice, or for the laudable purpose of making contributions to medical science. To us of the present day, it is an agreeable surprise to find that there were then so many medical men of literary ability[1] in our country.

The second quarter of the centennial period was distinguished by the introduction of vaccination, the occurrence of spotted fever, and the war of 1812. All of these were events which stimulated the profession to more extended studies, and became incentives to authorship; this was especially true of the disease known as spotted fever.[2] The war of 1812 proved to be another great school of experience, although it was not fruitful in medical reports or publications. The aspirations which it aroused in the profession, however, gave an impetus to the establishment of medical periodicals, and to the founding of medical colleges[3] and hospitals.

In following out the plan of dividing the century of our independence into quarters, the third may be marked as noted for the discovery of anæsthesia, the epidemics of Asiatic cholera of 1832 and 1848, and the war with Mexico, as well as the discovery and the application of many new and improved methods of physical exploration in the search of disease. These aids to diagnosis encouraged more than ever the recording of clinical observations and their publication. Medical journals multiplied,[4] and new medical colleges were founded in most of the States.[5]

The last quarter, which has just closed, is specially distinguished by the vast experience of the late war, which was a great school, and which

[1] In my own library I find pamphlets on the epidemics of yellow fever of 1793, and 1798 published before 1800, by the following authors: Dr. J. S. Addoms, Dr. Richard Bayley; Dr. Thaddeus Brown, Dr. C. Caldwell, Mathew Carey, Dr. Isaac Cathrall, Dr. W. G. Chadwell, Dr. Colin Chisholm, Dr. Thomas Condic. Dr. William Currie, Dr. John Beale Davidge, Dr. M. S. Davis, Dr. Jean Deveze, Dr. Richard Folwell, Dr. James Hardie, Dr. J. Henry C. Helmuth, Dr. Charles Holt, Dr. Alexander Hosack, Dr. William Linn, W. Marshall, Dr. D. Nassy, Dr. Pascalis, Dr. J. Patterson. Dr. Benjamin Rush, Dr. Charles Scot, Dr. James Tytler, Dr. Washington Watts, Dr. Noah Webster, and Dr. Nathaniel Weeks. There are also many reports by committees and boards of health, besides numerous articles on the subject in the "New York Medical Repository."

[2] The following are a few of the most noted writers on spotted fever: Elisha North, Job Wilson, Thomas Miner, William Tully, L. Danielson and E. Mann, Elijah Lyman, Samuel Woodward, Abraham Haskell, Mason Spooner and Jacob Holmes, John Bestor, Q. Fiske, and G. Williamson.

[3] In 1800, there were in the United States but four medical colleges organized and giving instruction, viz.: University of Pennsylvania, Pa.; Columbia College, N. Y.; Harvard College, Mass.; and Dartmouth College, N. H. The number of medical colleges fully organized and giving instructions in the United States in 1825, was eighteen, viz.: University of Pennsylvania, Pa.; College of Physicians and Surgeons, N. Y.; Harvard College, Mass.; Dartmouth College, N. H.; University of Maryland, Md.; College of Physicians and Surgeons Western N. Y., N. Y.; Yale College, Conn.; Medical College of Ohio, Ohio; Vermont Academy of Medicine, Vt.; Transylvania University, Ky.; Brown University, R. I.; Medical School of Maine, Me.; University of Vermont, Vt.; Berkshire Medical College, Mass.; Medical College, S. C.; Jefferson Medical College, Pa.; Columbian Medical College, D. C.; University of Virginia, Va.

[4] It has been ascertained that about two hundred medical journals have been started in the interest of regular medicine in the United States within the centennial period. Of this number, about thirty are now being published. From an estimate, based on pretty good data, I am persuaded that something over seven hundred medical men have been engaged as editors and assistant editors of journals within this period. Medical editors, as a class, are ready writers, well informed, enterprising, and progressive in their profession. The influence of medical journal literature is elevating, and encourages study and the reporting of cases.

[5] At the end of the third quarter, 1850, the United States census gave a list of thirty-seven medical colleges fully organized and giving instruction.

has benefited the medical profession of the whole country;[1] by the extended use of anæsthesia in painful surgical operations; by the increase of scientific means for exact diagnosis, and the introduction of new and potent remedies and modes of administration; and by the founding of hospitals and medical colleges in nearly all the large cities.[2]

It is from the professional men who labored not only in these more notable scenes, but from the whole profession who practised during this period, that I shall select for comment those who acquired honorable distinction among their contemporaries. It is well known to you that owing to the peculiar condition and growth of our country, from thirteen Colonies to forty-nine States and Territories during the century, it is difficult to so treat the subject as to be entirely impartial, and not to neglect the claims of any section of the country, old or new.

I will first speak of the medical men of the United States who have by their discoveries, writings, or special skill and devotion to their professon, won national fame, and then of those who have attained distinction within the several States. Having an earnest desire to make the address acceptable to this Congress, and for the sake of making it worthy of the occasion, I have collected formal biographies of one hundred of the most eminent medical men of the United States for the century. These sketches have been prepared at my request by physicians of note in possession of the requisite data.[3]

We know that greatness in any avocation, and particularly in the medical profession, is not a birthright, nor can it be thrust upon the undeserving. A kind of popular transitory applause may be achieved without merit, but only those who have done something to advance medical knowledge or to improve the methods of cure, will live in history. The discoverer or the expounder of a new truth, the recorder of an additional fact or a hitherto unobserved symptom, or of an improved procedure in surgery or in the treatment of disease, deserves and will secure a more enduring place in history than he who has gained great popularity or the largest fortune. The qualities and acquirements and the degree of these which can make a medical man illustrious either in his day or in history, I shall not attempt to define. The talent differs in kind and degree that attains eminence in different periods of time, and in the city as compared with the rural districts, in the surgeon[4] as compared with the physician,[5] and in the writer[6] as compared with the

[1] The late war did for us what the wars of Europe in 1848 and 1856 did there. It improved surgery much, and developed pathology wonderfully, brought hygiene into active exercise, improved the treatment of bone injuries by exsection and resection, and the treatment of wounded joints. The conservative treatment of limbs, the postponement of the period of operation, and the improvement in hospital construction and treatment, mark this period not less than the actual number of operations performed by the extended use of anæsthetics.

[2] The close of the last quarter of the centennial period records sixty-five medical colleges, not including dental schools or colleges of pharmacy.

[3] [The biographies referred to in the text, are necessarily omitted for want of space.—EDITOR.]

[4] Professor Samuel D. Gross, in the "Century of American Medicine," published in the American Journal of Medical Sciences, names nearly three hundred American surgeons, who, by their operations and reported cases, have won an enduring place in the literature of the profession.

[5] Dr. T. Gaillard Thomas, in the "Century of American Medicine," published in the American Journal of Medical Sciences, names nearly one hundred and thirty American physicians who have won distinction in obstetrical practice and gynæcological surgery.

[6] Professor S. D. Gross, in his Introductory Lecture entitled "History of American Medical Literature," names nearly three hundred medical authors for the century. From an

teacher.[1] But that there is a something in talent or character which commands success, and that this element stands in the relation of cause to effect, is generally conceded. Even when an elevated standard of professional ability has been adopted, I am embarrassed not only by the difficulty of selection, but by the superabundance of good material.

The names here presented as having won pre-eminence in the medical profession of America, have acquired their distinction chiefly by the possession of high natural endowments and of good education and medical knowledge, with the exercise of unceasing study, and devotion to professional duty. My study is too limited to assume that names not included may not be equally deserving.

The natural ability, habits of industry and systematic study, and scientific knowledge of Dr. Benjamin Rush, added to his acquaintance with men and public affairs, easily place him at the head of the list of the eminent medical men of the century. His professional skill, high moral and benevolent character, and frequent participation in the affairs of his country, rendered him popular with the profession and endeared him to the people. His fame has suffered but little by the lapse of time. His writings are numerous and valuable. (b. December 24, 1745; d. April 19, 1813.) But as the subject of medical literature has been assigned to an abler pen, I shall leave that branch entirely to him.[2]

Dr. Valentine Mott, by his daring and brilliant operations in surgery, held for many years the front rank. He had the physical and mental endowments, as well as scientific acquirements, application, and professional training, essential to a great surgeon. There is scarcely an operation, however hazardous or delicate, that he did not perform with consummate skill and success. His genius, too, was equal to devising new operations and important surgical procedures, and undertaking and executing some operations which before his day were passed over as too formidable to be attempted. He was scarcely less eminent as a writer and teacher than as a surgeon, and no American name is better known to the profession throughout the world. (b. August 1785; d. April 26, 1865.)

Dr. Philip Syng Physick was a surgeon of rare ability, self-possession, and fortitude. As was usual, when he commenced practice, he attended to the general business of physician and surgeon, but as early as 1794 he was appointed one of the surgeons to the Pennsylvania Hospital, and in 1805 Professor of Surgery in the University of Pennsylvania. He always carefully prepared his lectures, and was therefore particularly impressive, for he had thought over and mastered the subjects he presented. He was the author of so many improvements in the department

estimate I have made on this subject, I conclude that there are some four hundred authors who have published works, which from their size are entitled to be termed books. If we include pamphlets, it is probable that there are over fifteen hundred medical authors

[1] From a computation which I have made of the teachers of medicine in our country, for the century, I am inclined to think that the list will include nearly two thousand names.

[2] Professor L. P. Yandell, of Kentucky, has been designated to deliver the address on "American Medical Literature" for the centennial of our national existence, and to Prof. N. S. Davis, of Illinois, has been given the task of preparing an address on "American Medical Education" for the same period. To Prof. Austin Flint, has been assigned the duty of preparing a history of medicine in our country for the hundred years just past. A careful regard for the field of labor given to each of these essayists, has made me solicitous not to traverse their province, but to adhere strictly to biographical detail at the risk of being tiresome.

of surgery that he was justly entitled to the appellation given him of "The Father of American Surgery." His influence upon practice is felt to the present day. (b. July 7, 1768; d. Dec. 15, 1837.)

Dr. John Warren was an eminent physician and surgeon, and medical teacher. He acquired experience and reputation in the hospitals of the Revolution. While attached to the hospital at Boston, in 1782, he founded the Medical Department of Harvard College, in which he was Professor of Anatomy and Surgery. He was a man of great industry, and enthusiastically attached to his profession, and was during his lifetime the leading surgeon of New England. His genius and aptitude for this branch of the profession has continued to show itself in his descendants. (b. July 27, 1753; d. April 4, 1815.)

Dr. Daniel Drake possessed wonderful powers of original observation, and was the most indefatigable worker for the accumulation of knowledge and the elevation of the profession, that our country has produced. He was the founder of medical colleges in the West, and a writer and teacher of great force and ability. His systematic inquiries into the causes of disease are models worthy of study. The Doctor's valuable labors are certain to be appreciated in the future. (b. Oct. 20, 1785; d. Nov. 6, 1852.)

Dr. John Collins Warren was one of the founders of the Massachusetts General Hospital, and the surgeon in daily attendance to the time of his death. He was the first to use ether, and did much to hasten its introduction into general use in surgery. He was a brilliant operator, popular as a lecturer, and wrote on a great variety of subjects, in all of which he showed his exact knowledge and extensive scientific acquirements. (b. 1st Aug. 1778; d. May 4, 1856.)

Dr. Nathan Smith was a man of extraordinary natural endowments. His ability to acquire knowledge, his industry, perseverance, and success under almost insurmountable difficulties, have rarely been equalled. His founding of the Medical Department of Dartmouth College, and his teaching, by himself, for ten years, all the usual branches, show the character of the man. He was an admirable anatomist, a bold and successful surgeon, and a good general practitioner. By his teachings he did much to advance medicine. (b. Sept. 30, 1762; d. Jan. 26, 1829.)

Dr. Reuben Dimond Mussey was a distinguished surgeon and good general practitioner. His experiments on cutaneous absorption would have of themselves entitled him to a high rank, had he not become still more eminent as a surgeon. For daring and success, some of his capital operations have never been excelled. He was also a popular and successful teacher of surgery. The esteem in which he was held by the profession may be inferred from his election to the presidency of the American Medical Association. (b. June 23, 1780; d. June 21, 1866.)

Dr. James Jackson was a noted physician and author. He was one of the founders of the Massachusetts General Hospital. His great skill in diagnosis, and his eminent success in the treatment of disease, led to his appointment to the professorship of Theory and Practice in Harvard College. His observations, teaching, and writings, almost mark an epoch in medicine in this country. His professional life and labors were great lessons to the profession of high and honorable principles. (b. Oct. 3, 1777; d. Aug. 27, 1867.)

Dr. Nathaniel Chapman was a learned and eminent physician, and teacher of medicine. His special qualifications and success as a physician speedily attracted attention, so that his business was very large and

responsible. He was for a long time a professor in the University of Pennsylvania, and was in some respects the most popular lecturer with students ever resident in that great centre of medical education. His writings have had much influence over the medical opinions of his time. He was President of the American Philosophical Society, and the second President of the American Medical Association. (b. May 28, 1780; d. Jan. 1, 1853.)

Dr. Theodoric R. Beck was a prominent physician, author, and teacher. He was particularly notable for his literary and scientific attainments, power of classification, and scope of studies. Professor in several colleges, he filled one of the first chairs of medical jurisprudence established in this country. His work on this science would of itself entitle him to rank with the most cultivated medical men of the Union, and has been for years the class-book in foreign schools of medicine. (b. Aug. 11, 1791; d. Nov. 19, 1855.)

Dr. Samuel Jackson was a learned and accomplished medical man and teacher. In every department, whether at the bedside, or as President of the Board of Health, or in the lecture room, he showed himself the great physician. He filled a chair with eminent success in the University of Pennsylvania from 1825 till 1863 when he resigned. His membership in learned societies as well as his able writings testify to his distinguished ability. (b. March 22, 1787; d. April 4, 1872.)

Dr. Wm. Potts Dewees deserves prominent notice as a physician and author. He possessed a methodical and discerning mind, and was an extensive and careful reader of the best works in the profession. His great powers were brought out at the bedside. He was a good writer, and an admirable oral teacher. His lessons were law in the practice of obstetrics and diseases of women and children. Some of his works ran through many editions. (b. May 5, 1768; d. May 20, 1841.)

Dr. Samuel G. Morton was a conspicuous physician and naturalist. He was a most careful and laborious student, and did a prodigious amount of valuable original scientific work. His love for science, however, did not lessen his devotion to his profession, as his writings on consumption, and other medical works, prove. His fame is enduring, and rests chiefly on his original observations in medicine and craniology. (b. Jan. 26, 1799; d. May 15, 1851.)

Dr. Elisha Bartlett was a highly accomplished scholar and physician. He was a very popular medical teacher, and a ready lecturer, holding chairs in several leading medical colleges. His work on the "Fevers of the United States," published in 1842, at once secured him a place in the front rank of physicians. His purity of life and eminence as a clinical physician, as well as the high character of all his writings, have secured for his name an abiding place in the history of his profession. (b. Oct 6, 1804; d. July 19, 1855.)

Dr. John K. Mitchell was an able physician and a polished writer. He possessed a vigorous intellect, which was well trained and by careful study stored with facts. In the sick room he is said to have had a most encouraging influence over patients and their friends. He was an original thinker, with a vivid imagination, and one of the most charming of lecturers. His writings cover a variety of subjects, all exhibiting ability and versatility of talent. (b. May 12, 1796; d. April 4, 1858.)

Dr. Réné La Roche was a learned physician and author. He was a most laborious and conscientious student, exhausting all sources of information to the fullest extent on whatever subject he took up. He was for

many years connected with the Board of Health of Philadelphia, and was a most valuable member. His practice for years was large and responsible. His studies and writings on yellow fever are a marvel of labor and systematic presentation of all the known facts relating to this disease. (b. 1795; d. December 9, 1872.)

Dr. David Hosack was a celebrated physician, botanist, and author. He had a large practice, and yet found time to lecture in different colleges, and to write exhaustive treatises on medical subjects. He held important positions in the Almshouse, in the New York Hospital, and in the Bloomingdale Asylum. His contributions to medical science are numerous and valuable. (b. August 31, 1769; d. December 22, 1835.)

Dr. John Morgan was a man of learning and an eminent physician and surgeon. He enjoyed the distinction of being the founder of the first medical school in America. At that time he was perhaps the most thoroughly educated and accomplished physician in this country. He was a good writer, and was the first to suggest that pus was formed by a secretory process. He was surgeon-general of the Continental Army, and one of the founders of the American Philosophical Society. (b. 1735: d. Oct. 15, 1789.)

Dr. Josiah C. Nott was a prominent physician and surgeon, and a learned ethnologist. He was a man of commanding powers of intellect, with great industry and capacity for study. While he won a world-wide fame as an ethnologist, he did not neglect the duties of a large practice. He was eminent as a teacher, and as the founder of the Medical College of Mobile. His ethnological writings, however, are those which are best known, but he was equally distinguished as a physician and surgeon. (b. March 31, 1804; d. March 31, 1873.)

Want of time will prevent comment upon all our medical worthies. I however present the following list of names, incomplete though it be, in which it would be difficult to assign a precedence, for all of them have won national reputations, and are eminently deserving of commemoration. I am persuaded that I might more than double the list, and yet not include all who have won reputations that have extended beyond the confines of their own country:—J. Mason Warren, Caspar Wistar, Wm. Shippen, Samuel Bard, Wright Post, Chas. Caldwell, Benjamin S. Barton, Benjamin Waterhouse, David Ramsay, John D. Godman, Wm. E. Horner, Franklin Bache, Robley Dunglison, Warren Stone, John Ware, James Thacher, Wm. W. Gerhard, C. D. Meigs, Hugh L. Hodge, Edward Miller, Wm. Gibson, J. W. Francis, George McClellan, Henry Miller, P. S. Dorsey, B. W. Dudley, J. B. Davidge, J. A. Swett, Samuel Jackson, H. Green, C. A. Pope, Jeffries Wyman, John Eberle, T. Miner, John Jones, G. T. Elliot, A. A. Gould, S. L. Mitchell, S. H. Dickson, J. P. Harrison, Jonathan Knight, Ernest Krackowizer, Daniel Brainard, Charles Frick, G. C. Blackman, J. W. Heustis, Alexander H. Stevens, E. Hale, Erasmus Fenner, B. R. Welford, J. M. Galt, James McClurg, James Moultrie, Henry F. Askew, G. Mendenhall, Nicholas Romayne, J. R. Barton, R. S. Kissam, A. Brigham, T. D. Mütter, Ephraim McDowell, R. Bayley, W. Curry, John Jeffries, J. A. Cartwright, L. V. Bell, D. F. Condie, S. Forry, J. A. Gallup, S. G. Morton, W. Beaumont, Thomas Sewall, E. H. Barton, S. Brown, J. Watson, Jos. Parrish, C. A. Lee, J. Torry, Alden March, Lyman Spalding, Josiah Bartlett, J. M. Smith, Dixi Crosby, Valentine Seaman, E. A. Holyoke, Amos Twitchell, E. H. Smith, J. B. Beck, Usher Parsons, Caleb Fiske, J. E. Holbrook, William

Baynham, F. T. Stribling, L. M. Lawson, J. R. Coxe, T. C. James, R. W. Gibbes, John P. Mettauer.

In looking over the available records of the lives of the medical men who have labored in the several States during the century, I am struck with the evidence of the high average professional ability, and wonderful fidelity, with which they have discharged their duties. The testimony, too, is ample as to the high esteem in which the vast majority of medical men have been held by the communities in which they resided.

I am aware there are those who would draw this picture less rosy. And I will concede that the opinion held on the point depends to some extent upon the temperament and motive of the inquirer. If he seeks to find the illiterate, the incompetent, or the unworthy, bearing the title of doctor, I am sorry to say such can be found. But if, on the contrary, he looks as I have done to find the educated, the intelligent, the skilful, the conscientious and faithful physicians, they are to be found in great numbers in every city and village in our broad and prosperous republic. I am persuaded that the medical profession of the United States, notwithstanding all that has been said by its friends in their desire to elevate it, really has within its ranks proportionately more talent, nobility of character, and high scientific acquirements, than are to be found in any other profession in this or any land.

The number of great physicians who have by their discoveries and writings added new facts and extended the boundaries of medical science, is limited in any century and every country. This fact ought not to be lost sight of.

To the young and to the inconsiderate only do great reputations seem a matter of course. To the conceited nothing seems beyond their untried powers. Age and experience, however, demonstrate the limitations and possibilities of human intelligence and acquirements, which, it is conceded by the wisest, are comparatively narrow and circumscribed.

Medical science and the medical profession in America have made vast progress in the hundred years just passed, and have brought into prominence during that time many medical men of genius and rare abilities, and sometimes from unexpected quarters. If there is any one thing in which the American people has a united faith, it is in progress. As a nation and a people we believe we shall be better physically, morally, and intellectually, in the future than we have been in the past.

From a careful study of the average proportion of physicians to population, it is rendered probable that about 65,000 physicians have died in the United States during the century. Of even the few whose genius, skill, and devotion to medicine, in the several States, have rendered their lives notable, it is not possible to present more than the merest outline; little, indeed, beyond an imperfect list of names. To do even this meagre justice to my subject, I was obliged to correspond with physicians in different parts of our country, and beg them to assist me in collecting the necessary facts; and to these gentlemen I wish to return my most sincere acknowledgments for their valuable assistance. Whatever defects appear in this paper, and doubtless there are many, they are my own.

Alabama.[1]—In this State Josiah Clark Nott, a native of South Caro-

[1] Alabama was visited by De Soto as early as 1541. The first successful settlement was made upon Mobile Bay in 1702, by Bienville. This State formed a part of Georgia until

lina, was for years the leading medical spirit. His eminence as a surgeon, author, and teacher, placed him in the front rank; he has already been mentioned. (b. March 31, 1804; d. March 31, 1873.) Dr. J. W. Heustis wrote on the diseases of the South as early as 1817. In 1825 he published a more systematic treatise on the "Bilious and Remittent Fevers of Alabama." (b. 1784; d. 1841.) Dr. Wm. M. Boling, of Montgomery, was a physician of education, and extensively patronized. Dr. Thomas Fearn and his brother Dr. Richard Lee Fearn, of Mobile, were natives of Virginia, and accomplished physicians. Dr. Silas Ames, of Mobile, was a physician of skill, and esteemed remarkably successful in the treatment of fevers. The following also deserve mention, viz.: Drs. Samuel D. Holt, Leroy Anderson, A. Lopez, A. G. Mabry, H. S. Levert, James C. Harris, Nicholas Merriwether, P. H. Lewis, Edward Gant, and R. C. Armstrong.

Arkansas.[1]—Dr. Benj. P. Jett, of Washington, Hempstead Co., came from Virginia, and enjoyed a large and responsible practice. (b. Oct. 25, 1808; d. Dec. 29, 1865.) Dr. Craven Peyton, of Little Rock, served as a surgeon in the war with Mexico, and thereafter retained a preference for the practice of surgery, in which he was very successful. The great esteem in which he was held was manifested by the closure of the banks and business houses, in order that their employés might attend his funeral. (d. Nov. 7, 1872.) Dr. Charles B. Mitchell, a native of Tennessee, practised in Washington. His business habits, intelligence, and enterprise, made him many friends, and caused him to be elected U. S. Senator, 1860–61. (b. Sept. 19, 1815; d. Sept. 20, 1864.) Dr. Shephard Laurie, a native of the District of Columbia, was for many years one of the leading physicians and surgeons of Little Rock. I will also mention Drs. Geo. Eving, Bumford, G. G. Shumard, Alden Sprague, Burton, and Gibson.

California.[2]—This new State holds out such golden promises as to

1802; from this time till 1817 it was included in the Mississippi territory, and was then organized into a separate territorial government, and admitted as an independent State to the Union in 1819. Alabama has advanced rapidly in population and wealth, and has given much encouragement to general education and the advancement of the sciences. In 1870 she had 996,992 inhabitants. She has no large cities; about one in twelve of her people live in cities and towns of over five thousand inhabitants. One medical college exists, located at Mobile. (Note.—Summer schools, as adjuncts to colleges, are in no instance enumerated.) A State Medical Association, formed in 1847, has, by recent legislation, also the legal powers of a State board of health, and publishes a volume of Transactions annually. Institutions, sufficient for the care and treatment of unfortunates, have been established.

[1] In Arkansas settlements were first made about 1685; it was a part of the Louisiana purchase of 1803. In 1819 it was formed into a territory, and was admitted as an independent State in 1836. The population in 1870 was 484,471. It has no large cities, and only about one in thirty of its people live in cities and towns of over five thousand inhabitants. The State in 1870 had 1206 physicians. Its population is gradually increasing. A State Medical Association, formed in 1870, publishes annually a volume of Transactions. Institutions for the care of the insane and other unfortunates have been founded.

[2] The earliest settlement in the territory of the present State of California was in 1769, by a party of Franciscan friars who came from the Peninsula of Lower California In 1821 it became a territory of Royal Mexico, and continued a province under the republic. In 1846 the American navy seized Monterey, and from that time it has been under the control of the United States. The gold mines were discovered in 1848, population was at once led to them, and California was admitted as a State in 1850. She has been, therefore, but a little over a quarter of a century a member of the Union. Her population in 1870 was 582,031. Nearly one-third of her people live in cities of over five thousand inhabitants. She had at the date mentioned 1257 physicians. Two medical colleges have been established, and a State Medical Society, formed in 1870, publishes annually a

attract to it many able physicians. Dr. Thomas M. Logan, a native of South Carolina, in 1849 located at Sacramento. He was a close observer of the causes of disease, and a frequent contributor to the medical journals. He was influential in organizing the Medical Society of California and the State Board of Health, and was president of the latter at the time of his death. He was President of the State Medical Society in 1871, and of the American Medical Association in 1873. (b. Jan. 31, 1808; d. Feb. 13, 1876.) Dr. E. S. Cooper, a native of Ohio, was an excellent surgeon and a successful medical teacher in San Francisco. He founded and published the San Francisco Medical Press, and made valuable suggestions as to the physiological mode of repair in wounded joints and tendons. He was a careful student, a bold surgeon, and a frequent contributor to medical serial literature. (d. Oct. 13, 1862, æt. 40.) Dr. Isaac Rowell, a native of New Hampshire, enjoyed an extensive practice in San Francisco, and was particularly influential in organizing the University of the Pacific, in which he was a professor. (b. 1818; d. Jan. 4, 1871.) Dr. Wm. Burnett, of Petaluma, was a physician of culture. The following also deserve mention: Drs. Henry M. Gray and John T. Morse, of San Francisco.

Connecticut.[1]—Dr. Aneas Munson, of New Haven, served as a surgeon in the Revolutionary war. He was an observing and successful practitioner and surgeon. He contributed papers to the Connecticut Medical Society, and was an active promoter of art and science. He was one of the originators of the State Medical Society, and for some years its president. (b. June 24, 1734; d. June 16, 1826.) Dr. Jared Potter, of Wallingford, was a scholar, and had a passion for acquiring knowledge. His office was much frequented, and his instruction eagerly sought by young men entering the profession. (d. 1810, æt. 67.) Dr. Jonathan Knight, of New Haven, was remarkable for his urbanity and sound professional judgment. He held a chair in Yale College for twenty-five years. He was the first president of the American Medical Association. (b. Sept. 4, 1789; d. Aug. 25, 1864.) Dr. Wm. Tully was an earnest cultivator of the science of medicine. He was an author of merit, and published, along with Dr. Thomas Miner, in 1823, a work on fevers. (d. Feb. 28, 1859.) Dr. Thomas Miner was one of the founders

volume of Transactions. A State board of health, which also publishes annual Transactions, has been established by law. Institutions for the care of unfortunates have been created by the State, sufficient for the wants of the people.

The following medical journals have been published: California Medical Gazette, 1868; California State Medical Journal, 1856; Medical Gazette, 1870; Pacific Medical and Surgical Journal, 1858; San Francisco Medical Journal, 1858; San Francisco Medical Press, 1860; Western Lancet, 1872.

[1] Connecticut is one of the thirteen original States. The Dutch had a trading house at Hartford as early as 1631. An offshoot of Plymouth had also a settlement at Windsor about the same time. New Haven was for some years an independent colony. All the separate interests, however, were consolidated in 1665. The State took an early and an active stand in favor of American Independence. The population in 1870 was 537,454, with about two-thirds of the people living in cities and towns of over five thousand inhabitants. At the same time there were 680 physicians. Great encouragement has been given to education and the advancement of science. A State Medical Society was founded in 1792, and was one of the first in the country to publish Transactions. There is one medical college, a department of Yale. It was in this State that anæsthesia in surgical operations was first tried. The first school in America for the education of the deaf and dumb was organized at Hartford. Institutions for the care of the insane and other unfortunates have been founded by the State, sufficient for the wants of the people.

The following journal of a medical and scientific character has been published in Connecticut: American Journal of Sciences and Arts, 1846.

of the Medical Department of Yale College. He was particularly interested in the cure and treatment of the insane. His life and labors were important to his day and generation. (d. April 23, 1841.) Dr. Eli Todd, of Hartford, was an educated and intelligent physician. He was much interested in the treatment of the insane, and was chosen superintendent of the Retreat at Hartford. (d. Nov. 17, 1833.) Dr. Lemuel Hopkins, of Hartford, was a scholarly physician, writer, and wit. He was devoted to scientific studies and to the elevation of the profession. (b. June 19, 1750; d. April 14, 1801.) Dr. Eli Ives, of New Haven, was a distinguished physician and botanist. He was a man of quick perceptions, sound judgment, great industry and perseverance. For years he filled with ability a professorship in Yale College, and in 1860 was President of the American Medical Association. (b. Feb. 7, 1779; d. Oct. 8, 1861.) Dr. Mason F. Cogswell was a surgeon of the Revolution, and during his day the leading surgeon in Connecticut; he was the first person in this country to ligate the carotid artery. (b. 1761; d. Dec. 1836.) Dr. Thomas Hubbard succeeded to the chair of surgery in Yale College on the death of Dr. Nathan Smith, and was a worthy successor to one of America's most eminent surgeons. (d. June 18, 1838, æt. 63.) Dr. Worthington Hooker, of New Haven, was an accomplished physician and a writer of many educational works of merit. His brother Charles also enjoyed an enviable reputation. (b. March 3, 1806; d. Nov. 6, 1867.) I will also name Drs. Seth Bird, George Sumner, Samuel Woodward, Daniel Worden, and Horace Wells, the dentist and discoverer of anæsthesia.

DELAWARE.[1]—Dr. Edward Miller was an accomplished writer. He practised at Dover until 1796, when he removed to the city of New York. His reputation had preceded him, so that he rapidly acquired a lucrative business. In connection with Drs. E. H. Smith and S. L. Mitchell, he founded in 1797 the New York Medical Repository, the first medical journal in our country. (b. May 9, 1760; d. March 17, 1812.) Dr. James Tilton, of Wilmington, served as a surgeon in the hospitals of the Revolution, and proved himself remarkably well informed on the subject of hospital construction and management. He was a member of the Continental Congress from 1783 to 1785. During the war of 1812 he was appointed Surgeon-General of the U. S. Army. In 1813 he published a pamphlet on Military Hospitals. He was a man of clear perceptions and practical good sense, and in his official position did much to promote the efficiency of the medical department of the army. He was President of the Delaware State Medical Society. (b. June 1, 1745; d. May 14, 1822.) Dr. James Sykes was a physician of great personal and professional popularity; and was chosen to represent his district in

[1] Delaware was one of the thirteen original States. It has the smallest area of any State except Rhode Island. The earliest settlements were made by the Swedes and Finns in 1627. The Dutch of New York held the government in the year 1655, but it passed to the English in 1764. Delaware formed a part of the Pennsylvania grant of 1682, under the name of the "Three Lower Counties of Delaware." In 1701 it was permitted, in part, to have a separate government, but was subject to the Governor of Pennsylvania down to the Revolution. Delaware's population in 1870 was 125,015, about one in four of her people living in cities of over five thousand inhabitants. She had at the same time one hundred and seventy physicians. Her proximity to Philadelphia, and her position on the Delaware Bay, give her great commercial and manufacturing advantages. Her population and wealth are steadily increasing. She has a State Medical Society, formed in 1789, which publishes Transactions annually, and she has made ample provisions for the care and treatment of her insane and other unfortunates.

the State Legislature, and also in the State Senate for fifteen consecutive years, being much of the time the presiding officer. In 1801–2 he was acting Governor. He practised for a few years in the city of New York, but returned to Delaware, where he ended his days. (b. March 27, 1761; d. October 18, 1822.) Dr. James Couper, of New Castle, was a highly gifted physician. In 1840 he was a delegate from the State Medical Society to the Convention for revising the U. S. Pharmacopœia. He was President of the Delaware State Medical Society, and Vice-President of the American Medical Association in 1863. (b. Oct. 3, 1803; d. 1865.) Dr. Henry F. Askew was a physician of sound judgment and great experience. His professional life was characterized by close attention to professional duties. He had a well-trained mind, was a constant reader of the latest literature, and had a high regard for the dignity of the profession. He was President of the American Medical Association in 1867. (d. March 6, 1876.) Dr. George Munroe was a surgeon in the Revolutionary War. He practised at Wilmington, and was through life one of the foremost medical men of the State, and an occasional contributor to the "New York Medical Repository." (d. Oct. 11, 1819.) Dr. John Vaughn, a native of Pennsylvania, was a man of science and physician of ability. His manners in the sick room were gentle and assuring. In the winter of 1778–79 he delivered a course of lectures on Chemistry and Natural Philosophy. In the following year he published a "Chemical Syllabus;" he also contributed articles to the "New York Medical Repository." (d. March 25, 1807.) I will also name the following: Drs. Charles Ridegley, Allin McLane, Wm. Gibbons, Henry Latimer, Nicholas Way, John McKinley, Ebenezer Smith, Joseph Hall.

District of Columbia.[1]—Dr. Thomas Sewall, a native of Maine, was for many years extensively engaged in practice in Washington. He held the chair of Anatomy in the Medical Department of Columbia College from its organization in 1825 until 1839, and from that time to his death the chair of Pathology and Theory and Practice. His address at the opening of the school in 1825 gave a history of the Medical Colleges of the country up to that time. He was a good writer, and published a number of papers on Phrenology, Temperance, and Medicine. (d. April 10, 1845.) Dr. Tobias Watkins, a native of Maryland, was a physician, scholar, and author. He published in Baltimore, in 1809, "The Medical and Philosophical Recorder," and was for a time one of the editors of the "Portfolio." He served as a surgeon in the war of 1812, and was acting Surgeon-General from 1818 to 1821, and Fourth Auditor of the Treasury from 1824 to 1829. He translated a number of medical works from the French. (d. Nov. 14, 1855.) Dr. Thomas Henderson was a physician and surgeon of the U. S. Army, and an author. From 1825 to 1833 he was Professor of Theory and Practice in the Medical Depart-

[1] For three-quarters of a century Washington has been the seat of the government of the United States. Originally, the District or Territory of Columbia comprised a territory of ten miles square, but that part of it lying south of the Potomac River, containing thirty-six square miles, was retroceded to Virginia in 1844. The population in 1870 was 131,700. At the same time there were three hundred and twenty-six physicians. There are three medical colleges now giving instruction. A district medical society was organized in 1817, and publishes Transactions. Institutions for the care of insane and other unfortunates, sufficient for the wants of the District, have been established by the United States government.

The following Medical Journals have been published in the District of Columbia: American Botanical Register; National Med. Journal, 1870; Register and Library of Medical and Chirurgical Science, 1835.

ment of Columbia College. After his admission to the army he published "Hints on Examination of Recruits for the Army," which has gone through several editions. (b. Jan. 6, 1789; d. Aug. 11, 1854.) Dr. B. S. Bohrer, of Georgetown, was a fine scholar and an accomplished physician. He held for some years a chair in the Ohio Medical College. He possessed a fine library of medical and classical literature. He was a good practitioner, and a man of most courteous and agreeable manners. (d. Aug. 19, 1862.) Dr. Joseph Lovell, a native of Massachusetts, was Surgeon-General of the Army, and long a resident of Washington. Although not engaged in private practice, he was a member and supporter of the local medical organization, and was frequently called in consultation. (b. 1788; d. Oct. 17, 1836.) Dr. Thomas Lawson, a native of Virginia, was a surgeon in the army, and held the office of Surgeon-General. He was a man of great professional abilities and personal worth, and made a number of valuable reports on the diseases of the army and the climate of the western military posts. (d. May 15, 1861.) Dr. Frederick Dawes was a physician of fine abilities, and enjoyed for years an extensive practice. (d. Feb. 10, 1852.) To these I will add the names of Drs. Frederick May, Bailey Washington, R. K. Stone, Thomas Miller, John M. Thomas, the two Worthingtons, R. H. Coolidge, J. A. Brereton, and Joshua Riley.

Florida.[1]—Dr. Bosquet was a physician of note at St. Augustine, as early as 1800. Dr. Thomas Travers was in practice there in 1812. About the time the territory was purchased from Spain, Dr. Darcy was a leading physician. Dr. Henry Perrine was a physician, and a man of scientific attainments. The United States gave him a township of land near Biscayne Bay, for the purpose of encouraging the introduction and cultivation of tropical plants. The "Sisal Hemp" is now growing abundantly in Florida, the result of his enterprise. The Doctor was murdered at the light-house, by the Indians, during the Florida war (1841). Dr. Lewis practised as early as 1813, at Fernandina, and enjoys the distinction of having introduced vaccination into that section of country. Dr. Charles W. McCroskey was a good physician. He was connected with the army at St. Augustine, and died of yellow fever. Dr. W. H. Simmons, a native of South Carolina, lived at St. Augustine. He was a physician of skill, and a man of literary accomplishments. Dr. Richard Weightman, a native of Washington city, after serving in the army, settled on the St. John's River, and engaged for years in the practice of his profession. Dr. Seth Peck was also a physician of note in Florida. To the names already mentioned, I will add those of Drs. Lewis Willis, W. W. Waddell, Andrew Anderson, Theodore Turnbull, and William Davis.

Georgia.[2]—Dr. Lyman Hall was a well-educated physician, a member

[1] Florida was visited by Ponce De Leon in 1512, and in 1536 by De Soto. A settlement was made in 1564, the earliest by Europeans in any State of the Union. Florida belonged to Spain till 1820, when it was purchased by the United States, and admitted as an independent State in the Union in 1845. It has a large territory with almost tropical climate, but a sparse population. In 1870 it had 188,248 inhabitants. The census gave it 240 physicians. There are no large cities in the State. The United States Government maintains a navy yard, and also a military garrison. The State has a Medical Association, formed in 1874, which publishes annual Transactions. Provision has been made for the care and treatment of the insane, and other unfortunates.

[2] Georgia is one of the thirteen original States. The first settlement by the English was in 1732, the latest of any of the American Colonies. The population at the close of the Revolution did not reach 30,000. The population in 1870 was 1,184,109, with about one in every fourteen living in cities and towns of over five thousand inhabitants. It had at the

of the Continental Congress, and a signer of the Declaration of Independence. (b. 1725; d. Oct. 19, 1790.) Dr. Milton Antony, of Augusta, was a teacher and practitioner of ability. He was the founder of the Medical Academy at Augusta, in 1828, and of the Medical College of Georgia. In 1831 he started the first medical journal published in the Southern States. (b. August 7, 1789; d. Sept. 19, 1839.) Dr. Noble Wimberly Jones was a physician and a patriot. In the discharge of his professional duties he was attentive and skilful. Dr. W. C. Daniell, of Savannah, was a physician and author of note. His best known work is entitled, "Observations on the Autumnal Fevers of Savannah." In 1826 he advocated the use of capsicum internally and externally in yellow fever. (b. January, 1792; d. Dec. 28, 1868.) Dr. Richard Banks, of Savannah, and a native of Georgia, was a physician and surgeon of ability, and filled the chair of Surgery in the Savannah Medical College. (d. May, 1856, aged 62.) Dr. R. D. Arnold, of Savannah, a native of Georgia, was an accomplished physician and writer. He was one of the original members of the American Medical Association, and its Vice President in 1852. He was for many years Professor in the Savannah Medical College. He wrote and published papers of decided merit in the Transactions of the American Medical Association, and in the Medical Journals. (d. July 10, 1876, æt. 68.) Dr. W. R. Waring was for many years a leading physician of Savannah, and Professor in the Medical College. Dr. George M. Newton was a physician and successful teacher of Anatomy at Augusta. He left a fortune of about $200,000, to found an orphan asylum. (b. Jan. 30, 1810; d. Jan. 6, 1859.) In addition to the foregoing, I will name Drs. Tomlinson Fort, of Milledgeville, Thomas N. Hamilton, of Rome, G. B. Gorman, Charles W. West, Joseph H. Eve, of Augusta, Dr. Wildman, of Savannah, and Dr. Curtis B. Notingham.

Illinois.[1]—Dr. Daniel Brainard, a native of New York, was a physician and surgeon of distinction. He settled in Chicago as early as 1835, and soon acquired a leading professional business, especially in Surgery. He was one of the founders of the Rush Medical College, and long one of its

same time 1537 physicians. It has one good seaport, but no large cities. It has three medical colleges, distributed, one each, at Atlanta, Augusta, and Savannah; a State Medical Association was formed in 1849, and a State Board of Health has been organized and publishes reports. Institutions for the care of the insane and other unfortunates have been established.

The following medical journals have been published in Georgia: Atlanta Medical and Surgical Journal, 1855; Georgia Medical Companion, 1871; Georgia Medical and Surgical Encyclopædia, 1860; Oglethorpe Medical and Surgical Journal, 1861; Savannah Journal of Medicine, 1858; Southern Medical and Surgical Journal, 1837.

[1] The French had trading posts in Illinois as early as 1700. The earliest permanent settlement, however, was by the French about 1763. In 1783 this State formed a part of the "Northwestern Territory." In 1800 it was a part of the territory under the name of Indiana. Illinois was admitted as an independent State in the Union in 1818. Her population in 1870 was 2,539,891, with about one in five of her people living in cities and towns of over five thousand inhabitants. She then had 4861 physicians. Population and wealth are rapidly accumulating in this State. Three medical colleges are now giving instruction. A State Medical Society was organized in 1851, and publishes a volume of Transactions annually. Institutions for the care of unfortunates have been established, sufficient for the wants of the people.

The following medical journals have been published in Illinois: American Journal of Materia Medica, 1860; Chicago Journal of Nervous and Mental Diseases, 1874; Chicago Medical Examiner, 1860; Chicago Medical Journal, 1858; Chicago Times, 1869; Illinois and Indiana Medical and Surgical Journal, 1846; Illinois Medical and Surgical Journal, 1844; Lens, 1872; Northwestern Medical and Surgical Journal, 1849; Pharmacist, 1868; Tripod, 1871.

professors. He contributed articles of value to the Medical Journals, and to the Illinois State Medical Society's Transactions. (b. 1812; d. Oct. 10, 1866.) Dr. Wm. B. Herrick, a native of Maine, was one of the early physicians of Chicago. He served as a surgeon in the Mexican war, and was a physician of extensive experience and skill. He filled the chair of Anatomy in Rush Medical College with ability for twenty years. (d. Dec. 31, 1865.) Dr. Henry Wing settled in practice in Collinsville. He was a physician of education, and a scientist of extensive acquirements. He was one of the founders of the Chicago Medical College, and for years, held in it a professorship. His notes on the Botany of the Rocky Mountains form a part of Prof. Powell's Geological Report of that region. (b. 1822; d. Feb. 18, 1871.) Dr. Elijah D. Harmon, a native of Vermont, was one of the earliest physicians to settle in Chicago. He was physician to the Military Post of Fort Dearborn in 1832. He was a good physician and surgeon, and for his day, and the demands made upon his skill, was an efficient and successful practitioner. (b. Aug. 20, 1782; d. July 3, 1869.) Dr. J. V. Z. Blaney, a native of Delaware, was an educated and successful physician. He was one of the founders of the Rush Medical College, and the originator and publisher, in 1844, of the "Illinois Medical and Surgical Journal." (b. 1820; d. Dec. 11, 1874.) Dr. Rudolphus Rouse, a surgeon of the war of 1812, settled in practice in Peoria. He deserved, and soon attained, the first rank among physicians in that part of the State. (b. July 20, 1793; d. April 30, 1873.) The following physicians are also deserving of mention: Drs. Samuel Thompson, George W. Richards, Nicholas Hard, Harrison Noble, Stephen W. Noble, Levi T. Hewins, Josiah Goodhue, Alexander Walcott.

INDIANA.[1]—Dr. John Thomas Plummer, a native of Maryland, was a learned physician and excellent practitioner. He enjoyed in Indianapolis and its vicinity, a high reputation for skill and devotion to professional duty. In the natural sciences he was quite proficient, and was an excellent philologist; he assisted Noah Webster in collecting and defining the meaning of Western words for his unabridged Dictionary. (d. April 10, 1865, aged 58.) Dr. Isaac Casselberry, a native of Indiana, and long a leading physician in Evansville, added to skill in his profession a love of study, and good business habits. He was a successful promoter of medical organizations, and in every new enterprise worked with great intelligence and success. He was a member of the Board of Health, and a trustee of the Evansville Medical College, also President of the State Medical Society. (b. Nov. 26, 1821; d. July 9, 1873.) Dr. Charles Parry, of Indianapolis, was especially careful in diagnosis, and was a superior surgeon. He possessed the ability to make original observations, and was a writer of force. Prof. Geo. B. Wood quotes from a paper of his on pernicious fever. (d. August, 1861.) Dr. Asahel Clapp, a native of Massachusetts, practised with great success in New Albany, where he settled in 1817,

[1] Indiana was admitted to the Union in 1816. There were in it a few French settlements as early as about 1700. Immigration from the old States commenced about 1802. The population in 1870 was 1,680,637. It has no very large cities, about one in eight of its population living in cities and towns of over five thousand inhabitants. It had at the same time 3615 physicians. It has a State Medical Society, formed in 1849, which publishes annually a volume of Transactions, and has three medical colleges giving instruction. Institutions sufficient for the care of unfortunates have been established.

The following medical journals have been published in Indiana: Indiana Journal of Medicine, 1870; Indiana Journal of Medicine and Surgery, 1855; Indiana Medical Journal, 1854; Indiana Scalpel, 1860; Western Retrospect of Medicine and Surgery, 1872.

and was for forty years actively engaged in the duties of his profession. He was a good botanist, and made a report to the American Medical Association on the medicinal plants of the United States. He was fond of the study of the natural sciences, and collected a large cabinet of specimens which he presented to Yale College. Dr. John L. Richmond, a native of Massachusetts, although a well-read and intelligent physician, won his reputation by a single operation. He performed the Cæsarean section on the 23d of April, 1827, in a log cabin in the country, with no assistance except that rendered by the husband of the patient, and two women. The operation was successful, saving the mother's life. The child died. (b. April 5, 1785; d. Oct. 12, 1855.) Dr. John S. Bobbs, a native of Pennsylvania, was a successful physician and teacher of medicine in Indianapolis. He was for some years Professor of Surgery in the Indiana Central Medical College. In his will he left means for the establishment of a public Dispensary. He was President of the State Medical Society in 1868. (b. Dec. 28, 1809; d. 1870.) In addition to the above list, I will name Drs. T. Fry, E. Demming, V. Kersey, and J. W. Moody.

IOWA.[1]—Dr. David L. McGugin, a native of Pennsylvania, served as a surgeon in the Mexican war, and was for years the leading physician of Keokuk. He filled a chair in the University of Iowa at the time of his death. The State Medical Society in 1851 elected him President. As a practitioner he had no superior in the State. (b. 1807; d. June 23, 1865.) Dr. George Reeder, a native of Maryland, and a graduate of William and Mary College, Va., was a successful practitioner of medicine at Muscatine. He was one of the founders of the State Medical Society, and its President in 1854. The following also deserve mention: Drs. Ezra T. Fountain and G. W. Richards.

KENTUCKY.[2]—Dr. Benjamin W. Dudley, a native of Virginia, was an eminent surgeon and teacher at Lexington. After graduating, he spent four years in Europe perfecting himself in his professional studies. In 1817 he was elected Professor of Anatomy and Surgery in Transyl-

[1] Iowa was admitted into the Union in 1846. It was a part of the Louisiana purchase of 1803. Its earliest settlements were by the French. In 1870 it had a population of 1,194,320, with about one in ten of the population living in cities and towns of over five thousand inhabitants. It had at that time 1865 physicians. It has a rich and productive soil, and is rapidly increasing in wealth and population. Two medical colleges are giving instruction. A State Medical Society was founded in 1850, and publishes an annual volume of Transactions. Institutions sufficient for the care of the insane and other unfortunates have been established.

The following medical journal has been published in Iowa: Iowa Medical Journal, 1855.

[2] Kentucky was admitted to the Union in 1792. The first account we have of this region was by Dr. Thomas Walker, of Virginia, who visited it in 1745, and again in 1750. Dr. John Connolly had made location of a tract of land at the falls of the Ohio as early as 1770. The earliest emigrations to Kentucky were under Daniel Boone, who made a settlement at Harrodsburgh in 1774. The soil is very rich and productive; wealth and population are rapidly flowing in. The population in 1870 was 1,321,011, with about one in seven of her people living in cities and towns of over five thousand inhabitants. She had then 2414 physicians. Four medical colleges are now giving instruction in the State, and a State Medical Society, formed in 1851, publishes Transactions annually. Institutions for the care of unfortunates, demanded by enlightened humanity, have been established.

The following medical journals have been published: American Medical News, 1876; American Medical Weekly, 1874; American Practitioner, 1869; Kentucky Medical Recorder, 1853; Louisville Journal of Medicine and Surgery, 1838; Louisville Medical Journal, 1860; Louisville Medical Gazette, 1859; Louisville Review, 1856; Richmond and Louisville Medical Journal, 1868; Transylvania Medical Journal, 1849; Transylvania Journal of Medicine and Science, 1828; Western Journal of Medicine and Surgery, 1840 Western Lancet, 1842; Western and Southern Medical Recorder, 1841.

vania University. Through life he devoted himself strictly to the duties of his profession. He remained connected with Transylvania University till 1850, when he retired from practice, and lived in quiet on his farm. He was a bold surgeon and successful operator. He performed lithotomy 225 times, and did not lose a patient until after he had operated more than 100 times. He had a genius for originating new operations and devices. He contributed many articles of great value to medical journals, chiefly on surgery, but wrote no large work. (b. 1785; d. Jan. 20, 1870.) Dr. Charles W. Short, of Lexington, was a learned botanist, and an accomplished physician and teacher. He held the chair of Botany and Materia Medica in the Transylvania University. He published but little. His best known writings are his contributions to the Flora of Kentucky. Dr. Ephraim McDowell, a native of Virginia, originated the surgical operation of ovariotomy, which at first was severely condemned, but which is fully justified by the success attending the proceeding. (b. November 11, 1771; d. July 25, 1830.) Dr. Henry Miller, a native of Kentucky, was an obstetrician, writer, and teacher of ability. In 1835 he was appointed to the chair of Obstetrics in the Medical Institute at Louisville. He was the first physician in the West to use the speculum, and to practise local applications in diseases of the uterus. He was in many respects one of the most original and skilful physicians that Kentucky has produced. (b. Nov. 1, 1800; d. Feb. 8, 1874.) Dr. Samuel Brown, a native of Virginia, introduced vaccination in Lexington, as early as 1802. He was a man of decided ability, but exhibited a restless disposition, removing to New Orleans, then to Natchez, Miss., then to Huntsville, Ala., and finally returning to Lexington. He held the chair of Theory and Practice in Transylvania University. He was very entertaining as a lecturer. Contributions from his pen were published by the American Philosophical Society, and in the New York Medical Repository and other medical journals. As early as 1820 he suggested the formation of an American Medical Association. (b. Jan. 30, 1769; d. July 12, 1830.)

Dr. James Bush, of Lexington, filled with ability the chair of Surgery in Transylvania University from 1839 to the time of his death. (b. 1808; d. Feb. 9, 1875.) Dr. Wm. H. Richardson was a physician of high culture, and Professor of Obstetrics in the Transylvania University. He was a man of superior education and address, and enjoyed a large practice. Dr. Joshua Barker Flint, a native of Massachusetts, practised for some years in Boston, but removed to Louisville, where he held the chair of Surgery in the Louisville Medical College from 1837 to 1849, and, from this time to his death, the same chair in the Kentucky School of Medicine. In 1868 he published a work on Practice. He was the first surgeon in Kentucky who administered ether to produce insensibility for surgical operations, which he did in 1847. Dr. Charles Caldwell was an eminent physician, author, and teacher. As early as 1810 he filled the chair of Natural History in the University of Pennsylvania. In 1818 he was elected to the chair of Theory and Practice in the Transylvania University, which he held till 1837. He was a ready and voluminous writer. (b. May 14, 1772; d. July 9, 1853.) Dr. John Esten Cooke was a physician, author, and teacher of ability, and practised with success in the Shenandoah Valley of Virginia, and assisted in establishing the Winchester school, before removing to Kentucky. In 1827 he was elected to the chair of Theory and Practice in the Transylvania University. In connection with Prof. Short he founded the following year "The Transylvania Journal of Medicine." In 1828 he published a work

entitled "Pathology and Therapeutics." Dr. Lewis Rogers fairly won his high reputation by his intelligence and devotion to professional duties. From 1849 he filled a chair in the University of Louisville. He was President of the State Medical Society in 1873, and gave in his opening address an admirable history of medicine in Kentucky. (b. Oct. 12, 1812; d. June 17, 1875.) Dr. J. D. Jackson, of Danville, was, for his age, one of the most accomplished physicians and surgeons of Kentucky. He originated the movement for the erection of a monument to Ephraim McDowell. He performed with success many capital operations, including ovariotomy and tracheotomy. (b. Dec. 12, 1834; d. Feb. 8, 1874.) Beside the physicians named, I will mention Drs. Joseph Buchanan, Daniel L. Metcalf, Ethelbert L. Dudley, Robert Breckenridge, W. A. McDowell, James Fishback, Elisha Warfield, T. L. McLary, Walter Brashiers, Charles McCreary, and Frederick Ridgley.

LOUISIANA.[1]—Dr. Rouanet was a native of France, but in 1844 settled in New Orleans, where he soon acquired a leading position in the profession. He won high encomiums for his studies of the diseases of the heart in Paris, and contributed a number of papers to the French Academy on the physiological and pathological sounds of the heart. In New Orleans he gave office instruction to medical men and advanced students, but was not a teacher in any medical college. His practice for years was large and responsible, and he was recognized by the profession everywhere as a leading authority on diseases of the heart. (d. 1865.) Dr. Warren Stone, a native of Vermont, settled in New Orleans in 1832, and rapidly acquired business and professional reputation. In 1836 he was chosen to lecture on Anatomy in the University of Louisiana, and in 1837 was elected Professor of Anatomy and Surgery, continuing to hold this chair until he resigned in 1872. Throughout the South he was without a rival as a surgeon. In 1870 he was Vice-President of the American Medical Association. (b. 1808; d. Dec. 6, 1872.) Dr. John Hoffman Harrison, a native of Washington, D.C., was surgeon to Charity Hospital from 1833 to 1836. He originated and edited the New Orleans Medical and Surgical Journal. He contributed a number of papers on yellow fever, and wrote a work on the Nervous System. (b. Aug. 30, 1808; d. March 19, 1849.) Dr. Thomas Hunt, a native of South Carolina, settled in New Orleans in 1833. He was one of the founders of the Louisiana Medical College, was popular as a teacher, and a good writer. He was esteemed

[1] Louisiana was admitted to the Union in 1812. Settlements were made at Biloxi as early as 1699, but the earliest successful settlement was at New Orleans in 1712. Louisiana was purchased by the United States from France in 1803. It included all the territory west of the Mississippi, east of Mexico, and south of the British possessions. In 1810 the United States Census gave a population of 76,576. The population in 1870 was 726,915, with about one-fourth of her people living in cities and towns of over five thousand inhabitants. There were at this time 936 physicians. The soil of the State is alluvial and exceedingly rich, but much of it is rendered unfit for cultivation by overflow, checking agricultural production and the increase of population. The situation of New Orleans, at the mouth of the largest river in the world, gives it great commercial importance. A large and well-conducted hospital, known as "Charity Hospital," was rebuilt in 1815. Two medical colleges are now giving instruction. For many years a State Board of Health, having also cognizance of the health of the city of New Orleans, has existed, which reports to the Legislature and publishes a volume of Transactions annually. There is no State medical society, though medical organizations exist in the large cities.

The following medical journals have been published: New Orleans Journal of Medicine, 1868; New Orleans Medical Journal, 1844; New Orleans Medical News and Hospital Gazette, 1854; New Orleans Medical Record, 1866; New Orleans Medical and Surgical Journal, 1844; New Orleans Medical Times, 1861; New Orleans Monthly Medical Register, 1851; Union Medicale de la Louisiane, 1852.

specially successful in the treatment of yellow fever. (b. May 18, 1808; d. March 20, 1867.) Dr. Edward Barton, a native of Virginia, practised with success in New Orleans. He filled the chair of Materia Medica and Therapeutics in the University of Louisiana from 1835 to 1840. His writings on Meteorology, on Hygiene, and on Yellow Fever, show much research and professional experience. In addition I will name Drs. John Leonard Riddell, the inventor of the binocular microscope, J. Jones, Wm. M. Carpenter, Charles A. Luzenburg, and Frank Hawthorn.

MAINE.[1]—Dr. Stephen Cummings, of Portland, was a leading physician in extensive practice as early as 1800, and continued actively engaged in his profession for over 50 years. (b. 1773; d. 1854.) Dr. James C. Bradbury, a native of Maine, settled in practice at Oldtown, and for 35 years enjoyed the confidence of the community. Although not a surgeon, he performed such operations as became necessary in his practice, and on Oct. 11, 1851, amputated at the hip-joint with success. This was the fourth operation of the kind in America. (b. 1806; d. Oct. 3, 1865.) Dr. Amos Nourse, a native of New Hampshire, a physician of Belfast, Maine, and a surgeon of ability, although advanced in years, served in the late war as surgeon of the 20th Maine Regiment. He had been honored by the Presidency of the State Medical Society. Dr. Job Holmes, a native of Maine, settled in Paris, where he enjoyed a large practice. In a few years he removed to Calais, where he passed the remainder of his life in the active duties of his profession. Dr. Hosea Rich, a native of Massachusetts, was a practitioner of note in Bangor. His professional career began as early as 1805, and was continued till 1865. During the war of 1812, he was the surgeon of the 4th Maine Regiment. I will also name Dr. James Parker, who was a physician of ability, and in active practice for over 50 years. He served two terms in Congress.

MARYLAND.[2]—Dr. John Beale Davidge was a fine classical scholar and an extensive reader of the early masters in medicine. As early as 1798, he published an essay on autumnal and intermittent fevers. He was conspicuous and influential in the founding of the Medical Department

[1] Maine was admitted into the Union in 1820. From its settlement, in 1607, it was a part of Massachusetts, and remained under her control till admitted as an independent State. The population in 1870 was 626,915, about one-third of the people living in cities and towns of over five thousand inhabitants. The number of physicians was 818. One medical college, a department of Bowdoin College, is giving instruction. There is a State Medical Society, formed in 1834, that publishes Transactions annually. Institutions for the care of unfortunates have been founded, sufficient for the wants of the people.

The following medical journals have been published in Maine: Journal of the Medical Society of Maine, 1834; Maine Medical and Surgical Reporter, 1858.

[2] Maryland is one of the thirteen original States. Its earliest settlement was in 1634. The population in 1870 was 780,892, one-third of her people living in cities and towns of over five thousand inhabitants. The number of physicians was 1251. The State has great advantages for ocean commerce, owing to its situation on the Chesapeake Bay. It is rapidly increasing in wealth and population. There are three medical colleges giving instruction. Institutions for the relief of the insane and other unfortunates, have been established. There are also a State Medical Society, formed in 1789 under the name of The Medical and Chirurgical Faculty of Maryland, and a State Board of Health.

The following medical journals have been published in Maryland: Baltimore Journal of Medicine, 1861; Baltimore Medical Journal, 1870; Baltimore Medical and Philosophical Lyceum, 1811; Baltimore Medical and Physical Recorder, 1808; Baltimore Medical and Surgical Journal, 1833; Baltimore Monthly Journal of Medicine and Surgery, 1830; Baltimore Philosophical Journal and Review, 1823; Baltimore Physician and Surgeon, 1872; Maryland Medical Recorder, 1829; Medical Bulletin, 1868; North American Archives of Medical and Surgical Science, 1834; Vaccine Inquirer, 1822.

of the University of Maryland. In 1813, he published a new classification and nomenclature of diseases, in Latin. From 1814 to 1816, he published a work in parts, entitled "Physical Sketches," and was a contributor to Medical Journals. (d. 1829, æt. 60.) Dr. Horatio Jameson, of Baltimore, was a native of Maryland, and Professor of Surgery in the University of Maryland. In 1829 he started and edited with ability the Maryland Medical Recorder, which he continued until 1832. One of his best known works is on cholera. He published a pamphlet on the parts concerned in lithotomy. He was fond of the study of the natural sciences, and was a good geologist. In 1830, he attended a meeting of naturalists and physicians, in Hamburg, Germany. Dr. Nathaniel Potter was one of the original founders of, and filled for over thirty years, with distinguished ability, a chair in, the University of Maryland. His writings show much erudition as well as original observation. In 1805, he published a paper on the medical and deleterious properties of arsenic. The best known work from his pen is one on contagion and yellow fever, published in 1817. (b. 1770; d. July 2, 1843.) Dr. Tristram Thomas was a physician of large practice and great influence, on the Eastern Shore of Maryland. He was actively engaged in practice for over 50 years, and was an ardent lover of his profession, studious and observing, and an occasional contributor to the medical journals. His best known paper was one on Bilious and Remittent Fevers, which is referred to favorably in Eberle's Practice. (b. Dec. 25, 1769; d. Aug. 5, 1847.)

Drs. John and Thomas H. Buckler, brothers, were men of fine intellect, and good practitioners. For many years, they held the very front rank as skilful and able physicians. Dr. Philip Thomas, a native of Maryland, practised in Frederick, from 1769 to 1815. He attended the second course of lectures delivered in Philadelphia, under Morgan and Shippen. He was a good scholar, a man of correct habits, and of large influence in the profession of the State. (b. June 11, 1747; d. April 25, 1815.) Dr. John Archer, a native of Maryland, was noted as having introduced the use of senega in croup, and enjoyed the distinction of having been the first person that received a diploma from an American Medical College, which he did in Philadelphia, in 1768. He served in the State Legislature, and as a member of Congress, from 1801 to 1807. I will also mention Drs. Charles Frick, Samuel Baker, Frederick Dorsey, Ennals Martin, Miles Littlejohn, John Tyler, the two Murrays, of Annapolis, Samuel K. Jennings, John Baltzell, Gustavus Brown, Elisha Butts, and R. S. Stewart.

MASSACHUSETTS.[1]—Dr. Edward Augustus Holyoke, a native of Massa-

[1] Massachusetts was one of the thirteen original States, and the first to raise troops and to inaugurate the measures that led to armed resistance to the claims of Great Britain, and to American Independence. Settlements were made in her territory as early as 1620. She soon after began a system of public education, which has been developed and adopted in all the States. Her population in 1870 was 1,457,351, nearly one-half of her people living in cities of over five thousand inhabitants. She had 2047 physicians. One medical college, a department of Harvard University, has existed since 1782. Institutions on the most approved plans, for the care of unfortunates of all classes, have been established on a liberal scale. A State Medical Society, formed in 1781, publishes Transactions, and a systematic registration of vital statistics has been in successful operation for many years.

The following medical journals have been published in Massachusetts: Berkshire Medical Journal, 1861; Boston Journal of Chemistry, 1866; Boston Medical Intelligencer, 1823; Boston Medical and Surgical Journal, 1828; Journal of the Gynæcological Society, 1869; Medical and Agricultural Register, 1806; Medical Magazine, 1832; Monthly Journal of Medical Literature, 1832; New England Journal of Medicine and Surgery and Collateral Sciences, 1812; Worcester Journal of Medicine, 1845.

chusetts, was a man of learning and accurate observation, and an industrious and skilful practitioner. He died at the age of 100 years and 8 months. He was one of the principal founders of the Massachusetts Medical Society, and its first President. He published Astronomical observations, and articles in the Transactions of the Massachusetts Medical Society, and in the New York Medical Repository. (b. Aug. 1, 1728 (O. S.); d. 1829.) Dr. James Lloyd was the first surgeon in America to use ligatures, instead of searing wounds with the actual cautery, and to use the double flap in amputation. He also performed lithotomy. For nearly 60 years, he was the great physician and surgeon of New England. He was a warm advocate of inoculation for the small-pox. (b. April, 1728; d. March, 1810.) Dr. John Warren acquired much reputation as a hospital surgeon in the Revolutionary army. He was the head of a family of eminent physicians and surgeons of Massachusetts, and the brother of the patriot, Dr. and Gen. Joseph Warren. Dr. Warren was the earliest systematic teacher of Anatomy in Massachusetts, and the founder of the Medical Department of Harvard University, in which he held a chair up to the time of his death. He was President of the Massachusetts Medical Society, and a member of nearly all the learned societies of his time. He published observations on the "Mercurial practice," and contributed articles to the Transactions of the Massachusetts Medical Society, and of the American Academy of Arts and Sciences. (b. July 27, 1753; d. April 4, 1815.) Dr. J. Collins Warren, his son, was not inferior to his father. He was the first surgeon to use ether as an anæsthetic in surgical operations, and aided much to promote its use. He was also an author and teacher of great ability. (b. Aug. 1, 1778; d. May 4, 1815.) Dr. Benjamin Waterhouse, a native of Rhode Island, was, in 1799, the introducer of vaccination in America. He was a zealous and able defender of the practice of vaccination, and published two works upon the subject. He held the chair of Theory and Practice of Medicine, in Harvard College, from 1783 to 1812. He was a man of very active intellect and varied information, fond of study, and a voluminous writer. (b. March 4, 1754; d. Oct. 2, 1846.)

Dr. James Jackson, a native of Massachusetts, commenced practice in the year 1800. In 1810, with Warren and others, he initiated measures which led to the establishment of the McLean Asylum for the Insane, and also for the founding of the Massachusetts General Hospital. He was physician to the latter institution until 1835. In 1810, he was appointed clinical lecturer to Harvard College, and in 1812, to the chair of Theory and Practice. His contributions to the literature of medicine were numerous and valuable. (b. Oct. 3, 1777; d. Aug. 26, 1867.) Dr. John Ware, a native of Massachusetts, was from 1832 to 1858 Professor of Theory and Practice in Harvard College. He was a most admirable teacher, a close reasoner and original observer, and a beautiful and correct writer. (b. Aug. 1, 1778; d. May 4, 1854.) Dr. James Thacher, a native of Massachusetts, settled in practice in Plymouth, after having served as a surgeon in the Revolutionary army. During his military life, he kept a journal, which he subsequently published, and which has become a most valuable contribution to the history of the times. He was the author of the work entitled "The New Dispensatory," and of a number of other publications. His best known work is entitled "American Medical Biography," and is a great storehouse for information pertaining to the early profession in America. (b. 1754; d. May 24, 1844.) Dr. Elisha Bartlett, a native of Rhode Island, was a leading physician,

and the first Mayor of the city of Lowell. He was a close student and fine classical scholar. He was a sufferer from lead poisoning, but held professorships in quite a number of colleges. Although he wrote numerous works, his most popular one is on the fevers of the United States, published in 1842. (b. 1805; d. 1855.) Dr. Luther V. Bell, a native of New Hampshire, gained the Boylston Prize when about 30 years of age. By his ability in the treatment of the insane, he attracted the special attention of his contemporaries, and was placed in charge of the McLean Asylum, which continued under his control from 1837 to 1856. In 1857, he was President of the Massachusetts Medical Society. He wrote on many subjects of interest to the medical profession. During the late war, he went out as a surgeon and lost his life in the service of his country. (b. Dec. 1806; d. 1862.)

The following also deserve mention: Drs. John Brooks, Jeffries Wyman, H. H. Childs, A. A. Gould, George Hayward, Oliver Prescott, Lemuel Danforth, S. G. Howe, G. Darby, A. L. Peirson, C. T. Jackson, T. W. Harris, Enoch Hale, Winslow Lewis, and W. T. G. Morton, dentist and discoverer of the use of ether as an anæsthetic.

Michigan.[1]—Dr. Zina Pitcher, a native of New York, entered the army in 1822 as an assistant surgeon; was promoted to be full surgeon in 1832, and resigned in 1836, when he settled in Detroit. He was not only a good physician and surgeon, but an energetic and successful business man. He was mayor of the city, a faithful and intelligent manager of the school fund, and one of the founders of the State Insane Asylum. In 1856 he was honored with the presidency of the American Medical Association. (b. April 12, 1797; d. April 5, 1872.) Dr. Richard Inglis was a good classical scholar, full of information and energy, cheerful in the sick-room, and an agreeable companion in society. With the profession of the State he was exceedingly popular, and in 1869 was elected President of the State Medical Society. Mention should also be made of Drs. E. M. Clark and Porter, of Detroit; Robinson, of Palmyra; Robert Clark, of Monroe; Isaac C. Paddock, of Pontiac, and Denton and Brigham, of Ann Arbor.

Minnesota.[2]—Dr. Samuel Willey, a native of Massachusetts, after

[1] Michigan was admitted as a State in the Union in 1837. The earliest settlements within her territory were by the French, in the vicinity of Detroit, in the latter part of the seventeenth century. The lands are generally good and heavily timbered. The population as given by the census in 1870 was 1,187,234, about one-sixth of the people residing in towns and cities of over five thousand inhabitants. The same authority gives the number of physicians as 1034. Population and wealth have rapidly flowed into this State. From land grants by the United States a large educational fund has been established both for general education and colleges of a high grade. There are two medical colleges, a State Medical Society, formed in 1819, and publishing Transactions, and an efficient State Board of Health.

The following medical journals have been published in Michigan: Detroit Review of Medicine and Pharmacy, 1866; Michigan University Medical Journal, 1870; Peninsular Independent Medical Journal. 1858; Peninsular Journal of Medicine, 1873; Peninsular Journal of Medicine and Collateral Sciences, 1858.

[2] Minnesota was a part of the Louisiana purchase. This region was visited as early as 1650, the French having had posts in different parts along the Lakes. The United States government established, in 1819, a military post at Fort Snelling. In 1845 settlements began to be made, and immigration set in actively, and in 1859 a territorial form of government was organized. In 1853 Minnesota was admitted into the Union. The population as given by the United States Census of 1870 was 446,056, with about one-tenth living in towns of over five thousand inhabitants. There were then in the State 402 physicians. The population and wealth of Minnesota are steadily increasing. The State is fertile and well timbered, and is well situated as to commercial lines, by lakes, rivers, and railroads. A

graduating was for two years assistant physician in the State Lunatic Asylum, at Columbus, Ohio. In 1852 he settled in practice in St. Paul, and soon acquired a full business. In the fall of 1862 he was appointed pension surgeon and medical referee for the State. The organization of the State Medical Society was largely due to his efforts, as was that of the State Board of Health. He was twice elected president of the State Society, and in 1870 was one of the Vice-Presidents of the American Medical Association. (b. 1828; d. Nov. 21, 1872.) Dr. E. Herman Smith, a native of Connecticut, after serving a year as surgeon on the French steamer "Arago," settled at St. Paul, and soon acquired a fair business. He was accurate in diagnosis, and an expert in the use of the microscope. Dr. H. C. Hand, a native of New Jersey, settled in St. Paul in 1871, and formed a business relation with his brother, Dr. D. W. Hand, then in practice in that place. In 1872 he became editor and publisher of the Northwestern Medical and Surgical Journal, which he conducted with ability. He was one of the physicians to the St. Paul's City Hospital. (b. March 17, 1849; d. March 2, 1876.) Dr. Josiah S. Weiser, a native of Pennsylvania, located in 1855 at Shakopee, a settlement chiefly made up of Germans. He was induced to join Gen. Sibley's military expedition, as surgeon, in June, 1863, and was murdered by the Indians while holding a parley with them. The following deserve mention: Drs. N. B. Hill, A. E. Ames, J. K. Reiner, J. C. Jones, J. D. Ford, L. B. Smith, and G. T. Turner.

MISSISSIPPI.[1]—Dr. Samuel A. Cartwright, a native of Virginia, settled at Natchez, Miss., and was particularly successful in the treatment of fever, and of cholera, during the epidemic of 1832. He wrote a number of valuable papers. (b. Nov. 30, 1793; d. May 3, 1863.) Dr. Erasmus D. Fenner, a native of North Carolina, settled in 1832 at Clinton, where his ability obtained for him a good practice. In 1840 he removed to New Orleans, and in 1844, in connection with Dr. A. Heister, established the New Orleans Medical Journal. He was one of the original members of the American Medical Association, and a Professor in the New Orleans School of Medicine. In 1849 he commenced the publication of the Southern Medical Reports, which extended to two volumes. He also edited the New Orleans Medical News and Hospital Gazette. (b. 1807; d. May 4, 1866.) Dr. Allen Gillespie, a native of Tennessee, after graduating in medicine, served as an assistant surgeon in the army of the Southwest. In 1824 he removed to Mississippi, and in 1834 settled at Granada, where he spent his life in the active duties of his profession. He possessed in a high degree the qualities that go to make the successful physician. It is claimed that to him is due the credit of introducing the practice of giving large doses of quinia as an anti-pyretic in miasmatic fevers. After treating fevers in

State Medical Society, organized in 1855, publishes Transactions, and a State Board of Health is also in successful operation.

The following medical journal has been published in Minnesota: Northwestern Medical and Surgical Journal, 1870.

[1] Mississippi was admitted into the Union in 1817, having been erected into a territory with Alabama in 1798. It had been visited by De Soto in 1540, and by La Salle in 1682, but the first successful settlement by Europeans was at Natchez, in 1716, under Bienville. According to the United States Census in 1870 the population was 827,922, about one-tenth of the people living in cities of over five thousand inhabitants. There were at the same time 1511 physicians. The lands are generally low, alluvial, and subject along the water-courses to overflow. A State Medical Association, organized in 1856, publishes Transactions annually, and institutions for the care of unfortunates have been established.

this way for years, he made his views public in 1835. The use of quinia in large doses has been recommended by others, but only during the remission. (b. Aug. 18, 1801; d. 1869.) In addition to the above-named, I will mention Drs. Samuel B. Malone and Elvis McCrory.

MISSOURI.[1]—Dr. Wm. Beaumont, a native of Connecticut, entered the army as assistant surgeon in July, 1812, and resigned in 1839, and settled in the city of St. Louis. In 1825, while stationed at the military post Michilimackinac, he met the Canadian, St. Martin, who had received a wound which left an opening into the stomach, exposing in a measure the operation of that organ. The Doctor made a series of experiments upon the process of digestion, which he published in 1838. This is one of the most complete series of observations ever made on the living stomach. (b. 1785; d. April 25, 1853.) Dr. Charles A. Pope, a native of Alabama, settled in St. Louis in 1841. He was studious, energetic, and ambitious in his profession, and soon acquired a leading practice. He was elected to the chair of Anatomy and then to that of Surgery in the St. Louis University. His preference in practice was for surgery, and in this he was very successful. He was a fluent lecturer, and familiar with the history of the various surgical operations and procedures. He was surgeon to the St. Louis City Hospital. He identified himself with all movements calculated to advance the medical profession, and was President of the American Medical Association in 1854. (b. March 15, 1818; d. July 6, 1872.) Dr. M. L. Linton, a native of Kentucky, settled in practice in St. Louis in 1842. He was elected to the chair of Obstetrics in the St. Louis Medical College, and in 1844, was transferred to the chair of Theory and Practice. In 1843 he founded and edited the St. Louis Medical and Surgical Journal, the first medical periodical published west of the Mississippi River. He possessed high intellectual endowments and scientific culture, and wrote well, and on a great variety of subjects—medical, literary, religious, and scientific. He was a member of the Academy of Sciences, and contributed many papers to it. He had an extensive practice, and was widely known in the Southwest. (b. 1808; d. June 1, 1872.) Dr. B. F. Shumard, a native of Pennsylvania, commenced to practise in Kentucky, but became connected with one of the U. S. Government Pacific R. R. surveys and explorations. His scientific acquirements led him to be much employed in such pursuits. He removed to St. Louis in 1853. In 1860 he was engaged in a geological survey of the State of Texas, which was interrupted by the war, when he returned to St. Louis, and resumed the duties of his profession. He was elected to the chair of Obstetrics in the University of Missouri. He was President of the Academy of Sciences at the time of his death. (b. Nov. 24, 1820;

[1] Missouri, a part of the Louisiana purchase of 1803, was admitted as a State in the Union in 1821. Its earliest settlers were the French, who built a fort on the present site of the city of St. Louis as early as 1719. Another point of early settlement was at St. Genevieve. The population of Missouri in 1870 was 1,721,295, about one-fifth of the people living in towns and cities of over five thousand inhabitants. There were then in the State 3560 physicians. Missouri has a large school fund, and a successful system of free schools. There are four medical colleges. A State Medical Society, formed in 1850, publishes Transactions. Institutions for the care of unfortunates, sufficient for the wants of the people, have been established.

The following medical journals have been published in Missouri: Humboldt Medical Archives, 1867; Kansas City Medical Journal, 1871; Kansas City Medical and Surgical Review, 1860; St. Joseph Journal of Medicine and Surgery, 1859; St. Louis Medical Reporter, 1866; St. Louis Medical and Surgical Journal, 1845; St. Louis Probe, 1850.

d. April 14, 1868.) Dr. Lewis F. Linn, a native of Kentucky, settled in Genevieve in 1819. He served in the war of 1812, and after this took an active part in public affairs. He was a fluent and eloquent speaker. In 1827 he was elected to the Legislature, and in 1833 to the U. S. Senate, which office he held to the time of his death. (b. 1795; d. 1843.) I will also mention Dr. John H. Watters, of St. Louis, who was one of the earliest to write upon the theory of the Correlation and Conservation of Forces, Drs. J. N. McDowell, John A. Chambers, and John Edwards.

New Hampshire.[1]—Dr. Nathan Smith, a native of Massachusetts, first practised in Cornish, New Hampshire. He suggested the establishment of a Medical Department in connection with Dartmouth College, and in 1797 gave there a course of lectures. In 1798 he was appointed Professor in the Medical Department. He taught all the branches, and furnished all the apparatus needed in teaching until 1810, when he was joined by another professor. In a few years, by his genius and indomitable perseverance, fair classes attended this institution. In 1813 he was invited to the chair of Surgery in Yale College, which he accepted. He continued, however, to lecture at Dartmouth, at the Vermont University, and at Bowdoin College. His reputation as a surgeon had by this time become national. The credit of having first pointed out that dislocations of the hip and other joints should be reduced by manipulation, is due to Dr. Smith. In 1824 he published a particularly valuable essay on typhoid fever, in which he made a distinction between typhoid and malarial fevers. (b. 1762; d. 1829.) Dr. Amos Twitchell, a native of New Hampshire, was a close observer, and an indefatigable student. It seems to be well authenticated that he was the first to place a ligature on the common carotid artery, which he did six months before the occurrence of the case reported by Sir Astley Cooper. I can discover no publications of Dr. Twitchell, but he was in the habit of taking part in the discussions of the New Hampshire Medical Society. (b. 1781; d. 1850.) Dr. Josiah Bartlett, a native of Massachusetts, commenced to practise in Kingston, N. H., in 1750. His intelligence, high moral character, and ability, soon brought him a full share of professional business. From 1765 to the time of the Revolution he was annually chosen to the Legislature, and was a Lieutenant-Colonel in the State militia. In 1775 he was elected to Congress, and in 1776 was the first on the roll call to vote for the Declaration of Independence. He served as a Justice of the Supreme Court of the State, and was one of the originators and first President of the New Hampshire Medical Society. (b. 1729; d. 1795.)

Dr. Ammi R. Cutter, a native of Maine, served as surgeon in the Revolutionary war, and part of the time was Physician General to the Middle Department of the army. After the war he settled at Portsmouth, where he acquired a large practice, and was greatly esteemed throughout the State. For many years he was president of the State Medical Society. (b. 1735; d. 1820.) Dr. Matthew Thornton was a native of Ireland, and settled at Londonderry. He was a surgeon in the expedition in 1745 against Louisburg. In 1775 he was president of

[1] New Hampshire was one of the thirteen original States; its first settlement having been near Portsmouth, in 1623. Its population in 1870 was 318.300, nearly one-fifth living in cities and towns of over five thousand inhabitants. The number of physicians was 565. There is a State Medical Society, formed in 1791, which publishes Transactions annually, and one medical college. There are also institutions for the care of unfortunates, sufficient for the wants of the people.

the convention which assumed the government of New Hampshire in the name of the people. He was a delegate to Congress, and signed the Declaration of Independence, though not present to vote for it. (d. June 24, 1803, aged 88.) Dr. Lyman Spalding, a native of New Hampshire, began practice at Portsmouth in 1799. He was a good anatomist and a learned physician. He published in 1799 a new nomenclature of chemistry. In 1813 he removed to the city of New York. He conceived the idea of publishing a United States Pharmacopœia, and for years advocated the project, and called the convention which formed it in 1820, being also one of the committee of publication. (b. June 5, 1775; d. Oct. 31, 1821.) Dr. Dixi Crosby, a native of New Hampshire, commenced practice at Gilmanton, where he acquired a large business, and established his reputation as a surgeon. In 1836 he removed the scapula, humerus, and two-thirds of the clavicle at one operation, the patient recovering. In 1838 he was elected to the chair of Surgery and Obstetrics, in Dartmouth College, which position he held till 1868, when he resigned the branch of Surgery in favor of his son, lecturing for two years longer on Obstetrics. He contributed a number of able articles to the medical journals, and was President of the New Hampshire State Medical Society, and a member of the American Medical Association. (b. Feb. 9, 1800; d. Sept. 26, 1873.) I will also name Drs. Mathias Spaulding, Josiah Crosby, Charles P. Gage, W. Graves, Daniel Oliver, George B. Twitchell, and A. Smalley.

New Jersey.[1]—Dr. William Burnett, a native of New Jersey, settled in practice at Newark, and, during the Revolution, took a conspicuous part in the events of the day. He was a member of the "Committee of Safety," in 1776, and was one of the commissioners for issuing State bills of credit. He was physician of the hospitals, and Chief Surgeon of the State troops; and was a member of Congress from 1780 to 1781. In the latter year he was again elected to Congress, but resigned, and was appointed Chief Physician and Surgeon in the Eastern Department, serving till the close of the war. After peace was established he returned to private practice, and engaged to some extent in agriculture. He was Judge of the Court of Common Pleas and President of the State Medical Society, and throughout a long life was greatly beloved and esteemed. (b. 1730; d. 1791.) Dr. Moses Scott was a native of Pennsylvania, and on the outbreak of the Revolutionary war, took part with the patriots. He had been in military service with Braddock's expedition. In July, 1776, he was appointed physician and surgeon of the State troops, and in 1777, when Congress took entire control of the hospital department, was appointed Assistant Director General. He was present at all the battles fought in New Jersey. After the war he was esteemed one of the most experienced and learned physicians in the State. (b. 1738; d. 1821.) Dr. Jonathan Elmer, a native of New Jersey, was a surgeon in the

[1] New Jersey is one of the thirteen original States. The Dutch made two settlements at Bergen between 1614 and 1624, and a Swedish colony settled near the Delaware River in 1627. Some of the most stirring events of the Revolutionary war took place within the territory of New Jersey. Her position between the two great cities of New York and Philadelphia gives her great commercial advantages. Her population, as given by the census of 1870, was 906,096, a little over one-third of her people living in cities of over five thousand inhabitants. She had at the same time 1208 physicians. She has a State Medical Society, organized in 1766, which publishes Transactions annually. She has also a number of institutions for the care of unfortunates.

The following medical journal has been published in New Jersey: New Jersey Medical Reporter, 1847.

Revolutionary war, as was his brother Ebenezer. He was a man of extensive information, with great power of detail, and executive ability. He was elected to Congress in 1776, serving till 1778; again in 1781, serving till 1784; again in 1787, serving till 1788, and was United States Senator from 1789 until 1791. He was a member of the American Philosophical Society, and held many offices of trust in his native State. (b. 1745; d. 1817.) Dr. Nicholas Belleville, a native of France, settled in Trenton in 1791. He had a very extensive practice, and was physician to Joseph Bonaparte, ex-King of Spain, while he resided in New Jersey. His office was the resort of many students who subsequently rose to eminence in their profession. Dr. John N. Woodhull, a native of New Jersey, was a man of superior education. He settled at Princeton, and enjoyed the reputation through life of being a most skilful physician. He endowed a professorship in Princeton College. His whole life reflects honor upon himself, as a man, as well as upon the medical profession. (b. 1752; d. 1831.) Dr. L. A. Smith, a native of New Hampshire, was a physician of superior ability. He settled at Newark, and had the esteem of the profession, and was popular with the community. He was one of the early movers for, and an able advocate of, the establishment of the New Jersey Lunatic Asylum. He was devoted to his profession, and was an earnest promoter of medical organizations. He was President of the New Jersey State Medical Society, in 1837, and throughout his life was an active worker for the advancement of medicine and the elevation of the profession. (b. Nov. 11, 1795; d. Dec. 15, 1865.) The following names are also deserving of mention: Drs. John Beatty, W. Johnson, Isaac Ogden, Samuel Hayes, J. Ward, Isaac P. Coleman, Silas Condit, Isaac Pierson, and J. Lilley.

New York.[1]—Dr. Valentine Mott, a native of New York, and an

[1] New York is one of the thirteen original States. Its earliest known mention was in 1609, from the visit of Henry Hudson, who gave his name to the Hudson River. This is now the most populous and wealthy State in the Union. The population in 1870 was 4,387,464, nearly one-half living in cities of over five thousand inhabitants. The number of physicians in the same year was 6810. New York has nine medical colleges, distributed as follows, viz.: Albany, Brooklyn, Buffalo, and Syracuse, each one, and New York City five. A State Medical Society was organized in 1807, and has published Transactions almost continuously since. Institutions for the care and treatment of unfortunates have been provided in different parts of the State, sufficient for the wants of the people.

The following medical journals have been published in New York: Albany Journal of Neurology, 1843; American Chemist, 1870; American Journal of Indigenous Materia Medica and Repertory of Med. Science, 1860; American Journal of Insanity, 1844; American Journal of Medicine, 1852; American Journal of Obstetrics and Diseases of Women and Children, 1868; American Journal of Ophthalmology, 1862; American Journal of Syphilography and Dermatology, 1870; American Lancet, 1831; American Medical Monthly, 1854; American Medical and Philosophical Register, 1810; American Medical and Surgical Journal, 1851; American Medical Times, 1860; Annalist, 1847; Archives of Ophthalmology and Otology, 1869; Archives of Scientific and Practical Medicine, 1873; Buffalo Medical Journal, 1845; Buffalo Med. and Surgical Reporter, 1861; Medical Gazette, 1867; Medical Record, 1866; Medical and Surgical Register, 1818; Nelson's American Lancet; New York Journal of Medicine, 1839; New York Journal of Medicine and Collateral Sciences, 1843; New York Journal of Pharmacy, 1852; New York Lancet, 1842; New York Medical Gazette, 1841; New York Medical Gazette and Journal of Health, 1850; New York Medical Independent and Pharmaceutical Reporter, 1864; New York Medical Inquirer and Domestic Magazine, 1830; New York Medical Journal, 1831; New York Medical Journal, 1865; New York Medical Magazine, 1814; New York Medical and Pathological Journal, 1858; New York Medical and Philosophical Journal and Review, 1809; New York Medical and Physical Journal, 1822; New York Medical Press, 1860; New York Medical Repository, 1797; New York Medical Review, 1873; New York Medical and Surgical Reporter, 1845; New York Medical Times, 1851; New York Medico-Chirurgical Bulletin, 1851; New York

eminent surgeon and teacher, settled in New York City in 1809, and was appointed to the chair of Surgery in Columbia College. He was elected to fill the same chair in Rutgers College, and also in the College of Physicians and Surgeons. In 1818 he passed a ligature around the brachio-cephalic artery, about two inches above the heart, for aneurism. He excised the entire clavicle, and was the first in America to ligate the iliac artery, and to remove the lower jaw. Sir Astley Cooper says of him, that he performed more of the great operations than any other surgeon, living or dead. It would be impossible to enumerate here all his great achievements. In 1814, in connection with Dr. Onderdonk, he started and published the "New York Medical Magazine," which only extended to two parts. He translated Velpeau's work on Surgery, and contributed articles to the medical journals, and to the Transactions of the New York Academy of Medicine. (b. Aug. 20, 1785; d. April 26, 1865.) Dr. Samuel Bard commenced practice in 1767 in the city of New York. He was one of the projectors of the first Medical College organized in New York, and held the chair of Theory and Practice. He acted as examining surgeon for the admission of surgeons to the Medical Department of the Army during the Revolutionary war. In 1774, he was influential in founding the New York Hospital, in which he gave a course of clinical lectures. In 1791, when the public hospital was opened, he was appointed one of the visiting physicians. In 1798, he had retired to his country seat, but on the outbreak of the yellow fever returned to the city, and resumed practice. He was finally taken down with the fever, but recovered. He was General Washington's physician, when in New York, after the war. He published a compendium of Midwifery, and contributed to the Philosophical Transactions and to medical journals. (b. April 1, 1742; d. May 24, 1821.)

Dr. David Hosack, a native of New York, settled in practice in the city of New York in 1794. The following year he was appointed Professor of Botany in Columbia College, and shortly afterwards published a syllabus of the lectures. In 1796, he formed a partnership with Dr. Bard, which continued till 1800. In 1797 he was elected to the chair of Materia Medica, but in 1811, on the re-organization of the Faculty, was assigned to the chair of Theory and Practice, and afterwards held that of Obstetrics and Diseases of Women and Children, until 1826. He was visiting physician to the Almshouse and to the Insane Asylum. He wrote on fevers, and other medical topics, and was elected a member of the Royal Society of London. His paper on the causes of contagion was able, and received a wide appreciation. In 1810, with Dr. Francis, he started the American Medical and Philosophical Register, which was conducted with ability. (b. August 31, 1769; d. Dec. 22, 1835.) Dr. Theodoric Romeyn Beck, a native of New York, began practice in Albany in 1811. In 1813 he wrote a paper on the minerals of the United States. In 1815 he was appointed Lecturer on the Institutes of Medicine, and Professor of Medical Jurisprudence in the College of Physicians and Surgeons. He also held a professorship in the Fairfield Medical College from 1826 to 1836. In 1836 he was elected to the chair of Materia

Monthly Chronicle of Med. and Surg., 1824; New York Monthly Review of Med. Science and Buffalo Medical Journal; New York Register of Medicine and Pharmacy, 1850; North American Medical Reporter, 1858; Northern Lancet and Gazette of Legal Medicine, 1850; Opal, 1851; Quarterly Journal of Psychological Medicine, 1867; Sanitarian, 1873; Syracuse Medical and Surgical Journal, 1854; United States Medical and Surgical Journal, 1834.

Medica in the Albany Medical College. From 1840 to 1850 he was President of the Albany Academy, and was the originator of the Albany Institute. He was President of the New York State Medical Society, and was one of the managers of the State Lunatic Asylum. In 1823 he published the first edition of his great work on "Medical Jurisprudence." He edited for some years the "American Journal of Insanity." (b. Aug. 11, 1791; d. Nov. 19, 1855.) Dr. J. W. Francis, a native of New York, commenced to practise in 1811, and formed a partnership with Dr. Hosack. They together published the American Philosophical Register. In 1813 he was lecturer in the Institute of Medicine, and Professor of Materia Medica in the College of Physicians and Surgeons. About this time he visited Europe, and on his return was elected to the chair of Medical Jurisprudence, and in 1819 filled that of Obstetrics, in Rutgers College. In 1822 he was one of the editors of the New York Medical and Physical Journal. He was an accomplished writer, and took an active part in promoting the objects of the New York Academy of Medicine, the Historical Society, the Woman's Hospital, etc. (b. Nov. 17, 1789; d. Feb. 8, 1861.)

Dr. A. H. Stevens, a native of New York, commenced practice in the city of New York in 1812. In 1814, he was invited to the chair of Surgery in Queen's, afterwards Rutgers College. He was one of the visiting surgeons to the New York Hospital. In 1817, he was assigned to a chair in the College of Physicians and Surgeons. In 1848, he was President of the New York State Medical Society, and also President of the American Medical Association. His general as well as his surgical practice was very large. His contributions to the literature of medicine are found in Journals, Reports, and Addresses. (b. 1789; d. March 30, 1869.) Dr. Wright Post, a native of New York, commenced practice in 1786. The chair of Surgery in Columbia College was tendered him in 1792, but in 1793 he was transferred to the chair of Anatomy, which he continued to hold for twenty years. He was also a member of the Faculty of the College of Physicians and Surgeons, and its president from 1821 to 1826. He was for thirty years consulting physician of the New York Hospital. He was a bold, original, and successful operator. He tied the subclavian artery, and also the carotid. (b. Feb. 19, 1766; d. June 14, 1828.) Dr. John Torrey, a native of New York, although a learned physician, never acquired a large practice, and being greatly interested in botany and the collateral sciences, his time became occupied by them. In 1817, he published a catalogue of plants growing within thirty miles of New York city, and in 1831 published the first volume of the Flora of North America. In 1824 he was appointed Professor of Botany in the Military Academy at West Point, and from 1827 to 1850 was Professor of Chemistry in the College of Physicians and Surgeons of New York. He was subsequently appointed director of the New York Assay Office. The Doctor was one of the most accomplished botanists of our country, and did much to elevate the science by his life and teachings. (b. 1798; d. March 10, 1873.)

Dr. Samuel L. Mitchell, a native of New York, commenced practice in New York city, and, with Dr. E. Miller and Dr. E. H. Smith, started, in 1797, the "New York Medical Repository." He was connected with it as editor for sixteen years, and survived both of his associates. He was a member of the State Legislature, and for a number of years a member of Congress, and also U. S. Senator. He was a man of very extensive scientific acquirements, and of a remarkably retentive memory. He

wrote on a variety of topics. making substantial additions to scientific literature. (b. Aug. 20, 1764; d. Sept. 7, 1831.) Dr. Joseph M. Smith, a native of New York, commenced practice in 1811. He was one of the original members of the Medico-Philosophical Society, which published its first volume in 1817. He was Professor of Theory and Practice in the College of Physicians and Surgeons for thirty years. His most noted work is perhaps his "Elements of Etiology, and Philosophy of Epidemics." He was a frequent and valued contributor to medical serial literature. (b. March 14, 1789; d. April 22, 1866.) As of almost equal merit, I will name Drs. John Watson, J. Kearney Rodgers, J. Stearns, Joseph White, Richard Bayley, W. W. Reid, John A. Smith, Charles A. Lee, Alden March, John B. Beck, James Stuart, Elisha Smith, H. D. Bulkley, John A. Swett, Amasa Trowbridge, Amariah Brigham, George T. Elliot, James McNaughton, H. Green, J. H. Armsby, Alban Goldsmith, R. S. Kissam, and H. M. Onderdonk.

North Carolina.[1]—Dr. Charles Harris, a native of North Carolina, served in the Revolutionary war before he had completed his medical education. He first practised with success at Salisbury, but removed to Favonia, the name of his plantation in Cabaries County, where he remained actively engaged in practice to the close of his life. Ninety-three physicians studied with him, his reputation being such as to make him a desirable preceptor. His professional life extended over forty years. (b. 1763; d. Sept. 21, 1825.) Dr. James Norcom, a native of North Carolina, commenced practice at Edenton in 1799. He acquired a large but laborious business, often visiting patients at a distance of 100 miles, on horseback. His practice was chiefly that of a physician, although performing such surgical operations as usually fall to the lot of rural practitioners. His obstetrical practice was large. In 1812, he was commissioned Assistant Surgeon in the U. S. Army, but resigned in January, 1813. He wrote a paper on the Winter Epidemic of 1816, and made a number of contributions to medical journals. (b. 1788; d. Nov. 9, 1850.) Dr. James Dickson, a native of North Carolina, having practised for a couple of years in South Washington, removed to Fayetteville, where his practice became large. In 1835, he performed successfully the operation of transfusion of blood. In 1828, 1831, and 1833, he performed urethrotomy. In 1835, he divided the tendo Achillis, for the relief of club-foot. About this time he ligated the external iliac artery. He was everywhere recognized as a skilful and successful surgeon. Dr. Benjamin Robinson, a native of Vermont, commenced practice in his native place, but removed to North Carolina in 1804, and settled at Fayetteville, where he spent the remainder of his life, laboriously engaged in

[1] North Carolina is one of the thirteen original States. Sir Walter Raleigh attempted to establish a colony in it in 1585, but failed. The earliest point successfully settled was on the Roanoke and Chowan Rivers, by emigrants from Virginia, in 1653. In 1693, North and South Carolina were separated. This State took an early and active part to secure American Independence. Ephraim Brevard, at a meeting of the citizens at Charlotte, on May 20, 1775, prepared what is known as the "Mecklenburg Declaration of Independence." This embodied many of the sentiments of the Declaration adopted more than a year later, at Philadelphia. The population of the State, as given by the census of 1870, was 1,071,362. There are no large cities or towns, and but about one in forty of the population live in towns of over five thousand inhabitants. There were, in 1870, 1143 physicians. A medical college has been projected, but is not yet organized. There is a State Medical Society, formed in 1850, which publishes Transactions. Institutions for the care of unfortunates, sufficient for the wants of the people, have been provided.

The following medical journal has been published: Medical Journal of North Carolina, 1858.

his profession. While a student of medicine, he performed an operation for the relief of strangulated hernia, with success. He was distinguished through life for his untiring assiduity and sound common sense. His memory is still cherished by the descendants of the community in which he passed his useful life. (d. March 8, 1857, aged 82.) Dr. Charles E. Johnson, a native of North Carolina, settled in practice at Edenton, and thence removed to Raleigh in 1835. He was especially interested in nervous diseases and physiological studies. He wrote a paper on the Medico-Legal Relations of Insanity. (b. March 15, 1812; d. April 1, 1876.) Dr. Armand John De Rosset was a native of North Carolina, and a graduate of Princeton College. His immediate ancestors, father, grandfather, and great grandfather, were physicians. The duration of his practice extended to 69 years. He was a good classical scholar, and a cultivator of the sciences, which he applied to the practical and every-day requirements of his profession. (d. April 1, 1859, æt. 92.) I will also mention Drs. James J. Philips, of Edgecombe Co.; James Furgus McRee, of Wilmington; R. H. Shield, of Winton; Wm. H. McKee, of Raleigh; Wm. Perry, of Franklin Co.; Elisha Battle, of Edgecombe Co.; Henry M. Shaw, of Kurrituck Co.; Joel B. Houston, of Columbia; Matthias E. Sayer, of Edenton; Simon J. Baker, of Raleigh; and Pinckney Caldwell.

OHIO.[1]—Dr. Daniel Drake, a native of New Jersey, but reared in Kentucky, began to practise in Cincinnati in 1804. He rapidly rose to be recognized as a leading physician, and as one of very extensive knowledge and scientific attainments. In 1810 he published "Notes" concerning Cincinnati, and in 1815 a work entitled "A Picture of Cincinnati." In 1817 he was elected a professor in the Transylvania University, at Lexington, Ky. The following year he obtained a charter and founded the Medical College of Ohio, at Cincinnati, and in 1819 he established the Commercial Hospital. From 1827 to 1839 he edited the "Western Journal of American Medical Sciences." Its publication was transferred to Louisville, Ky., and the name changed to "Western Journal of Medicine and Surgery." He continued as one of the editors till 1848. In 1832 he published an essay on Medical Education and Medical Men of the United States, and the same year a pamphlet on Cholera. His great work, which contained the observations of nearly half a century, was published in 1850, entitled "Diseases of the Interior Valley of

[1] No European settlement prior to the Revolution is known to have been made in Ohio. The first emigration to this State was in 1788, from New England to a point known as Marietta, on the Ohio River, it having been included in 1781 in the general territorial legislation pertaining to the territory northwest of that stream. The population in 1870, as given by the U. S. Census, was 2,605,260, nearly one-fourth of the population living in towns of over five thousand inhabitants. There were at that time 4638 physicians. Population and wealth are still rapidly accumulating in this State. It has seven medical colleges, three at Cincinnati, two at Cleveland, and two at Columbus. A State Medical Society was established in 1846, and publishes reports annually. Institutions for the care of unfortunates have been provided, sufficient for the wants of the people.

The following medical journals have been published in Ohio: Annals of Science, 1853; Cincinnati Journal of Health, 1844; Cincinnati Journal of Medicine, 1869; Cincinnati Lancet and Observer, 1858; Cincinnati Medical News, 1858; Cincinnati Medical News, 1872; Cincinnati Medical Observer, 1856; Cincinnati Medical Repertory, 1868; Cincinnati Medical and Surgical News, 1860; Cleveland Medical Gazette, 1859; Clinic, 1871; Columbus Review of Medicine and Surgery, 1860; Critic, 1853; Northern Ohio Medical and Scientific Examiner, 1848; Ohio Medical Recorder, 1876; Ohio Medical Repository of Original and Selected Essays and Intelligence, 1826; Ohio Medical and Surgical Journal, 1848; Ditto, 1876; Western Journal of Medical and Physical Sciences, 1828; Western Journal of Medicine, 1840; Western Lancet, 1842; Western Medical Gazette, 1832; Western Quarterly Reporter of Medical, Surgical, and Natural Science, 1822.

North America." A second volume of this series, in preparation at the time of his death, was published subsequently. This great work deserves to be better known. (b. Oct. 20, 1785; d. Nov. 6, 1852.) Dr. Reuben D. Mussey, a native of New Hampshire, commenced practice in Essex Co., Massachusetts, in 1805, after having received the degree of Bachelor of Medicine from Dartmouth College. In 1807 he attended lectures at the University of Pennsylvania, and wrote a thesis on Cutaneous Absorption, based upon original observations, his experiments proving certain facts, while an opposite doctrine was being taught in the colleges. He made many other experiments upon this subject, which have not been published. Fresh from the college at Philadelphia, he settled at Salem, Mass., where he obtained a good practice, and where he resided for six years, devoting much time to the surgery of the eye. In 1814 he was elected to the chair of Theory and Practice in Dartmouth College, and in 1819 to the chair of Anatomy and Surgery in the same institution, holding the latter position till 1838. He also gave lectures on chemistry at Middlebury College, and at Fairfield College, New York. In 1829 the doctor visited Paris, to study medical institutions abroad. During the year 1837 he was tendered three professorships, at Nashville, New York, and Cincinnati, and accepted the latter. His fame as a surgeon preceded him, and in a few years he was one of the best known surgeons in the Valley of the Mississippi. He possessed much mechanical skill, and had a genius for original operations. He performed many capital and heroic operations, tying on the same patient within twelve days both carotid arteries, and saving his patient's life. His whole professional life was one of eminent success, and sheds lustre upon the profession. He was President of the American Medical Association in 1850. (b. 1780; d. 1866.)

Dr. John Delemater, a native of New York, commenced practice in 1809, in the city of New York. He subsequently practised in Pittsfield, Mass., and was connected with the Medical College there. In 1843 he removed to Cleveland, Ohio, and was elected to the chair of General Pathology and Midwifery in the Western Reserve College at that place. (d. April, 1867, aged 80.) Dr. George Mendenhall, a native of Pennsylvania, commenced practice in Cleveland, but in 1843 removed to Cincinnati, where by great devotion to his profession he acquired a large business. In 1852 he was elected to the chair of Obstetrics in the Miami Medical College, and, on the consolidation of that college with the Medical College of Ohio, was re-elected and continued to serve till his death. He published a "Vade Mecum," which passed through several editions. He was one of the editors of the "Western Lancet," and afterwards of the "Lancet and Observer." The journals contained many articles from his pen. He was President of the American Medical Association in 1870. (b. 1814; d. 1874.) Dr. Jesse P. Judkins, besides having a large practice, was Demonstrator of Anatomy in the Ohio Medical College, and Professor of Anatomy in Starling Medical College, at Columbus. Afterwards he held the chair of Special Pathology in the Miami Medical College, filling the latter position with ability up to the time of his death. He wrote a number of excellent papers which were published in the Cincinnati Medical Journal. (d. Dec. 6, 1867, aged 52.) Dr. Horace Ackley settled in practice at Akron. He was elected professor in Willoughby College in 1837. In 1843 he was elected Professor of Surgery in the College then at Cleveland. He was an early and efficient member of the Ohio State Medical Society, and for some years its president. He

was physician to the Marine Hospital at Cleveland, and one of the trustees for the Central and Southern Lunatic Asylums. His surgical practice was for years quite large, and he was esteemed a safe and skilful operator. (d. April 22, 1859, æt. 47.)

Dr. Leonidas M. Lawson, a native of Kentucky, first practised in Madison, and started the "Western Lancet;" and in 1844, commenced the reprint of Hooper's Pathological Anatomy. The same year he was invited to a chair in the Transylvania University. In 1845 he visited England and France, to study medical institutions abroad. In 1847 he was offered the chair of Materia Medica and Special Pathology in the Medical College of Ohio. He also at this time held chairs in the Kentucky School of Medicine and in the University of Louisiana. (d. Jan. 21, 1864, æt. 51.) Dr. John Dawson, a native of Virginia, after practising for some time in Green County, removed in 1851 to Columbus, and in 1853 was elected to the chair of Anatomy in the Starling Medical College. As a teacher he gave great satisfaction to his associates and to the students. About this time he became editor of the Ohio Medical and Surgical Journal. He was an original observer and forcible writer. (d. Sept. 1866, æt. 55.) Dr. G. W. Boerstler, a native of Maryland, settled in practice in Lancaster, 1853. All the sympathies and all the energies of his nature were directed to the discharge of his duties as a physician. His practice was very large. (d. Oct. 19, 1871, æt. 78.) Dr. George C. Blackman, a native of Connecticut, commenced to practise in the State of New York, where he acquired reputation as a surgeon. In 1854 he was elected to the chair of Surgery in the Medical College of Ohio. He was surgeon to the Cincinnati Hospital, and also to the Good Samaritan Hospital. On the breaking out of the war he entered the medical service of the army as a Surgeon of Volunteers. He was a man of fine presence, and had an air of resolution. (d. July 19, 1871.) I will name in addition, Drs. Edward Tiffin, John P. Harrison, John Butterfield, G. F. Mitchell, R. L. Howard, Daniel Tilden, Philip J. Buckner, John T. Shotwell, J. W. Russell, and S. M. Smith.

PENNSYLVANIA.[1]—Dr. Benjamin Rush commenced practice in Philadelphia in 1769, and the same year was elected to the chair of Chemistry

[1] Pennsylvania is one of the thirteen original States, and was settled by the benevolent William Penn in 1682. This State took an active part in the measures that ushered in the struggle for American Independence, and was, during the war, the chief seat of Congress. On her territory were fought several of the important battles of the Revolution. The population in 1870, according to the U. S. Census, was 3,522,050, nearly one-third living in towns of over five thousand inhabitants. The number of physicians at the same time was 4843. There are three medical colleges, all in Philadelphia. There is a State Medical Society, formed in 1848, which publishes a volume annually. Institutions for the insane and other unfortunates have been provided in different parts of the State. Pennsylvania has from the settlement of our country taken the lead in matters relating to medical publication and medical education, and, as a consequence, we find, as would be expected, a larger proportion than usual of eminent medical men resident within her borders.

The following medical journals have been published: Æsculapian Register, 1824; American Journal of the Medical Sciences, 1827; American Journal of Pharmacy, 1825; American Library and Intelligencer, 1836; American Medical Intelligencer, 1837; American Medical Recorder, 1818; Barrington & Haswell's Medical Bulletin, 1849; Bulletin of Medical Science, 1843; Cholera Gazette, 1832; Compendium of Medical Science, 1868; Hospital Register, 1863; Journal of the Philadelphia College of Medicine, 1829; Medical Examiner, 1838; Medical Reporter, 1853; Medical Review and Analectic Journal, 1826; Medical and Surgical Reporter, 1856; Medical Times, 1870; North American Medical and Surgical Journal, 1826; Philadelphia Journal of Medicine and Physical Sciences, 1820; Philadelphia Medical Museum, 1805; Philadelphia Medical and Physical Journal, 1804; Philadelphia Medical and Surgical Journal, 1853; Philadelphia Monthly Journal of Medicine and Surgery, 1828.

in the Philadelphia College. In 1787 he was assigned to the chair of Theory and Practice. He possessed a remarkably well trained and observing mind, and, in addition to his professional duties, took an active part in all public affairs. He was for many years physician to the Pennsylvania Hospital and other benevolent institutions. In the State convention he moved that the colony should express its sentiments on the subject of separation from the mother country. He was shortly afterwards elected to Congress, and signed the Declaration of Independence. His writings embrace many topics in medicine, and are everywhere characterized by originality and ability. His conduct through life as a citizen and physician was such as to make him a marked man. (b. Dec. 24, 1745; d. April 19, 1813.) Dr. Philip Syng Physick, a native of Pennsylvania, commenced to practise in Philadelphia in 1793, after having spent a number of years in visiting the hospitals of Europe. This was the year of the great yellow fever epidemic in Philadelphia. Dr. Physick was placed in charge of Bush Hill Hospital, and discharged his duties with marked ability. In 1794 he was appointed one of the surgeons of the Pennsylvania Hospital, and from 1801 to 1816 he was one of the consulting surgeons of the Philadelphia Almshouse Infirmary. In 1815 he was appointed Professor of Surgery in the University of Pennsylvania, in which he afterwards filled the chair of Anatomy. In 1825 he was elected a member of the Royal Academy of Medicine of France, and in 1836 a member of the Royal Medical and Chirurgical Society of London. (b. 1768; d. Dec. 15, 1837.) Dr. John Morgan, a native of Pennsylvania, after serving as surgeon to the Militia in the French war in 1759, resigned in 1760, and went to Europe to further perfect his knowledge of medical science. He returned to Philadelphia in 1765, and was then perhaps the most learned physician in America. The same year he delivered an address at the commencement of the Philadelphia College, and the opening of the first medical college in America, established through his own and Dr. William Shippen's influence, and in which he was appointed Professor of Theory and Practice. In 1775 Dr. Morgan was appointed Director General and Physician in Chief of the Hospital Department of the American Army, but political cabals and the exigencies of the times caused his removal in 1777. A report by Congress shows him to have been an efficient officer. In his thesis he proposed the theory that pus was formed by a secretory process. He was one of the founders of the American Philosophical Society, and a member of the Royal Philosophical Society of London. (b. 1735; d. Oct. 15, 1789.)

Dr. Benjamin Smith Barton, a native of Pennsylvania, settled in practice in Philadelphia in 1789. He was the same year appointed Professor of Natural History and Botany, and in 1813 Professor of Materia Medica in the University of Pennsylvania. He contributed papers to the American Philosophical Society, and in 1804, started the "Medical and Physical Journal." He was an indefatigable worker, and all his writings were practical contributions to medicine. (b. Feb. 10, 1766; d. Dec. 19, 1815.) Dr. Caspar Wistar, a native of Pennsylvania, commenced practice in Philadelphia in 1787. In 1789 he was elected Professor of Chemistry and Physiology in the University of Pennsylvania. In 1792 he was elected physician to the Philadelphia Dispensary, and to the Pennsylvania Hospital, and the same year was appointed Adjunct Professor of Anatomy, which position he filled until the time of his death. His abilities as a physician secured him a large practice, and assisted in popularizing the school. He was a member of

many learned societies, and President of the American Philosophical Society. His best known work is his "Anatomy," in two volumes, the first of its kind contributed by an American. (b. Sept. 13, 1761; d. July 22, 1818.) Dr. Wm. P. Dewees, a native of Pennsylvania, commenced practice in the interior of the State, but removed to Philadelphia in 1793. He devoted his time chiefly to Obstetrics, but, his health failing in 1812, he retired to his farm at Phillipsburg, where he remained for five years, when, having recovered, he returned and resumed practice, which he continued with increasing popularity. In 1825 he was chosen assistant professor of Midwifery in the University of Pennsylvania, and in 1834 was elected to the full chair. But his health had gradually failed, and the following year he resigned and was elected emeritus professor. (b. May 5, 1768; d. May 20, 1841.) Dr. Nathaniel Chapman, a native of Virginia, commenced practice in Philadelphia in 1804. By his assiduity in professional duties, and by a fortunate marriage, he speedily acquired a large business. About this time he commenced a course of lectures on Obstetrics to medical students, and in 1808 associated himself with Professor James in a summer course of lectures. From 1813 to 1816 he was Professor of Materia Medica in the University of Pennsylvania, and from 1816 to 1850, Professor of Theory and Practice and of Clinical Medicine. In 1817 he founded the Medical Institute, and lectured in it during every summer for twenty-five years. He was President of the American Philosophical Society from 1846 to 1848, and President of the American Medical Association in 1847. (b. May 28, 1780; d. Jan. 1, 1853.) Dr. Wm. E. Horner, a native of Virginia, having resigned his commission in the U. S. Navy, commenced practice in the city of Philadelphia. His abilities soon attracted the attention of the leading physicians, and he shortly after received the appointment of Demonstrator of Anatomy in the University of Pennsylvania. In 1819 he was made adjunct, and in 1831 full Professor of Anatomy. In 1824 he discovered the "Musculus Horneri," which had escaped the observation of previous anatomists. His whole life was a continuous struggle of mind and duty over physical suffering. In 1847 he founded St. Joseph's Hospital, to which he bequeathed his library and instruments. About this time he published a treatise on Pathological Anatomy, which was shortly afterwards followed by other important publications. (b. Jan. 3, 1793; d. March 13, 1853.)

Dr. John K. Mitchell, a native of Virginia, settled in practice in Philadelphia in 1822, and soon acquired a large practice. After taking his medical degree he made three voyages to China as surgeon in a merchant ship. In 1824 he delivered a course of lectures on the Institutes of Medicine and Pathology before the Philadelphia Institute, and in 1826 was appointed to the chair of Chemistry and Applied Arts in the Franklin Institute. He was a ready writer, and frequently contributed to the medical journals. In 1841 he was elected to the chair of Theory and Practice in Jefferson Medical College, a position he filled with rare ability till the time of his death. He was a gifted lecturer, and occasionally introduced experiments with good effect, as he was an expert manipulator. His writings were numerous, and always contained important additions to medical knowledge. (b. March 12, 1796; d. April 4, 1858.) Dr. George McClellan, a native of Connecticut, shortly after graduation commenced practice in the city of Philadelphia. In 1825, with a few able associates, he founded the Jefferson Medical College, in which he was a professor till 1838. In 1839 he organized the Medical

Department of Pennsylvania College, from which he retired in 1844, and devoted his time to private practice. He was a fluent and agreeable lecturer. For a time he edited a medical journal in connection with Dr. John Eberle, and was a frequent contributor to the columns of periodical medical literature. (b. Feb. 22, 1796; d. May 9, 1847.) Dr. Robley Dunglison, a native of Scotland, was invited to the United States in 1824, and elected Professor of Medicine in the University of Virginia, where he continued till 1833. From 1833 to 1836 he was Professor of Materia Medica and Therapeutics in the University of Maryland, and from 1836 to 1868 was Professor of the Institutes of Medicine and Medical Jurisprudence in the Jefferson Medical College. His Medical Dictionary is by far the most convenient work of the kind known to the profession. (b. June 4, 1798; d. April 1, 1869.) Dr. T. D. Mütter commenced practice in Philadelphia about 1831, and shortly afterwards established a "Quiz class," and soon became a teacher of surgery in what was known as "Chapman's Medical Institute." He was made Adjunct Professor of Operative Surgery in Jefferson Medical College to assist Dr. Randolph, and on the resignation of the latter, was elected to the full chair of Surgery. It was in this field that he acquired his great reputation as a teacher and as an operator in rhinoplastic surgery. He has left an enduring monument to his name in the establishment of the "Mütter lectureship" and the "Mütter Museum" of the College of Physicians. Besides the names already given, I will mention Drs. William Shippen, Samuel Jackson, Franklin Bache, T. S. James, P. S. Dorsey, H. L. Hodge, C. D. Meigs, J. D. Godman, Adam Kuhn, J. Redman Coxe, Robert Hare, Samuel G. Morton, Réné La Roche, W. Darlington, John S. Parry, Samuel Jackson (of Northumberland), Peter Mowry, Nathaniel Bedford, James Agnew, Joseph Gazzam, B. R. Reese, W. W. Gerhard, Jacob Randolph, David Gilbert, G. W. Norris, Thomas T. Harrison, Joseph Parrish, Thomas Harris, and P. B. Goddard.

RHODE ISLAND.[1]—Dr. Isaac Senter, a native of New Hampshire, served as a surgeon in the Revolutionary war, and in 1779 settled in practice in Pawtucket. In 1780 he was appointed Surgeon and Physician-General to the State troops, and removed to Newport, where he passed the remainder of his life actively engaged in the practice of his profession. He contributed articles on medical subjects to the journals, and was an honorary member of the Massachusetts Medical Society, of the Medical Society of London, and of the College of Physicians of Philadelphia. He was for some years President of the Rhode Island State Medical Society. (b. 1735; d. Dec. 20, 1799.) Dr. Pardon Bowen, a native of Rhode Island, served during the Revolutionary war as a surgeon on a privateer, and in 1779 was taken prisoner, and after being exchanged re-entered the service. He settled in practice in his native town, and gradually acquired a large business. He was an active and efficient member of the Rhode Island State Medical Society, and for some years its president. He was also a member of the Board of Trustees of Brown University. His profes-

[1] Rhode Island is one of the thirteen original States. Its first settlement was in 1636 by Roger Williams, at the present site of Providence. Other settlements were soon made at Newport and at Warwick. The population in 1870, according to the United States Census, was 317,353. Nearly one-half of the people live in cities and towns of over five thousand inhabitants. The number of physicians in 1870 was 260. One medical college, a department of Brown University, is giving instruction. Rhode Island has a State Medical Society, formed in 1812, which publishes Transactions annually, and a good system of registration of vital statistics. Institutions for the care of the insane and other unfortunates have been established, sufficient for the wants of the people.

sional life extended to nearly half a century. (b. 1757; d. Oct. 25, 1826.) Dr. Usher Parsons, a native of Maine, became on graduating a surgeon in the navy during the war of 1812, and was in the battle of Lake Erie. After ten years' service he resigned, and settled in practice in Providence. He held a professorship in Brown, Jefferson, and other medical colleges, and was President of the Rhode Island Medical Society, and Vice-President of the American Medical Association. He wrote a number of medical works, and took four Boylston prizes. He was an influential promoter of the establishment of the Rhode Island Hospital, and a most agreeable and witty writer. (b. Aug. 18, 1788; d. Dec. 19, 1868.)

Dr. Solomon Drowne, a native of Rhode Island, served as a surgeon's mate in the Revolutionary war, and on the restoration of peace settled in Providence. In 1788 he joined a party going to the new settlement in the West, at Marietta, on the Ohio River. He attended, during his last illness, General Varnum, one of the Ohio Land Company, and, at the request of the community, pronounced his funeral eulogy. He resided for a time at Morgantown, Va., and for seven years at Uniontown, Fayette County, Pa. Here he delivered four orations commemorative of American independence, and a eulogy on General Washington. In 1801 he returned to Providence and engaged in private practice. In 1811 he was Professor of Materia Medica and Botany in Brown University. The Rhode Island Medical Society, in 1819, appointed him a delegate to the convention that formed the National Pharmacopœia. He possessed a fine classical education, and was a beautiful writer and an interesting lecturer. (b. March 11, 1753; d. Feb. 5, 1834.) Dr. Wm. Bowen studied medicine with his father, then a leading physician, and speedily acquired a large practice for himself. He avoided surgery, whenever practicable, and was deemed specially skilful in diseases of women and children. His instruction was much sought by pupils who were preparing to enter the medical profession. His reputation in the treatment of fevers was unequalled by any physician in his section. (d. 1832, æt. 86.) Dr. Caleb Fiske, a native of Rhode Island, served as a surgeon in the Revolutionary war, then settled in practice in Scituate, and passed a long and successful professional life, residing all the time in the house in which he had been born. He was one of the original members of the Rhode Island Medical Society, and in 1823 its president. At one time he was Judge of the Court of Common Pleas. He has perpetuated his name by endowing the Fiske fund for the promotion of medicine. (d. Sept. 1835, æt. 82.) To the names already mentioned I will add those of Drs. Levi Wheaton, Joseph Mauran, L. L. Miller, and David King—the first to introduce vaccination in Rhode Island.

South Carolina.[1]—Dr. James Moultrie, a native of South Carolina,

[1] South Carolina was one of the thirteen original States. The first settlement was at Port Royal by the English, in 1670. The government was a proprietary one till 1719, when South Carolina became a colony under the crown. During the Revolution the State was very active, and suffered much from the depredations of the British troops. The population in 1870, according to the U. S. Census, was 705,606, with about one in thirteen living in towns and cities of over five thousand inhabitants. The number of physicians was 798. There are two medical colleges, one at Charleston, and the other at Columbia. A State Medical Society was formed in 1789, and a State Medical Association in 1848; the latter publishes annually a volume of Transactions. Institutions for the care of unfortunates have been established.

The following medical journals have been published in South Carolina: Carolina Journal of Medicine, Surgery, and Agriculture, 1825; Charleston Medical Journal and Review, 1848; Charleston Medical Journal and Review, 1873; Southern Journal of Medicine and Pharmacy, 1846.

commenced practice in Charleston in 1813, and was soon afterwards appointed physician to the fort and jail. He took an active part in public health matters, and was soon recognized as the leading sanitarian of the city. In 1819 he was elected by the Medical Society of South Carolina a delegate to the first convention for forming the National Pharmacopœia. In 1822, he, with other influential medical men, memorialized the Legislature for a charter to establish a medical college, which was granted in 1823. He was elected to a chair in the college, but declined. In 1832, however, he accepted the chair of Physiology, and here developed the high powers of his well-stored and vigorous mind. He was President of the American Medical Association in 1851, and had previously been President of the Medical Society of South Carolina. (b. 1793; d. 1869.) Dr. Tucker Harris, a native of South Carolina, commenced practice in Charleston in 1771, in partnership with his preceptor Dr. Lionel Chalmers, and served as a hospital surgeon in the Revolution. Immediately after the restoration of peace, he resumed his practice. From 1783 to 1786 he had a business partnership with Dr. Oliphant. He was one of the earliest members, and an ardent friend, of the Medical Society of South Carolina, and from 1796 to 1799 was chosen its president. (b. 1747; d. July 6, 1821, æt. 74.) Dr. J. L. E. W. Shecut, a native of South Carolina, commenced practice in Charleston in 1791. He was attentive to business and a close student. In 1806 he exhibited an ingenious electrical machine of his own construction, and the same year published a work entitled "Flora Caroliniana." His publications were quite numerous. (b. 1770; d. 1836.) Dr. Alexander Baron, a native of Scotland, settled in practice in the city of Charleston in 1769. His fund of general information was great, and his ability equal to any position in professional or civil life. He was one of the founders of the South Carolina Medical Society, and its vice-president in 1790. In 1770 he joined the St. Andrew's Society, and was shortly after elected its president, an office which he held for twenty-eight years. (d. July 9, 1819.) Dr. J. Edwards Holbrook, a native of South Carolina, established himself in Charleston in 1822. In 1824 he was elected Professor of Anatomy in the Medical College of South Carolina. He was universally recognized as a learned scientist, and as a popular and successful physician. He avoided surgery, and was particularly noted for his sympathy for suffering patients. He was a successful medical teacher, and particularly popular with his class. Although contributing valuable articles to medical journals, his greatest works, and those on which his reputation as an author will rest, are his "American Herpetology" and "The Fishes of South Carolina." (b. Dec. 30, 1794; d. Sept. 8, 1871.)

Dr. John Bellinger, a native of South Carolina, commenced practice in Charleston, where his eminent abilities soon carried him, through all difficulties, to a full recognition of his professional abilities. His high reputation pointed him out as a proper occupant for the chair of Surgery in the Charleston Medical College, to which he was elected. His skill in surgery was only equalled by his acuteness as a physician. He was a thorough pathologist, and, although a busy practitioner, kept pace with the progress of the profession, and with the latest and best literature. (d. Aug. 13, 1860, æt. 56.)

Dr. Samuel H. Dickson, the physician, author, and teacher, commenced practice in Charleston in 1820. In 1823 he delivered a course of lectures on Physiology and Pathology before a medical class. He was influential in the establishment of the medical college at Charleston, and, in 1824,

was called to the chair of Institutes and Practice of Medicine. He was Professor of the Practice of Medicine in the University of New York from 1847 to 1850, when he returned to Charleston; but in 1858 was elected to the chair of Practice of Medicine in the Jefferson Medical College at Philadelphia, which he held nearly to the time of his death. He contributed many able papers to medical journals, and wrote a work entitled "Manual of Pathology," etc. (b. 1798; d. 1872.) Dr. B. B. Simons was a native of South Carolina. In 1801 he commenced practice in Charleston, and soon acquired business and reputation. His abilities were recognized by his appointment as Lecturer on Chemistry in the College of South Carolina. He was a thorough anatomist, and of rare skill and boldness in surgery, in which he acquired an extensive reputation and practice. (b. 1766; d. 1844.) Dr. Richard Evans Wylie, a native of Charleston, settled in 1832 in Lancaster Co., where he acquired distinction in his profession. He was a fine anatomist, and made many *post-mortem* examinations, and was a good pathologist. He was of a charitable and generous disposition, and was well versed in medical literature. (b. 1810; d. 1875.) Dr. Robert Wilson Gibbes was assistant and afterwards Professor of Chemistry, Geology, and Mineralogy in the South Carolina College, which position he filled with ability. He commenced practice in Columbia, where he acquired a good professional business. He was for several years sent as a delegate to the American Medical Association, and was President of the Medical Society of South Carolina. During the late war he was Surgeon-General of the State troops, and was the originator of the "wayside hospital system." Dr. Gibbes was an ardent student of the sciences, and the author of several valuable contributions to the Academy of Sciences and to the Smithsonian Institution. He was the author of Gibbes's History of South Carolina, and made a large collection of autographs, coins, and specimens in palæontology, geology, etc., as also a collection of the fossils of South Carolina. He was a member of the Royal Societies of England, France, Germany, and Denmark. (b. 1809; d. 1866.) I will add the names of Dr. David Ramsay, Dr. Geddings, and Dr. Peter Fayssoux.

TENNESSEE.[1]—Dr. Wm. H. Deadrick, a native of Virginia, having acquired his professional education, settled in East Tennessee. His operation for the removal of the inferior maxillary bone, in 1810, and other capital operations performed at that early date, have won for him an enduring name in the literature of the profession. (b. 1785; d. 1857.)

[1] Tennessee was the first State west of the Alleghany Mountains, settled by emigrants from the colonies, anterior to the Revolutionary war. The settlers were chiefly from North Carolina, to which State the territory belonged. Fort Loudon, in West Tennessee, was built as early as 1757. An attempt was made to found a State out of this territory under the name of "Franklin," but after three or four years' trial the effort was abandoned. Tennessee was admitted as a State in the Union in 1796. It has no very large cities. The population, according to the census in 1870, was 1,158,520, with about one in fifteen living in towns of over five thousand inhabitants. The number of physicians at that time was 2220. One medical college, organized in 1850 at Nashville, is well supported, and there is another, a department of Vanderbilt University. The profession of the State has a Medical Society, formed in 1830, which publishes Transactions annually. Institutions for the care of unfortunates have been organized in different parts of the State, sufficient for the wants of the people.

The following Medical Journals have been published in Tennessee: Memphis Journal of Medicine, 1853; Memphis Medical Journal of the Progressive Medical and Physical Sciences, 1851; Memphis Medical Recorder, 1852; Nashville Journal of Medicine and Surgery, 1851; Nashville Medical Recorder, 1860; Record of Medicine and Surgery, 1852; Southern Journal of Medical and Physical Sciences, 1853.

Dr. Felix Robertson, a native of Tennessee, wrote, for graduation in 1805, a thesis entitled "Chorea Sancti Viti," giving in it a history of the dancing mania which had exhibited itself in Tennessee, Kentucky, and Ohio about 1802. He was well read in his profession, and possessed strong common sense and original powers of observation. He was twice Mayor of the city of Nashville, and a member of the Board of Trustees of the University; and through a long life maintained the dignity and characte1 of an able physician. (b. 1781; d. 1865.) Dr. Wilson Yandell, a native of North Carolina, settled in 1804 in Hartsville, Rutherford Co. He was a man of mental vigor, untiring industry, and had a love for the acquisition of knowledge. His devotion to his profession rapidly advanced him to the front rank of practitioners in the State. He received from the University of Maryland the honorary degree of M.D. in 1823. He was the immediate ancestor of the physicians of the same name in Kentucky. (b. Dec. 17, 1774; d. Oct. 1, 1827.) Dr. A. H. Buchanan, a native of Virginia, commenced practice in East Tennessee, then removed to Columbia, and finally to Nashville where he became a professor in the Medical School on its organization. He was a man of learning and high character. (b. 1808; d. June 20, 1863.) Dr. John McClaran Watson, a native of North Carolina, settled in Murfreesboro, Tenn., but on the organization of the Medical Department of the University of Nashville, was induced to take the chair of Obstetrics and Diseases of Women and Children. He was a minister of the Gospel, as well as a successful practitioner, and a promoter of medical education. (b. 1796; d. 1866.) Dr. A. P. Merrill, a native of Massachusetts, served as a volunteer surgeon with General Jackson's Army in the War of 1812, and in 1819 was appointed surgeon in the U. S. Military service. In 1823 he resigned, and settled in practice at Natchez. In 1850 he removed to Memphis, Tenn., and assisted in organizing the Medical College at that place, taking the chair of Theory and Practice. He originated and edited the Memphis Medical Recorder. He was an able practitioner and an original writer, his contributions to medical literature being varied and valuable. (b. April 1793; d. Nov. 1873.) In addition to these I will name Drs. T. R. Jennings, James Roane, W. Walker, R. Porter, J. L. Armstrong, and Samuel Hogg.

TEXAS.[1]—Dr. Benjamin Briggs Goodrich, a native of Virginia, practised for some time after graduating in Alabama and Mississippi, finally locating in Washington, Texas. He was a member of the State Legislature, and on Texas resolving upon independence, was one of the signers of its declaration. (b. 1800; d. 1860.) Dr. Anson Jones, a native of Massachusetts, a patriot of Texas, commenced his career as a practitioner of medicine at Brazoria. He entered the Texan army as a surgeon, and was

[1] Texas until 1836 was a part of Mexico. In 1821, emigration to it from the United States became considerable. In 1830, it demanded admission to the Mexican confederation, as a separate State, but being refused, declared its independence, and conquered in a contest of arms, in 1836, maintaining its independence as a republic till it was admitted as one of the United States, in 1846. Its area is the largest of any of the States. There are no large cities in Texas; but it has a fertile soil, navigable rivers, and rich minerals. The population in 1870, according to the U. S. Census, was 818,899, with about one in twenty-five living in towns of over five thousand inhabitants. At the same time there were 1906 physicians. Population is increasing, and wealth accumulating. One medical college is located at Galveston, and is giving instruction. A State Medical Society was formed in 1869, and publishes Transactions annually. Institutions for the care of unfortunates have been established.

The following medical journals have been published in Texas: Galveston Medical Journal, 1866; Texas Medical Journal, 1873.

at the battle of San Jacinto. In 1837–8, he was a representative in the Texan Congress. In 1838, he was Minister to the United States, where he made an unsuccessful endeavor to secure the annexation of his country. On his return, he was made Senator from Brazoria to the Texan Congress. He was Secretary of State of Texas in 1841, and in September, 1844, was elected President of the Republic, and held this office up to the time of the annexation of Texas to the United States. (b. 1798; d. 1858.) Dr. Edward A. Pye, a native of Maryland, was a physician of eminence. He had a good practice at Hearne, in Robertson Co. When the yellow fever at Calvert had exhausted the medical men of that place, he offered his services, which were gladly accepted by the authorities. But this heroic act cost him his life. He contracted the disease, and died. During the war, he served as surgeon at Beaumont. (b. July 9, 1818; d. Nov. 9, 1873.) Dr. John A. Pettus, a native of Virginia, settled at Fort Bend, Texas, where he acquired a large practice. During the late war, he was Medical Director of the Corps of Surgeons of U. S. Volunteers. After the war, he returned to practice, and was elected Professor of Physiology and Medical Jurisprudence in the Galveston Medical College. (d. Sept. 26, 1870, aged 52.) Dr. E. L. Massie was a skilful and benevolent physician of Houston. During the war, he served as a surgeon in the Confederate army, and was afterwards Medical Purveyor of Texas. He was one of the leading physicians of Houston. I will name in addition, Drs. E. T. Bonney, Alva Connell, W. Riddell, George W. Peete, W. D. Robinson, Wm. Richardson, W. R. Smith, and Wm. McCraven.

Vermont.[1]—Dr. Joseph Adams Gallup, a native of Connecticut, settled in practice at Woodstock, Vermont, in 1800. He possessed a vigorons mind, and was a close observer and correct reasoner. By skill in his profession, and by force of character, he acquired a leading business. He was Professor in the Castleton Medical College, and in the Medical College of the University of Vermont. In 1827 he founded the medical school of Woodstock. His best known writings are entitled "Sketches on Epidemic Diseases in Vermont," published in 1815, and "Outlines of Institutes of Medicine," published subsequently. (b. Mar. 30, 1769; d. Oct. 12, 1849.) Dr. Noadiah Swift, a native of New York, commenced practice in Burlington about 1802, and enjoyed a large practice for nearly fifty years. He was a man of great energy of character, and of correct habits, and possessed rare skill in his profession. His reputation extended far beyond the community in which he resided. He was elected at different times to the State Senate. (b. Nov. 24, 1776; d. Mar. 21, 1860.) Dr. Benjamin R. Palmer commenced practice in Woodstock, Vermont, where he acquired much popularity, and a good professional business. He possessed decided talents as a teacher of anatomy and physiology. He was elected to the chair of Anatomy in the Vermont Medical College, where he taught for many years with success. He also held the same

[1] Vermont was settled by emigrants from Massachusetts, New Hampshire, and New York, each of which States claimed her territory. In 1791 Vermont was admitted as the first new member of the Union. The population in 1870, according to the United States Census, was 330,531. She has no large cities, and about one in twenty-three of her population live in towns of over five thousand inhabitants. She had at the time mentioned 569 physicians. Vermont has one medical college, a department of the University of Burlington. A State Medical Society was organized in 1814, and publishes Transactions annually. A registration of vital statistics has also been established. Institutions for the care of unfortunates have been founded, sufficient for the wants of the people.

The following Medical Journal has been published in Vermont: Vermont Medical Journal, 1874.

chair in the Berkshire Medical College of Massachusetts. His reputation as a teacher gained him the election to the chair of Anatomy in the Louisville Medical College, when he removed to Kentucky, where he enjoyed an extensive practice up to the time of his death. Dr. Benjamin Chandler, a native of Connecticut, commenced practice in 1792 in Rutland, Vermont, where he acquired reputation, and whence he removed to St. Albans in 1807. As a successful operator in surgery, he had no equal in that section of the country. He was one of the founders of the Vermont State Medical Society, and of the Franklin County Medical Society. (b. 1772; d. 1818.) Dr. John Pomeroy, a native of Massachusetts, was a physician of ability. He settled in Cambridge, Vermont, in 1787, and in 1792 removed to Burlington, where he remained actively engaged in practice for over forty years. He was a man of vigorous intellect, and was characterized by simplicity of manner, directness of purpose, and strong common sense. Although he could not claim superior education, he was an ardent supporter of it. He was a member of the Corporation of the University of Vermont, and a professor in the Medical Department. (b. April 9, 1764; d. Feb. 19, 1844.) Dr. Jonathan Adams Allen, a native of Massachusetts, commenced practice at Windham, in 1814. In 1820 he commenced to give lectures on Chemistry, at Middlebury College, and continued till 1826. In 1822 he removed to Middlebury, where he became extensively engaged as a physician. He was an active member of the State and County Medical Societies. (b. Nov. 17, 1787; d. Feb. 2, 1848.) Dr. Horace Eaton, a native of Vermont, commenced practice in Enosburgh about 1824, and soon became popular as a physician. He was a man of great energy, and fully qualified in the duties of physician and surgeon. He was elected to the Legislature of the State, and was six years in the State Senate. He was also Lieutenant Governor and Governor of the State for two years. He was greatly interested in education, and was for years State Superintendent of Schools. In 1848 he was elected Professor of Natural History and Chemistry in Middlebury College, a position which he filled with credit to himself and the Institution till the time of his death. (b. June 22, 1804; d. July 4, 1855.) To these names may be added those of Dr. Theodore Woodward, of Castleton; Dr. David Palmer, of Woodstock; Dr. Ezekiel Porter, of Rutland; the Drs. Tudor, of Middlebury, and Dr. Danforth.

VIRGINIA.[1]—Dr. Wm. Baynham, a native of Virginia, studied medicine with his father, and then with the celebrated Hunters of London. He was an excellent anatomist, and was held in high esteem by his London preceptors, and by Mr. Cline and Mr. Else. He made some deli-

[1] Virginia was, in 1607, the seat of the first permanent settlement by the English in America, at Jamestown. Her first charter was annulled in 1677, and a new one, less liberal, granted. She took an early and decided stand in favor of American Independence. Her territory at that time was very large, and in 1781 she ceded the whole northwestern part to the U. S. government. In 1861 the western part of the State was formed into a separate State, under the name of West Virginia. Her geographical position gives her great commercial advantages. The population of Virginia, according to the U. S. Census in 1870, was 1,225,163, about one in ten living in cities of over five thousand inhabitants. The number of physicians was 2226. There are two medical colleges, one at Richmond and the other at the University, at Charlottesville. The State Medical Society, formed in 1821, publishes Transactions annually. Institutions sufficient for the care of unfortunates have been organized in different parts of the State. The first Insane Asylum in this country, under State authority, was established at Williamsburg in 1773.

The following medical journals have been published in Virginia: Monthly Stethoscope and Medical Reporter, 1856; Richmond Medical Journal, 1866; Virginia Clinical Record, 1871; Virginia Medical Monthly, 1874; Virginia Medical and Surgical Journal, 1856.

cate dissections and minute injections, which are still on the list of preparations at St. Thomas's Hospital. He practised for some years in London, but returned to Virginia, and settled in Essex Co. He was, without question, the most celebrated anatomist and surgeon in his day in America. Although being in a sparsely settled region, he was consulted by many persons from a distance, and frequently made long visits to perform surgical operations. (b. Dec. 7, 1749; d. Dec. 8, 1814.) Dr. James McClurg, a native of Virginia, served during the Revolutionary war as a surgeon, and part of the time as Medical Director. He commenced practice at Williamsburg in 1773, and soon won his way to recognition as a physician of high culture and skill. He had published in London, in 1772, a thesis on the Bile, which attracted the attention of the profession. On the removal of the State government from Williamsburg to Richmond in 1793, the doctor took up his residence in the latter city. By virtue of his decided skill, he was for nearly fifty years at the head of the profession of Virginia. (b. 1746; d. July, 1823.) Dr. Wm. B. Selden, a native of Virginia, settled in practice in Norfolk in 1799, and was fully employed for half a century. He was an elegant scholar, retaining his love for the classics to the close of his life. He was a careful reader of the best medical literature, and a close observer of disease. In 1799 he obtained from Dr. Jenner some vaccine virus, with which he continued to vaccinate, and with which he kept up a continuous supply for fifty years. He declared that he could see no variation in the appearance of the vesicle, nor any failure in its power of protection in all this time. (b. Aug. 31, 1773; d. 1849.) Dr. Beverly R. Welford, a native of Virginia, settled in practice in Fredericksburg in 1816, when he was scarce twenty-one years of age, and soon won the confidence and affection of a large circle of friends and patients. He was a man of liberal studies, and enthusiastically devoted to the advancement of the medical profession. In 1854 he was elected to the chair of Materia Medica and Therapeutics in the College of Virginia, and removed to Richmond. His courteous manners, ripe experience, and professional knowledge soon made him the popular physician of that city. In 1852 he was President of the American Medical Association. (b. July 29, 1797; d. Dec. 20, 1870.)

Dr. Hugh Holmes McGuire, a native of Virginia, commenced his career as a medical man in Winchester, Va., and soon developed a preference for surgery. His first operation was for cataract, which he successfully performed with a needle made under his direction by a mechanic in Winchester. In 1827, with other physicians, he established the Winchester Medical School, in which he was Professor of Anatomy and Surgery. In 1847 this instituton obtained a charter, and continued in successful operation till the beginning of the war. In 1862 the buildings were burned by soldiers under Gen. Banks. In the same year the Doctor was commissioned as surgeon in the Confederate Army, and served till the close of the war. He was tendered a professorship in a medical school at Philadelphia, and also at New Orleans. In 1850, he was elected one of the Vice-Presidents of the American Medical Association. He was a man of great industry, and had wonderful success as a surgeon. He performed lithotomy thirty times without losing a case. (b. 1801; d. 1875.) Dr. James Craik, a native of Scotland, was a surgeon in Braddock's expedition against Fort Duquesne, and also a surgeon in the Revolutionary Army. After peace was established, he settled in practice near Mt. Vernon, in Virginia. He was a warm personal friend of Gen. Wash-

ington, who mentioned him in his will, as his "old and intimate friend." The Doctor was one of the three physicians who attended Gen. Washington in his last illness. He was a thoroughly educated physician and surgeon, and a man of great probity of character. (b. 1731; d. Feb. 6, 1814.) Dr. George Cabell, a native of Virginia, practised for years with success and reputation at Lynchburg. He was a man of superior endowments and much culture. His practice extended for many miles along the valley of the James River. (d. 1823.)

Dr. J. P. Mettauer, a native of Virginia, practised in Prince Edward County. He was in many respects one of the boldest and most successful surgeons in the State. He commenced practice in this sparsely settled region about 1809, and during a long life maintained the character of a learned and skilful physician and surgeon. No medical man in the South was better known for his success, and for his valuable and numerous contributions to medical literature. Even when past eighty years of age, he was active and energetic in practice, and performed important surgical operations with success. (d. Nov. 22, 1875, aged 88.) Dr. Francis T. Stribling settled in Staunton, Va., where he soon acquired a large practice by devotion to professional duties. In 1836 he was elected physician to the Western Lunatic Asylum, and in 1840 was appointed Superintendent, which position he filled with great success to the time of his death. He possessed a strong intellect, and introduced many improvements in the management of the insane. (b. Jan. 20, 1810; d. July 23, 1874.) In addition I will mention Drs. Charles B. Gibson, James Henderson, John Minson Galt, Thomas Massey, Goodridge Wilson, Thomas Robinson, P. C. Spencer, Micajah Clark, James Bolton, John Cullen, James Currie, R. W. Haxall, B. H. May, A. L. Warner, H. Selden, G. N. Upsher, J. B. Strachan, J. S. Giliaim, J. B. McCaw, A. G. Strachan, John Field, John F. Peebles, T. R. Atkinson, D. J. Claiborn, R. B. Butt, besides the forty-five martyrs who died of yellow fever at Norfolk in 1855.[1]

[1] I believe that a complete record of the names of the physicians who died during the great epidemic of yellow fever in Norfolk, in 1855, has not found a place in medical literature. The list given here is taken from the Virginia Medical and Surgical Journal for Oct. 1855, p. 338, from the Portsmouth Relief Association's report, from Forrest's "Great Pestilence in Virginia," and from other sources. It will be observed that in some cases no first names are given. In the midst of the terrible panic and distress that existed, these noble men were buried without any special ceremony, but were conveyed silently to their graves. We honor their courage and devotion to professional duty, which was equal to that which, in the church, purchased the title of Saint. Over one hundred physicians volunteered their services, and actually went to Norfolk and Portsmouth during the epidemic. Forty-five medical men are known to have died of yellow fever in these cities, and, so malignant was the disease at one time, that seven physicians died upon one day, and five on another. A majority of the deaths of physicians took place among those who came from other places to render assistance to their medical brethren and the citizens in their great distress. While it is probable that I have not obtained the names of all, it is possible that some in the list may have been druggists and not graduates in medicine. The following are the names in alphabetical order, with the place of residence when known:—

Bache, Berry, J. L. (Memphis, Tenn.), Berry, R. B. (Tenn.), Blow, Richard (Sussex, Va.), Booth, T. (Baltimore, Md.), Briggs, J. A. (Norfolk, Va.), Burns, Capri (a Hungarian), Cole, C. (Philada., Pa.), Collins, Wm., Constable, T. F. (Norfolk, Va.), Craycroft, T. H. (Philada., Pa.), Crowe, N. J. (Richmond, Va.), Dabershe (Dist. of Columbia), De Berane, Dillard (Montgomery, Ala.), Fliess (Baltimore, Md.), Gilbardt, Leon (Richmond, Va.), Gooch, P. C. (Richmond, Va.), Halson, G. I. (Norfolk, Va.), Handy, T. W. (Philada., Pa.), Higgins, F. L. (Norfolk, Va.), Howe (Baltimore, Md.), Howle, Thos. P. (Richmond, Va.), Hunter, Edwin (Brooklyn, N. Y.), Jackson (Dist. of Col.), Lovett, M. P. (Portsmouth, Va.), Marshall (Baltimore, Md.), Mierson, T. (Philada., Pa.), Morse, Nash, Thos. (Norfolk, Va.), Nicholson, L. P. (Portsmouth, Va.), Obermuller (Georgia), Parker, R. H. (Portsmouth, Va.), Rizer (Penn.), Schell (N. Y.), Selden, Henry (Norfolk, Va.), Sil-

West Virginia.[1]—Dr. James W. Clemens, a native of Pennsylvania, commenced practice in Wheeling in 1819. He was not only solicitous for business, but ambitious for knowledge, and a constant reader of the best medical literature. He had a private chemical and pathological laboratory attached to his office, in which he was constantly experimenting. He was a ready writer and a fluent speaker, and enjoyed through life the reputation of a successful physician. (b. May 26, 1795; d. Nov. 21, 1841.) Dr. Martin L. Todd, a native of New York, commenced practice in Wheeling in 1810, and soon acquired reputation in the treatment of a then prevalent low grade of fever, popularly called the "Cold Plague." He served as a surgeon with the State troops in the war of 1812. He was remarkable for his acuteness in diagnosis, as well as for his choice of efficient remedies. (b. 1782; d. March 8, 1866.) Dr. S. P. Hullihen, a native of Pennsylvania, commenced practice as a surgeon and dentist in 1832, and the same year received the degree of Doctor of Medicine from Washington Medical College, Baltimore. He was a man of genius, gifted with original conceptions, and with ability to overcome difficulties in surgical operations, and to make efficient appliances. He never was a general practitioner. To him belongs the credit of originating the movement for the establishment of the Wheeling Hospital. He contributed some valuable articles to Medical Journals, the result of observation and experience. His death was regretted by all classes, and at a public meeting a spontaneous subscription was made to erect a monument to his memory. (b. Dec. 10, 1810; d. March 27, 1857.) Dr. R. H. Cummings, a native of Pennsylvania, commenced practice in Wheeling. His professional qualifications were of the first order. He was a ready writer, and a close observer of all the conditions that affect health. He occasionally contributed to medical literature, and was President of the State Medical Society in 1872. (b. Feb. 17, 1817; d. April 12, 1873.) In addition to the foregoing, I will name Drs. Samuel McKeehan, John W. Moss, H. D. Chapman, and James Tanner.

Wisconsin.[2]—Dr. Mason C. Darling, a native of Massachusetts, settled in 1830 in Fond-du-Lac, Wisconsin, the pioneer physician of that place. He had formerly practised for about thirteen years in Greenwich, Mass., where he had acquired experience and reputation. In addition to being a good physician, he was an energetic far-seeing business man. These qualities caused him to be put forward in politics, and he was elected to

vester, Richard J., and Silvester, R. W. (Norfolk, Va.), Smith, J. C. (Columbia, Pa.), Thompson, Robt. (Baltimore, Md.), Trugien, J. W. H. (Portsmouth, Va.), Tunstall, R. B. (Norfolk, Va.), Upsher, G. L. (Norfolk, Va.). Walters, Charles (Baltimore, Md.).

[1] West Virginia formed a part of Virginia until 1861, when a convention at Wheeling formed it into a new State; and it was admitted into the Union Dec. 31, 1862. The population in 1870, according to the U. S. Census, was 442,014, about one in twenty living in cities and towns of over five thousand inhabitants. The number of physicians was 612. A State Medical Society, organized in 1867, prints annually a volume of Transactions. Institutions for the care of the insane and other unfortunates have been established.

The following medical journal is published in West Virginia: West Virginia Medical Student, 1875.

[2] Wisconsin was early visited by the French missionaries, and permanent settlements were made in the latter part of the 17th century. It was erected into a territory in 1836, and admitted as a State in the Union in 1848. Population and wealth are rapidly moving to it. The U. S. Census of 1870, assigns to it a population of 1,064,985, with about one in eight living in towns of over five thousand inhabitants. The same authority gives 915 physicians. The agricultural resources are of the first order. A State Medical Society and a State Board of Health have been organized; the first, formed in 1842, has for years published Transactions.

the Legislature and also to the State Senate, where he took a prominent part in favor of educational projects. He was one of the originators and first President of the State Historical Society. (b. May 18, 1801; d. March 12, 1856.) Dr. John B. Donsman, a native of Michigan, commenced practice in 1826 with his preceptor Dr. Twitchell, of New Hampshire. In 1840 he came to Wisconsin, and settled in Milwaukee, where he practised with success up to the time of his death. He was a man of clear perceptions, great stability of character, and devotion to his profession. He was a member of the State Medical Society and of the American Medical Association. (b. 1807; d. 1868.) Dr. Clark G. Pease, a native of New Hampshire, commenced practice in Holyoke, Mass., in 1849, but in 1850 removed to Janesville, Wisconsin. His agreeable address and skill as a physician soon won him a numerous class of patrons. He was an influential member of, and one of the best workers in, the State Medical Association, acted on nearly every committee of importance, and was elected President in 1857. He contributed papers to the State Society, and also to the American Medical Association. He served as a surgeon in the late war, and lost his life from a dissection wound. (b. Dec. 31, 1821; d. June 27, 1864.) Dr. Alexander McDill, a native of Pennsylvania, after practising for fifteen years in his native State, removed in 1856 to Wisconsin, and settled in Plover, Portage Co., where he became fully employed. In 1862–3 he was sent to the Legislature, and the following year to the Senate. In 1868 he was chosen Superintendent of the State Hospital for the Insane. In 1872 he was elected to Congress, but at the expiration of his term of office, resumed his place as Superintendent of the Asylum. (d. Nov. 12, 1875.) In addition to the foregoing, I will name Drs. Henry M. Lilly, Moses Barrett, H. O. Crone, A. Blanchard, B. F. White, and Corydon Farr.

My task here ends. A century of American Medical Biography has been traversed. Of necessity, the memoirs are very brief. The claims of every part of our country have however been considered. The retrospect is finished: I would it were worthier of the occasion. And in conclusion I wish to bear testimony to the fact that the study of the lives of the physicians of America, for the Centennial period, has produced in me, as I trust it will in others, a profound conviction that eminence and honorable distinction in the profession, are the legitimate results of good education and professional knowledge, conjoined with high moral character and devotion to duty. These qualities, with a desire to do good and an active sympathy for our race, stand out so prominently in the lives of all truly eminent physicians, as to appear to be essential traits of professional character. So uniformly are love of man and devotion to duty characteristic of the true physician, that it is not arrogant to claim for him a higher approbation, as in the vision of Abou Ben Adhem, who saw that the name of him who loved his fellow men "led all the rest," on the roll "of those whom love of God had blest."

When in the course of time another century of American Independence shall have been concluded, the professional men then occupying our places may perchance choose to meet as we have done, and, casting a retrospective glance across the second century, contrast their position and the progress then attained with that which marks our era. And while I trust that they may have abundant reason to congratulate themselves upon the advance which medicine shall have made, I doubt not they will have an honest pride in the monuments, legacies, and records left by

the profession during the century just closed. These have in part been referred to in the addresses and papers read before this Congress. Permit me to indulge the hope that the biographer of the next Centennial may have as great pleasure and satisfaction in the study of the lives of the medical men of the second century of our country, as I have enjoyed in the preparation of this address. I am persuaded that the temple of fame holds no more honored tablets than those on which are inscribed the names and deeds of the worthy members of the Medical Profession. That the members of this Congress may deserve to have their names perpetuated to fame through all succeeding ages, is my earnest prayer. To have my own there enrolled, I may not hope, since this is granted to but the immortal few.

ADDRESS ON OBSTETRICS.

BY

THEOPHILUS PARVIN, M.D.,

PROFESSOR OF OBSTETRICS AND THE MEDICAL AND SURGICAL DISEASES OF WOMEN, COLLEGE OF PHYSICIANS AND SURGEONS OF INDIANA.

"There is no department of medicine or surgery, superior to midwifery in dignity and utility."—*Mauriceau.*

Of American Obstetrics, with its associate branches, Gynæcology and Pædiatrics, we commemorate the centennial.

What have Americans done, in the century just closed, to advance these great departments of Medicine? How easy the question and how difficult the answer! Contemporaneous discoveries and successes are found in these as well as in other fields of human effort, and who, then, shall be *primus inter pares!* Sometimes, too, that which is supposed to be new[1] is really old, and the discoverer has unconsciously trodden in the footsteps of another.

Beside this, much of medical knowledge is merely provisional, the best expression of the truth for the time being, and serves only a present utility—a mere ladder by which we ascend to higher platforms and larger planes, and which is then cast aside. Nor does everything claimed as valuable by him who first points it out, prove to be such when thoroughly tested. Alas, for the many Ixions who mistake a cloud for a goddess! Alas, for the fool's gold that so often delights, so surely disappoints! Change is not synonymous with improvement; so far from change always being progress, it may be retrogression.[2]

Nor are these the only difficulties. Add to them the limitation of individual knowledge, and the infirmities of human judgment, so liable to error in estimating the value of things present, no matter how determined and desirous one may be *suum cuique tribuere.* And surmounting all these, there stands in bold relief the fact that the very work of this occasion has been largely anticipated by an able paper in the American Journal of the Medical Sciences,[3] from the pen of one who has himself contributed so much to the glory of American Medicine. Confronted by such difficulties, and addressing such auditors, one even of ample qualifications might well shrink from attempting the duty assigned me.

However, strengthened by your recognition of these impediments, and

[1] Professor Blackie, *Horæ Hellenicæ*, observes: "Even in the free exercise of poetical talent in the case of individual poets of highly potentiated imagination, we constantly stumble on comparisons which have been made independently by other poets at other times or in distant countries, and which superficial critics are sometimes eager to fasten on as plagiarism." Quite similar facts are observed in the history of medicine.

[2] Baudelocque, Vol. II, pp. 34, of *L'Art des Accouchemens*, Paris, 1781, in referring to the obstetrical forceps remarks: ". . *car si plusieurs ont travaillé à sa perfection, les autres ne l'on rendu que plus imparfait.*"

[3] July, 1876. A Century of American Medicine. III—Obstetrics and Gynæcology. By T. Gaillard Thomas, M.D.

confiding in your earnest sympathies with this work, I address myself to it with zealous desire to honor justly both the dead and the living of our country, who have added useful knowledge in obstetrics, and in diseases of women or of children, and to help exalt their fame in all they have thus done to lift or lighten the cross of human suffering.

In this proposed exposition, the subject of Obstetrics will, for obvious reasons, be presented first. After some brief allusions to the condition of obstetric art and science in the last century, and the sources from which the first American practitioners of this art derived their knowledge, suitable topics will be found in works on Obstetrics, and in special contributions to obstetric pathology and therapeutics.

The eighteenth century was marked by great advances in obstetric knowledge and improvements in practice, though history has shown the mistake of a distinguished physician of South Carolina, Dr. David Ramsey, who, in an address before the Medical Society of his State, on the first day of the present century, asserted "that the art of obstetrics had been brought to such perfection, further improvements were scarcely to be expected."

Standing at the commencement of the eighteenth century is the famous Hollander, Deventer, who declared theory as essential to practice as body to shadow—surely a very doubtful compliment to practice—who asserted that those[1] ignorant of the obliquities of the uterus were equally blind with him that saw trees as men walking, but who, despite these and similar exaggerations, despite plain, palpable errors, advocated cephalic version, and taught the genu-pectoral position as part of the treatment in one variety of prolapse[2] of the cord, and in some cases of transverse[3] presentation.

Near its close we have the illustrious Baudelocque, to whom so many of our obstetric authors acknowledge their obligations, and whose fame, notwithstanding his twenty-three presentations and ninety-four positions, has suffered only partial eclipse in the revolutions of more than four-score years. And interposed we have Chapman coming from the country to London once a year to give lectures on obstetrics, the first,[4] and the first ever given in that city, being in 1730, and revealing the beneficent instrument which the Chamberlens had kept secret with cruel cupidity, the obstetric forceps; we have Sir Fielding Oulde, the wit's "Lord deliver

[1] . . quicunque obliqui situs uteri sunt ignari, in artibus obstetricantium æque cecutiant, ac ille, qui homines instar arborum videbat ambulantes. Henrici à Deventer, Medicinæ Doctoris, Operationes Chirurgicæ Novum Lumen exhibentes Obstetricantibus, etc., Leyden, 1701.

[2] Similiter accidit sed rarius, ut *funiculus* à capite retrorsum *vertebris* vel *ossi sacro* apprimatur: tunc caput alterutra manu removendum, prout scilicet plus minus in alterutrum latus vergit; parturienti (si vires suppetant) in *genua provolutæ* à tergo manum sospitam admoveat *obstetrix:* aut, si infirmior fuerit, in *alterutrum latus* declinetur, *uno pede ad ventrem attracto*, ut plus spatii suppetat, etc., Ibid., cap. xxxviii. De partu difficili, ab infante prævia vena umbilicali prodeunte.

[3] . . . ideoque parturientem, tali situ disponat, quo uteri spatium suppetat, nec ille cum infante in manum *obstetricis* delabatur; nimirum *utero propendulo* convenit parturientem *procumbere genibus*, aut *in dextrum latus inclinare*, posteriore corpore aliquatenus elevato, et ut plurimum prono, etc., Ibid., cap. xl. De infantibus transversim positis.

[4] My authority is Dr. Thyme. Vide MSS. Lectures, 1794, Surgeon-General's Library, Washington. Dr. T. remarks, "Chapman had no machines, nor were his students allowed to attend midwifery cases. To Smellie we owe those improvements." Denman, however, credits Dr. Maubray, 1724, with the first lectures.

us," giving a glimpse at the mechanism of labor, that mechanism soon to be much more fully and clearly expounded by Smellie and Solayres; we have Smellie and William Hunter, representing opposite but concordant elements of obstetrics, mechanism and physiology, the one writing a work upon obstetrics, the fruit of forty years' study, which for nearly three-quarters of a century was the best English text-book, and enduring the harmless criticism of Dr. Burton, who probably had his reward in being made the Dr. Slop of Tristram Shandy; the other preparing those plates of the human gravid uterus, which can never become obsolete; we have Levret, the geometric obstetrician, explaining the mechanism of labor before the Paris Academy of Surgery, using in his demonstrations the egg of an ostrich and a "mattrice mechanique," and dividing with Smellie[1] the honor of an important improvement in the forceps. With these two men, using the words of Baudelocque, commenced the most brilliant epoch of obstetric art.

But it is not my purpose to mention further the famous obstetricians of the eighteenth century, and allude to any of their special contributions. Enough to add that the two first American obstetric practitioners had been instructed by two of the most eminent of London teachers, for Lloyd,[2] in Boston, and Shippen,[3] in Philadelphia, were pupils of Smellie and Wm. Hunter. Thus we see that the germ of American Obstetrics was British rather than French, in so far giving probable contradiction to the assertion of Dr. Tyler Smith when he states, "notwithstanding the blood relation between the United States and this country, American Midwifery is far more the child of France than of England." So, too, probable contradiction is given by these other facts, that Dr. Samuel Bard, the author of the first American work on obstetrics, had received his professional training largely in Edinburgh; and that there, too, after having previously been a house-pupil in London under Drs. Osborne and John Clarke, was instructed Dr. T. C. James, the first professor of obstetrics in the University of Pennsylvania. And the famous Dewees, who bears the same paternal relation to American Obstetrics that Physick does to American Surgery, or Rush to American Practice, and who "has the high honor of first attempting a full course of lectures on obstetrics in America,"[4] was too independent a thinker and original investigator to be unduly swayed by the teachings of any man or of any school.[5] And finally, the obstetric books of the American profession have been British or American much more than French.

Seventy or eighty years ago, the practice of obstetrics was almost exclusively in the hands of women, even in the long settled parts of the United States, and, as the tide of population passed westward, the female midwife was still the trust of the matrons among the early settlers who

[1] Possibly, according to the recent researches of Dr. McClintock, each of them was anticipated by Dr. Pugh (see Dublin Journal, June, 1876), so that there is an end to the contest in this matter between France and England.

[2] Dr. Bartlett, of Massachusetts, states that Dr. James Lloyd, of Boston, was the first systematic practitioner of midwifery in that section of the country, 1754.

[3] In 1756 Dr. William Shippen engaged in the same practice at Philadelphia, and subsequently was Professor of Anatomy, Surgery, and Midwifery in the University of Pennsylvania.

[4] Eulogium upon William P. Dewees, by Prof. Hugh L. Hodge. These lectures were first delivered in 1797.

[5] "Drs. James and Dewees should be regarded as the fathers of obstetric science in America: the former, erudite and polished, gave currency to the teachings of the British schools; the latter, more vigorous and energetic, exemplifying theoretically and practically the doctrines of the French obstetricians."—Preface to Hodge's Obstetrics.

swept away the forests and founded an empire beyond the Alleghanies. Dr. J. G. F. Holston[1] has given a most graphic description of early midwifery in Ohio; but the compensation of these practitioners, as stated by him, seems so meagre that none could have had a lucrative practice, no matter how large it was—one dollar, and in cities not more than three dollars, and in the country the midwife was also nurse the first week or until the ninth day.

Nor had the American midwife any of the posthumous honors which were once the fortune of her sisters in France. That famous *sage-femme*, Louise Bourgeois—whose picture[2] Dr. Goodell has with such perfect art recently exhibited to the profession—in her Instructions to her daughter, Paris, 1617, states, "There are now very few women who have such an affectionate regard for their midwives as prevailed formerly, when it was the custom, if a midwife died, for her friends to put on deep mourning for her, and they prayed God not to send them any more children,"[3] etc.

However the fact remains that the general practice of obstetrics in this country has been but for little more than half a century chiefly devolved upon medical men, and even to this day, in most of our large cities, a considerable portion of this practice, more especially among the foreign population, is attended to by females.

The first American work on obstetrics was the Compendium of Midwifery, by Dr. Samuel Bard, of New York, the first edition being issued in 1808, the fourth and last in 1817. Using the phrase of the present day, this book is remarkable for its amount of *padding*, page after page being filled with extracts from Smellie, Perfect, Clarke, and various writers in the Medical and Physical Journal. But it also contains many judicious thoughts and directions well expressed by Dr. Bard, and from the very number and variety of important cases quoted, is really an excellent compend of clinical obstetrics. In meeting that *nodus obstetricus*—why labor occurs at nine months, Dr. Bard does not shelter himself with Avicenna behind the grace of God, but assails the question with a brace of questions, Why do strawberries ripen in June and peaches in August?

In reference to the delivery of the placenta, he certainly was greatly in advance of many obstetric authors of recent days, advising almost essentially Crêdé's method before Crêdé was born, as the following passage clearly shows. "While this is about," *i. e.* attention to the new-born child, "let an assistant or the woman herself, place her hand on the abdomen, a little above the fundus uteri, so as in some measure to grasp it in the palm, and make a moderate pressure upon it. This can possibly do no harm; it has been my general practice; and I think I have found a manifest advantage from it in promoting the contraction of the uterus and in disengaging the placenta."

And in the management of women after parturition the following instructions seem wise, though even at this day they are too generally neglected. "After one or two days, women should rise from their beds, and sit up for a longer or shorter time every day, according to their

[1] Transactions of the Ohio State Medical Society, 1857.

[2] A Sketch of the Life and Writings of Louyse Bourgeois—The Annual Address of the Retiring President before the Philadelphia County Medical Society, by William Goodell, A.M., M.D. Delivered June 5th, 1876.

[3] Dr. John Moultrie, for forty years at the head of the profession in Charleston, died about 1773; and several of the ladies of the city bedewed his grave with their tears, and went into mourning for him.—Thacher's Medical Biography.

strength and inclination." "The lochia require no other attention than sitting up a short time every day to promote their evacuation."

Of wider scope, of more originality, and destined to a much larger fame, was the Compendious System of Midwifery, by Dr. Dewees, first issued in 1826, of which no less than twelve[1] editions have been published. Dewees himself had some years before, 1807, published an abridgment of Heath's translation of Baudelocque, with annotations, and it passed through at least three editions. Burns's Principles of Midwifery was republished in 1810, with notes by Dr. Chapman, and several subsequent editions edited by Dr. James; in 1821, Denman's Introduction to the Practice of Midwifery, with very valuable notes by Dr. John W. Francis, was issued in New York. But not Baudelocque, nor Burns, nor Denman was to be the first great obstetric teacher of the American profession. He who was to have this honor was no favored child of fortune, but one early in youth left fatherless and poor, with no heritage but a good name, with few facilities for acquiring a complete literary education amid the desolations of the Revolution, yet bravely struggling up against rude adversities. His first professional labors were those of a country practitioner in one of the pleasantest[2] of villages—once hidden away, surrounded by farms or forests—now almost lying within reach of the outstretched arms of this great city. There he acquired those habits of close observation and reflection which were the foundation of his future greatness and fame. Then, after four years' experience casting his lot in Philadelphia, and without fortune, without family influence, without professional friends save as they were attracted to him by the recognition of his abilities—no adventitious aids—he succeeded by his own inherent power and perseverance. The name of William Potts Dewees should live forever in the memory of the American profession.

Dr. Hodge, in speaking of Dr. Dewees in 1842, observed: "He is our representative to other nations on the science of obstetrics, and as such is continually quoted by European authorities, as if he constituted one of their own number.[3]" As to the merits of Dewees's Midwifery, let me again quote the opinions of Dr. Hodge. He considered it, twenty years after its publication, as "probably the best practical book in our profession." "It takes a stand decidedly in advance of Denman, Osborne, Burns, and other English authorities in general use in our country at that period, and even of Baudelocque himself," etc. Without acceding to the truth of Pope's couplet—

> "Authors, like coins, grow dear as they grow old;
> It is the rust we value, not the gold,"

[1] "An honor rarely, if ever, bestowed on any similar work."—History of American Medical Literature. By Professor S. D. Gross.

[2] Abington, Penna.

[3] "Dr. John Ramsbotham, of London, dedicated the second part of his 'Practical Observations on Midwifery' to Dr. Dewees, in connection with Sir C. Mansfield Clark. Dr. Edward Rigby, of London, and author of a most excellent work on Obstetrics, which has lately been republished in this country, writes to Dr. Dewees in August, 1834, in the following manner:—'I trust you will pardon the liberty I have taken in writing to you, as well as the motives which have induced me to do so. I have been accustomed, for some years, to hold such frequent intercourse with you in reading your admirable system of Midwifery, and work on children, that I cannot refrain from requesting a more direct intercourse between us,' etc. The July number of the British and Foreign Medical Review, for 1839, contains the following handsome compliment:—'The Philadelphia school of Midwifery has for many years been looked upon with great respect by the obstetricians on this side of the Atlantic. The high name and professional standing of Dr. Dewees, his great experience, and, above all, his inestimable, compendious system of Midwifery and other valuable publications, have mainly contributed to this result.'"

let me predict that as with pious care and grateful appreciation one of the most celebrated of the Dublin school is bringing the "Treatise" of Smellie before this generation, so this or a succeeding generation will see the avatar of the "Compendium" of Dewees in like modern form.

To Dr. Dewees, probably, we are more indebted than to any one else, for the general preference on the part of the American profession for the long forceps.[1] Dr. Dewees[2] accomplished great good in exposing the errors of Denman in his aphorisms as to the use of this instrument, and the practice of the former is much more in accordance with that of to-day, than is that of the latter: an apparent timidity and conservatism ruled Denman's teachings in this regard, which conduced to the interests of neither mother nor child—he was a representative of that *physiological* school of obstetrics, originating with William Hunter, in opposition to what Tyler Smith has termed the *mechanical* school of Smellie—but our American obstetrician represented both in harmonious union.

To Dr. Dewees belongs the honor of pointing out the value of venesection not only in overcoming rigidity of the os uteri, but also in diminishing the resistance of vaginal cicatrices—the utility of his teaching and practice in this regard being recognized by foreign obstetricians.

It is sad to recall the fact that this famous man, with whom the history of American obstetrics really begins, should ever have been an unsuccessful candidate for the chair of midwifery in the University of Pennsylvania;[3] sadder still to know that when, twenty-four years later, the honor came by unanimous voice of the trustees, failing health permitted him to hold it but a year—the infirmities of age are upon him, almost the shadow of the grave is in his path before he reaches the noble goal of his life, and grasps the coveted prize. How true it is that his

—"laurel crown
Rustled most when the leaves turned brown."

The next American work on Obstetrics was "The Philadelphia Practice of Midwifery," by Dr. Charles D. Meigs,[4] published in 1838; it was brief and elementary; a second edition was issued in 1842. Three years subsequently Dr. Meigs was elected a professor in Jefferson Medical College, and became famous as one of the best of teachers and one of the most brilliant of lecturers, while his genial manners in social life won all hearts. Almost equally fascinating with his uttered are his printed words, so that one can now read page after page until hours pass away without weariness. Fluency and force, strength and beauty, characterized his literary composition in a remarkable degree; not only

[1] It is stated by Hutchinson (Biographia Medica, London, 1799), that Smellie for a time endeavored to substitute wooden for steel forceps.

[2] In referring to Dr. Denman, Dr. Dewees remarks: . . "there was a time in my life when I looked upon Dr. Denman to be the highest authority in midwifery."

[3] The election of Dr. James seems really to have been effected by a combination between him and Dr. Chapman—there were only three candidates—the latter to have half the emoluments, and to succeed to the next vacant chair in the University (see Hodge's eulogium upon Dewees)—a sort of bargain and sale which at this distance does not seem quite immaculate.

[4] A very interesting memoir of Dr. Charles D. Meigs, by his son, Dr. J. Forsyth Meigs, has been published, but unfortunately did not come into the writer's possession until he was correcting the proof-sheets of this Address. In the memoir the author states that his father wished to call his home in the country "Paraclete," the Comforter. Was not this in imitation of Abelard, who, when his admiring students, following him to his desert retreat, built him a new oratory, called it the Paraclete?

the treasures of his favorite department were his, but he was at home in general literature and familiar with classic story, while a Gallic vivacity, if not a poetic genius, flashed out as he moved on his brilliant path, like the phosphorescent spray from a ship's prow as she ploughs the tropic seas.

In 1849, Dr. Meigs re-issued his work on obstetrics in a much enlarged and improved form; since that date four editions have been published, this fact indicating the great favor with which the American profession regarded it. The most valuable discovery in obstetric pathology made by Dr. Meigs is the usual cause of sudden death after delivery, which he announced in 1849. Before and since, various other explanations of this terrible accident have been given, such as the entrance of air by the uterine veins, nervous exhaustion, shock, puerperal poisoning, etc., but to-day the theory of Meigs has general professional acceptance, even if the heart clot, or pulmonary thrombosis, the immediate cause of fatality, should be proved to be itself a result of myo-cardiac[1] degeneration.

As we read how earnestly Dr. Meigs declares against Cæsarean section, save in the interests of the mother, we shall find additional reasons for questioning Dr. Smith's allegation as to American obstetrics being so much more nearly a product of French than of English teaching—the chief alleged characteristic of the French school being the proneness to sacrifice the mother's rather than the infant's life whenever the rights of the two are brought in conflict.

Dr. Meigs's views as to puerperal fever were boldly expressed, and no concealment of them is now required. The painter in the Sacrifice of Iphigenia simply drew a veil over the face of Agamemnon: Would that a veil had forever hidden, not the views of Dr. Meigs only, but of other teachers of the Philadelphia school, and many sacrifices, blinder, more terrible, might have been averted! He who held the non-contagiousness of measles, of scarlet fever, even, might also hold to the non-communicability of puerperal fever under all circumstances. To-day, at least, the American profession are generally agreed that while the disease is frequently auto-genetic, it sometimes at least, and in its most terrible forms, is hetero-genetic, and it is the merest *petitio principii* to allege, as Dr. Meigs did, that, because he did not know of his having communicated it, either by conveyance of the poison from one patient to another, or from autopsies, therefore this never happened to others.

In 1849, "A Theoretical and Practical Treatise on Human Parturition," by Professor Henry Miller, of the University of Louisville, was published. In 1858, a republication of this work, much enlarged and improved, was made. From Dubois, Dr. Miller took his anatomy, and from Dugès, his classification of presentations and positions, while his principles of practice were essentially the same as those of Burns and Hamilton. Dr. Miller's views in reference to the importance of inflammation of the lining membrane of the uterus as a frequent cause of abortion, were certainly in advance of obstetric writers of his day. So, too, the higher honor must be given of being the first American obstetric author who advocated anæsthesia in labor.

Dr. Miller was a man of vigorous mind; he was clear and original in thought, concise in expression, severe almost to bitterness in controversy.

[1] De la Myocardite Puerpérale, comme cause la plus fréquente des Morts Subites apres l'Accouchement. Par Maurice Coste. Paris, 1876.

Like Dr. Dewees, he had few early advantages in education, but he learned to use his pen with more ease and force than the latter, and probably was a man of better original endowments. His work on obstetrics was declared by the late Dr. Condie[1] to be a most able one, conferring the utmost credit upon the author.

The Principles and Practice of Obstetrics, by Gunning S. Bedford, M.D., Professor of Obstetrics in the University of New York, was published in 1861; and five subsequent editions have been issued. Dr. Bedford's volume, which is probably the most learned of American works on obstetrics, is in the form of lectures, and with here and there some extravagances in rhetoric, is written clearly, plainly, indeed sometimes assuming a colloquial style. It is evidently "the result of much labor and research."[2] From its long list of authors quoted, and from its full and carefully prepared index, it is admirable as a book of reference.

We find this author arraying himself on the side of the contagionists as to puerperal fever, but a rejector of anæsthesia in normal labor. His teaching as to the choice between craniotomy and the Cæsarean section is almost directly the opposite of that of Dr. Meigs and most other American obstetricians, and while he protests against craniotomy, he does not fully recognize that podalic version may be the true alternative, and not the Cæsarean operation.

In 1864, the most original and elaborate of American Obstetrical works was issued in Philadelphia, the admirable treatise of the late Dr. Hugh L. Hodge, the honored successor of Dewees in the University of Pennsylvania. After thirty-one years of obstetric teaching, he prepared this rich legacy for the profession.[3]

Dr. Hodge was the first to illustrate the peculiarities of the female pelvis, its axes and its planes, by taking a plaster cast of it, and making sections of the cast. He was the first to use photography,[4] to exhibit with perfect accuracy the relations of the fœtal head to the mother's pelvis, in the various presentations and positions, and in the different stages of simple and of complicated labor, and also the applications of the forceps in different positions. He contributed materially to restore the vectis to use in the remedying of malpositions. The application of the obstetrical forceps to steady and isolate the head, in craniotomy, then after the operation the use of the instrument for compression and extraction, must be regarded as a most valuable improvement, for which the profession is indebted to Dr. Hodge. He also improved the operation of cephalotripsy by devising a simple and efficient instrument —his compressor cranii. He taught the doctrine of synclitism, or parallelism of the plane of the child's head, in cases of natural presentation, to the planes of the pelvis and vagina, as early as the year 1832, and from that time until his death presented this doctrine with more precision and detail than has any other author. He was among the first to recommend the induction of premature labor, in cases where labor had previously terminated fatally to the child in consequence of its great size and the complete ossification of its head. His obstetric forceps is the one

[1] American Journal of the Medical Sciences, Jan. 1857.
[2] British and Foreign Medico-Chirurgical Review, July, 1862.
[3] "Dr. Hodge's treatise is, nevertheless, full of much valuable information, and although its price alone will prevent its ever becoming popular amongst students in this country, still we strongly recommend that it should have a place in the library of every obstetric physician."—British and Foreign Medico-Chirurgical Review, October, 1865.
[4] This was done at the suggestion of his son, Dr. H. Lenox Hodge.

held in most favor by the American profession. Few men have studied the mechanism of labor more thoroughly, none have expounded it more clearly. Generations may come and depart, until another century pours its treasures upon the race, but it is doubtful if among these will be found another work on obstetrics of greater relative merit and of more enduring value than the treatise of Dr. Hodge.

In the year 1870, a Treatise on the Theory and Practice of Obstetrics, by Dr. W. H. Byford, was published, a second edition appearing in 1873. The design of Dr. Byford was to furnish the student and busy practitioner with a more concise work than any in general use, and yet embodying all the necessary practical information. No one can question how well its able and industrious author has succeeded in this design.

A work entitled Elements of the Principles and Practice of Midwifery, by Dr. David H. Tucker, was issued in 1848. The Obstetric Catechism of Dr. Warrington was, in Philadelphia, held in high esteem twenty or more years ago, especially by those who were fortunate enough to avail themselves of the practical instructions of this excellent teacher. Dr. Cock's Manual of Obstetrics, 1853, was held in like favor in New York.

Having devoted so much space to the consideration of treatises upon obstetrics, the topics of obstetric pathology[1] and therapeutics must necessarily be briefly considered.

The present century has been marked by some of the most important advances in obstetrics; and "it is not going too far to assert that, notwithstanding the brilliant improvements in surgery, and the solid and wise modifications in medicine, the changes in our art have preserved more lives, and relieved more human suffering and misery."[2] In promoting these advances, and in effecting these changes, our country has borne her part. Few obstetricians of the present day would accept the statement of William Hunter, that upon the whole the forceps had "done more harm than good," or participate in the "doubt" of Denman "whether it would not have been happy for the world if no instrument of any kind had ever been contrived for, or recommended in, the practice of midwifery," or believe—and we quote again from Denman[3]—that "the doctrine of applying forceps before the bulk of the head has passed the superior aperture of the pelvis, carries great danger and insurmountable difficulties with it;" nor again, does the obstetrician now wait until he can "feel an ear," or until "the head has rested six hours upon the perineum," before resorting to an instrument that is at once for the good of both child and mother.

Anæsthesia in obstetricy must be counted one of the greatest glories of the century—nay, the brightest, broadest beam of perennial light that has ever fallen upon woman's pathway, darkened by the primeval curse. In 1846, the great discovery of anæsthesia by inhalation of ether vapor, with which the names of Wells and Morton are so inextricably inter-

[1] The topics of obstetric anatomy and physiology would be appropriate could their history be made really complete. Under the former head would be mentioned in detail Dr. Hodge's studies as to the female pelvis, already referred to; Dr. John Neill's, as to the shape of the thyroid foramen, and numerous similar investigations; and under the latter, studies such as Dr. Dalton's, of the *corpus luteum;* Dr. Geo. J. Englemann's recent, patient, thorough, and interesting researches as to the mucous membrane of the uterus; Dr. Isaac E. Taylor's demonstration of the non-shortening of the cervix in the latter months of pregnancy; and Dr. J. R. Beck's observations upon the entrance of the spermatozoa into the uterus.

[2] Dr. Thomas Edward Beatty's address before the Dublin Obstetrical Society, 1862.

[3] Criticism of Leake.

woven, was made; in January, 1847, the illustrious Simpson, of Edinburgh, proved the value of ether in child-birth. The discovery found Walter Channing, the first professor of obstetrics[1] in Harvard, past his three-score years; but he entered heartily into the study of this question, and the next year he presented a volume upon Etherization in Child-bed, in which the records of nearly six hundred cases were given, and which greatly contributed to the frequent—I wish at this day one could say *general*—use of anæsthetics in labor. Other names deserving of recognition in this connection are those of Drs. F. Barker, of New York; H. R. Storer, of Boston; J. C. Reeve, of Dayton, Ohio; and H. Miller and L. P. Yandell, of Louisville, Kentucky.

The administration of chloral is another of the beneficent means of recent times for the relief of the sufferings of parturition, which has had many advocates in this country.

The opium treatment of peritonitis is one of the most important advances in therapeutics. It may be now true, as asserted by Dr. Spender,[2] that "the bare mention of that terrible disease, peritonitis, is suggestive of the siren lullabies of opium." But this treatment was the suggestion and practice of Dr. Alonzo Clark, as early as 1841,[3] though it was not until 1848 that he had an opportunity of demonstrating its value in the puerperal form of peritoneal inflammation. Opium alone, is the treatment of *phlegmasia dolens* advocated by Dr. Clark,[4] of Oswego, and he claims that the remedy is as efficient as quinia in ague.

A marked advance has been made in the more liberal diet and rational hygiene of women after labor. That former horror of the lying-in room,

[1] This position Dr. Channing held for nearly forty years, and only a few weeks since passed away from earth, having lived to be upwards of ninety years of age.

[2] Therapeutic Means for the Relief of Pain. London, 1874.

[3] The following recent note from Dr. Clark will be read with interest:—

DEAR DOCTOR: I continue to use opium in the treatment of simple peritonitis and puerperal peritonitis, and peritonitis after perforation, with the fullest confidence that it will cure a much larger proportion of cases than any other treatment yet proposed. It is accepted by all the physicians of this city and State, I think; at least I hear of no other, not only for peritonitis in its different forms, but in all operations, injuries, and wounds that are likely to be followed by this inflammation. I do not know how far it is approved abroad, but infer that it has received little attention in England, from the fact that Dr. Barker, of this city, made it the topic of an address at a meeting of English physicians a year or so ago.

Dr. Graves, it is said, used opium in large doses for the cure of peritonitis after perforation of the intestine. When he began that treatment I do not know; but I do not find any reference to it in his lectures published in 1843. Watson's Lectures, one of the later editions, refers to a Mr. Bates, who treated peritonitis with opium; but when he began, does not appear. Still the English physicians generally seem to know little of the power of opium in the different forms of peritonitis. In the Am. Journ. of Med. Sci., July, 1876, p. 144, you may find a statement of the origin of this treatment in my own mind, in 1841; and before 1843 cases of intestinal perforation had been successfully treated on this plan.

Regarding the rules, I begin with two grains of opium, or its equivalent opiate, and in two hours give the same, or more, or less, according to the effects produced. Patients resist or yield to the narcotic effects of the drug very differently. In some cases, twenty-four grains of opium a day is all that is required; in a few, twelve or sixteen grains is sufficient. In most, two to four grains at a dose are needed; in a few, more than this. The aim is to get and maintain the symptoms of *safe narcotism*, or, as I sometimes call it, semi-narcotism, indicated by subsidence of the pain; contracted pupils; itching of nose and skin; a continuous sleep, from which, however, the patient is easily aroused; reduced frequency of respiration, followed by reduced frequency of pulse; and absolute quiet of the bowels.

Regarding the respiration, the aim is to reduce its frequency to twelve in the minute, and in the attempt to do this it is often found to fall as low as seven, without danger, if the opium is then withheld for a few hours, till it rises to ten, when a smaller dose is given, to be increased or not afterwards. Yours truly, A. CLARK.

[4] New York Medical Record, June, 1870.

the very existence of which is cast in doubt—milk fever—which was to be averted by a liberal use of the canonical caudles of Smellie's day, and the prophylactic panadas of more modern times, and by poisoned air, no longer governs practice; and the recent parturient is given cold water as she desires, inhales pure air, and is fed with wholesome food, that is, such as will best repair her exhausted powers and furnish suitable material for making good milk. How early any publications were made advocating this course, I cannot ascertain, but certainly it has been pursued for some years, and possibly it may not have been at the instance of any professional leader, but simply a general tendency of physicians themselves. The names of Drs. F. Barker, William Goodell, and William B. Atkinson, are among the more prominent of those who have advocated it.

During the present year a most able volume on Extra-uterine Pregnancy, by the late Dr. John S. Parry, has been published. In it the talented author, removed too early for the honors that surely awaited him, and for a career of great usefulness not half accomplished, has collected five hundred cases of this accident, and from a careful study of them has deduced most important practical rules. Such works are at once permanent contributions to medicine, and indices of its higher development and more scientific character.

Some of the most remarkable cases of operation for extra-uterine pregnancy are those by Dr. William Baynham,[1] of Virginia, who operated twice, 1791 and 1799, successfully by abdominal incision; one by Dr. John King, of South Carolina, 1813, who incised the vagina at full term, saving both mother and child, and one by Dr. T. G. Thomas, the pregnancy having advanced to three months, who also incised the vagina, but with the galvano-caustic knife. So, too, the case reported by Clarke, in 1806, where he passed his hand into the bowel, and with his finger in the child's mouth exerted such traction that he withdrew the head *per anum*, should not be forgotten among the extraordinary cases, especially as he thus showed the feasibility of the method of abdominal exploration recently proposed by Simon. Dr. Easley,[2] of Little Rock, Ark., has quite recently reported an interesting and successful case of abdominal section for the removal of a dead fœtus.

The Induction of Premature Labor was first resorted to in this country, in 1810, by Dr. Thomas C. James[3]—the cause, contracted pelvis, and the result fortunate to both mother and child. Since that time a much larger range has been given this operation. We have already stated Dr. Hodge's views as to its being indicated in cases where the head is large and completely ossified at full term. Dr. Thomas[4] has enumerated ten other conditions indicating it, the most important being placenta prævia and aggravated uræmia. The teaching and experience of Dr. Thomas have contributed much towards the recognition of the propriety of inducing premature labor, in the interest of both the mother and the child, in cases where the placenta is prævia. A distinguished British obstetrician[5] observes: "We have arrived at a very generally admitted conclusion as to

[1] Dr. William Baynham was long considered the most eminent surgeon in the Southern States, and was particularly distinguished for his accurate knowledge of anatomy.—Thacher's Medical Biography.

[2] American Practitioner, September, 1876.

[3] Memoir of Dr. Thomas C. James. By Professor Hugh L. Hodge, Philadelphia, 1853.

[4] New York Medical Journal, February, 1870.

[5] Dr. Playfair. British Medical Journal, May 4th, 1872.

the danger of temporizing, and as to the advisability of artificially inducing labor as soon as the existence of placenta prævia has been fully determined." It is true Dr. Thomas's publication was in some measure anticipated some six years by a paper from Dr. Greenhalgh,[1] advocating this practice; but he himself was partially anticipated by Dr. A. S. Donkin,[2] who proposed to expedite labor and check the flooding in placenta prævia by inserting into the os uteri a sponge-tent prepared for the purpose. Dr. S. C. Busey,[3] of Washington, has presented an able argument for the induction of labor in uræmia.

Placenta Prævia has been the subject of special study by Dr. J. D. Trask,[4] and by Dr. Wm. Read.[5] Dr. Read was regarded by the highest[6] critical authority in Great Britain as having rendered valuable service to his profession, and considerably enlarged and strengthened the foundations of obstetric science; and it is declared that the impartial and philosophic manner in which he has used statistics for the purpose of deciding grave practical problems is worthy of all praise and imitation.

Dr. Trask, whose prize essay on placenta prævia has been alluded to, has also laid the profession under obligations by quite a thorough study of rupture[7] of the uterus; four hundred and seventeen cases of this accident are tabulated, and important lessons deduced. This monograph ranks by the side of the one by Crosse on uterine inversion.

Dr. John Stearns,[8] of New York, in a letter to Dr. Akerly, published in the Medical Repository, 1807, announced that ergot[9] was capable of exerting a specific action upon the uterus; that it greatly augmented the power of that organ during the efforts of parturition; and that, in lingering and protracted cases, it speedily induced forcible pains, and greatly expedited delivery. And in June, 1813, Dr. Oliver Prescott, in a communication made to the Massachusetts Medical Society, pointed out the value of this agent in *post-partum* hemorrhage. Thus the two most important obstetric uses of ergot were first made known by American physicians. An able paper, by Dr. William Goodell, on Concealed Accidental Hemorrhage,[10] was published in 1869. This paper contained an analysis of one hundred and six cases, and presented the symptomatology more clearly and definitely than had been done before. Diphtheria of Puerperal Wounds has been made the subject of important contributions by Drs. Lusk and Parry.

We now make some brief references to operative obstetrics, including both manual and instrumental.

One of the most valuable contributions to obstetric art ever made was

[1] Transactions of the London Obstetrical Society, vol. vi.
[2] Edinburgh Medical Journal, 1859.
[3] American Journal of Obstetrics.
[4] Prize Essay. Transactions American Medical Association, vol. viii.
[5] Library of Practical Medicine. Published by order of the Massachusetts Medical Society. Vol. xxii. Philadelphia, 1861.
[6] British and Foreign Medico-Chirurgical Review, 1862.
[7] American Journal of the Medical Sciences, 1848 and 1856.
[8] The American Dispensatory. By Dr. James Thacher. Boston, 1817.
[9] *Pulvis ad partum* was the name soon appropriated to the powdered ergot. Hosack, however, shortly substituted for it, *Pulvis ad mortem*. This is suggestive of Guy Patin's remark in regard to an antimonial preparation which was in great favor in his day, known as the *Vinum vitæ*. In one of those letters, first published at Geneva, 1683, of which Voltaire has observed, "they were read with eagerness, because they contained anecdotes of such things as every body loves, and satires, which are liked still more," Patin writes that the *Vinum vitæ* should be called *Vinum mortis*.
[10] American Journal of Obstetrics, August 1869.

that of Dr. M. B. Wright,[1] of Cincinnati, in the year 1854. To him is justly due the credit of originality in the method of such bimanual operation as will in shoulder-presentation succeed in converting it into a presentation of the head. Even if the method of Wright were precisely the same as that of Hicks,[2] still it was published six years before. But the essential of each is in the use of both hands—the external hand, not as by former operators merely to steady the uterus, but to assist the action of the other. True, Dr. Wright applied the method only to what is called cephalic version; Dr. Hicks gave greater prominence to the accomplishment of podalic version.

Next among important manual operations must be placed the postural treatment of prolapsed cord and of shoulder presentations, known in its first application as the method of Thomas, in its second as that of Maxon. In foot-notes I have given extracts from the original work of Deventer, conclusively showing that this treatment was advised by him, though it was pressed with no urgency, and seems to have been theoretical rather than supported by clinical facts. Dr. Bard, in considering podalic version in presentations of the breast, belly, and back, says expressly: "And in all cases of particular difficulty, we may facilitate the operation by a judicious choice of the posture of the woman; or by changing it from the side to the back, or from the back to the knees and elbows. Deventer particularly recommends this position." No one in the face of these testimonies can doubt the priority of the Hollander in the suggestion of postural treatment in these labor-complications. So, too, in Dr. Wright's essay, already referred to, the suggestion of the knee-elbow position is made. Nevertheless, to Dr. Thomas, of New York, and to Dr. Maxon, of Syracuse, is due the credit of having demonstrated by abundant clinical facts the great value of this method in prolapse[3] of the cord, and in transverse presentation, and of establishing it in professional confidence.

A passing notice is due Dr. Goodell's "Management of Head-Last Labors,"[4] in which constant supra-pubic pressure conjoined with continuous traction may so expedite delivery as to greatly diminish fœtal mortality; and Dr. Parry's[5] method of correcting with the hand faulty presentations and positions.

In obstetric surgery no country can claim more brilliant operations than those of the late Dr. William Gibson, Cæsarean section twice successfully performed upon the same subject, the children also saved, and the gastro-elytrotomies of Thomas and Skene.

Of course it is impossible to refer to even a tithe of all the valuable American contributions to obstetrics and puerperal diseases in the limits of this Address—a simple bibliography would consume more than the time allotted it; and a dictionary of dates, with a catalogue of cases, would illy meet the requirements of the occasion. Nevertheless, three contributions in this department demand special notice.

In the year 1843, Dr. Oliver W. Holmes published, in the New England

[1] Prize Essay. Ohio State Medical Society. "Difficult Labors and their Treatment."

[2] Dr. Hicks published his method in the Lancet in 1860, giving five cases of placenta prævia in which he had resorted to bimanual version, and in 1863 (London Obstetrical Society's Transactions, vol. v.), presented a fuller account of it.

[3] Prolapse of the cord has been made the subject of an elaborate monograph by Dr. George J. Englemann, American Journal of Obstetrics, 1873–4.

[4] Philadelphia Medical Times, March 20, 1875.

[5] American Journal of Obstetrics, vol. viii. p. 138.

Quarterly Journal of Medicine and Surgery, a paper afterwards issued in book form, in reference to the contagiousness of puerperal fever. Empedocles combined in one, poet, priest, politician, and physician; and history records that he rescued a city from desolation by blocking up a mountain gorge through which pestilential winds were sweeping. In like manner an American, whom the world so knows as poet, essayist, and philosopher, that, arrayed in these glories, crowned with these honors, we almost forget that he is a physician and medical teacher, once by his bold, decisive utterances, his startling array of facts, his invincible logic, greatly contributed to destroy a pernicious doctrine taught by some of our leading obstetric authors—a doctrine which was more certainly laden with death than the foul wind that was destroying the citizens of Agrigentum.

In 1868, the Obstetric Clinic, a Practical Contribution to the Study of Obstetrics and the Diseases of Women, by Dr. George T. Elliot, was published. Its author, whose talents and culture were admirable, and who, alas, was taken away in the prime of life and power, utters these noble and just words explaining his motives in writing this book, words which ought to be well pondered by all who have similar opportunities. "The work is presented as a partial discharge of the debt due to the profession by all who enjoy hospital advantages; and in grateful recognition of the benefits which the author has derived from the experience of others." Elliot's Obstetric Clinic has taken its place among the classics of our profession, and cannot fail to be of value to any one who consults its pages.

In 1874, the first edition of Dr. F. Barker's Lectures on the Puerperal Diseases was issued, and the second edition the present year. This, too, is a clinical work, and eminently practical. It filled a gap in medical literature, met an urgent need of the practitioner, and was received with great favor, both at home and abroad.

In considering the second division of the subject, an order similar to that observed in the first will be pursued, and accordingly American treatises on Diseases of Women will first be referred to. These works have had the following authors: Dewees, Meigs, Hodge, Byford, Chapman, and Thomas. The first edition of Dewees's work was issued in 1826, the ninth and last in 1847; this contains the revisions and additions made by the author a short time before his death, and may be regarded, therefore, as embracing Dr. Dewees's best utterances upon the subjects discussed in it. To say that the book is badly written, would be a mild way of stating that there are therein frequent instances of violations of the simplest rules of grammar and the plainest laws of rhetoric. But what of the matter? More than one-third of the five hundred and odd pages are occupied with the disorders of pregnancy, puerperal fever, phlegmasia dolens, milk abscess, and hysteria; diseases of the ovaries are discussed in a little more than a page, and the moderately well-behaved irritable uterus of Gooch, becomes a monster of inflammation for the conquering of which general depletion is sometimes, and local depletion is always, necessary. On pages 256 and 257, there is given the history of one of the most strangely managed cases, as seen by the light of to-day, that ever was recorded. In brief, nature tried to extrude from the vagina a uterine fibrous polypus as large as a child's head, and the doctor with might and main resisted, but vainly, the extrusion; and then, instead of dividing the short, thick pedicle, he tried to push the tumor back because the womb was inverted; but the

tenderness of the parts prevented, the tumor became black and offensive, and the next day the patient died.

However, Dr. Dewees did advance the knowledge of the profession in regard to the symptoms of uterine displacements, and the means for relieving them; he taught when and how to use tincture of cantharides in amenorrhœa, and his name will not be forgotten as long as dysmenorrhœa ever suggests the ammoniated tincture of guaiacum. But Dr. Dewees's great fame is that of an obstetrician, not a gynæcologist.

Woman, Her Diseases and Remedies—the work of Dr. Charles D. Meigs—was issued in 1847, and the fourth edition in 1859. The volume is in the form of letters, and, of course, less dignity of style and greater freedom of expression are permitted than in a purely didactic treatise. Here are not only sketches of disease, but also lessons of high morality, of professional honor, and of tender sympathy; here are odd words and odd forms of expression; sometimes ludicrous dialogues, in which refinement and dignity are thrust in the background by realism; a fluent stream, apparently wandering at its own sweet will—now rippling with music and sparkling with sunshine, here narrow and strong, there diffuse and almost wearisome in its slow progress—but still always advancing, and strengthening in the advance; or a picture-gallery (and the æsthetic element in his nature was so strong that, possibly, Dr. Meigs might have been a great poet, or a great painter, had he not been a great physician), in which various representations, persons and scenes are collected, some altogether mean, and given with painful minuteness of sketch and color, others noble in conception and expression, but each filling a destined place, all real, vivid, and combining in a common design and purpose—such are these Letters that never lose their charm. Dr. Condie, who had no patience with anything but the plainest English, severely criticized in the American Journal of the Medical Sciences the style of these letters. But the style was the man; the letters are not less his teachings than they are Dr. Meigs himself.

Undoubtedly the instructions of Meigs were very much in advance of those of Dewees. Was he not, too, the first American at least to describe vaginismus under his horrible designation of sphincterismus? He hated abdominal surgery, and so rejected ovariotomy; cervical surgery, too, and so in stenosis adhered to Mackintosh rather than to Simpson; he clung with devout faith to Gooch's canula in the removal of uterine polypi, and regarded complete perineal ruptures as incurable.

Clinical Lectures on the Diseases of Women and Children, by Dr. Gunning S. Bedford, were published in 1855, the seventh edition in 1862. To this work was paid the high compliment of translation into French and German. With much that is valuable even to-day, and impressively given, there are some things that would be utterly rejected as rules of practice. For example, who would now think of salivating a patient because she had ovarian dropsy, or expect such dropsy to disappear under the magnetic influence of "patting the tumour with the ends of the fingers several times during the day, together with pressure, and the internal administration of muriate of lime." The author's utterances, too, as far as manner is concerned, are at times either simply grandiloquent, or degenerate into tedious and undignified dialogues, or trite commonplaces.

A far better book than any of those yet referred to is the Practice of Medicine and Surgery applied to the Diseases and Accidents incident to Women, by Dr. William H. Byford, published in 1865. Its great merit

is its evidence of faithful observation and reflection; it was the work of a painstaking, conscientious man with large experience, and it has contributed much to the promotion of a knowledge of diseases of women among the American profession. Dr. Byford (p. 348) has proposed and practised an original method of treating uterine fibroids, which is probably deserving of more professional attention than it has received, and he also first made use of tents of slippery elm. Dr. Byford speaks of the "philosophy" of uterine displacements, and in the misuse of this term[1] has been imitated by some others: would it not be quite as correct to speak of the philosophy of boot-blacking or of coat-cutting?

Diseases Peculiar to Women, by Dr. Hugh L. Hodge, the first edition in 1860, the second edition revised and enlarged in 1868, and Hysterology, by Dr. E. N. Chapman, New York, 1872, are two works diametrically opposed to each other, or at least presenting entirely different theories of uterine disease. Dr. Chapman's pathology is congestion, and his local therapeutics are chiefly scarification and nitrate of silver; where pessaries are required, preference is given for those that are globular, and appropriate constitutional treatment is directed. His work is largely clinical, though unfortunately most of his reports of cases are too brief to be of great utility. Dr. Hodge, on the other hand, taught that the condition of uterus characterized by tenderness, congestion, increase of secretion and of size, was not inflammation, and should not be so treated. Here was the irritable uterus rescued from the inflammation of Dewees which was devouring it, and restored to the position which Gooch and Addison meant to assign it; nay, given an importance and extent in uterine pathology which they could hardly have anticipated. According to Dr. Hodge, "displacements of the uterus are the most frequent cause, original or secondary, of irritable uterus;" and "the mechanical treatment of uterine displacements by intra-vaginal supports is essential, a *sine quà non* for their perfect relief."

Whether accepting in full, or only in part, the views of Dr. Hodge upon uterine pathology, no one can deny the great service that he rendered uterine therapeutics in the device of his lever pessary, an instrument in such common use in all lands where diseases of women are studied. His pessary was no happy accident, but the result of much thought and of many experiments; as he himself once expressed it, he had hundreds of abortions before producing it. "Great poetry, great philosophy, great scientific discovery, every intellectual production which has genius, work, and permanence in it, is the fruit of long thought, and patient and painful elaboration."[2] Not only with the form of the pessary, but with the material, is Dr. Hodge to be credited. No man ever accomplished as much with these instruments, or explained their modes of application and their utilities more clearly and completely. He showed their value not only in relieving the ordinary results of uterine displacement, but also in curing sterility, and in the prevention of abortions when these, too, were consequences of such mal-positions. So, too, he demonstrated their value as repositors, gradually replacing the uterus when in mal-position. He

[1] Sir William Hamilton, Discussions on Philosophy and Literature, remarks upon the vague universality which is given to the terms *philosophy* and *philosophical* in common English—an "indefinitude" limited especially to Great Britain: "Mathematics and physics *may* here be called philosophical sciences; whereas, on the Continent, they are excluded from philosophy, philosophy being there applied emphatically to those sciences which are immediately or mediately mental."

[2] Froude's Address to the Students of St. Andrews, March 19, 1869.

showed how useful they were, too, in relieving some of the urgent symptoms of uterine fibroids.

But the American work on diseases of women most widely known is the Practical Treatise of Dr. T. G. Thomas, published in 1868, and passing to its fourth edition in 1874. As a clear, concise, and practical exposition of gynæcology, it has no superior. At home, its merits have been so fully appreciated by the profession that nearly twenty-five thousand copies are now in their hands, helping them in their daily work—a success which, when we compare it with that of even popular[1] literary productions, is simply astonishing. Abroad, its great merits have been fully acknowledged by the leading journals of the profession, and it has been translated into French, German, Spanish, and Italian. Some of Dr. Thomas's views as to uterine diseases, and some of his special contributions to gynæcology, will be referred to in the next topics presented.

Laying aside the briefer mongraphs, whether appearing in the form of reports in the Transactions of Medical Societies, lectures, addresses, etc., the *libelli* of our literature, and contributions to our medical journals—all these numbering not merely hundreds but thousands, and of course an analysis of which, even if it might be instructive, would be impossible —four works relating to diseases of women, demand especial attention. These are Clinical Notes on Uterine Surgery, by Dr. Sims (New York, 1869); Vesico-Vaginal Fistula, by Dr. Emmet (New York, 1868); General and Differential Diagnosis of Ovarian Tumors, by Dr. Washington L. Atlee (Philadelphia, 1873); and Ovarian Tumors, their Pathology, Diagnosis and Treatment, especially by Ovariotomy, by Dr. E. Randolph Peaslee (New York, 1872).

The Uterine Surgery was published not only in New York, but also in London, and a French translation appeared in Paris. It met, as it deserved, with much favor at the hands of the profession, although some parts of it did not escape[2] severe criticism. One of the leading British journals[3] spoke in these terms of the book: "It is a collection of bedside studies of the uterine diseases of which it treats, notable for their accuracy, and continually challenging our admiration for the sagacity and originality of their author, the fertility of resource, the unfailing patience, the successful adaptation to new purposes of simple means, the exact perception and clear statement of the vital points of every case." "For the first time in the history of medicine, the sterile condition is here subjected to a full and philosophical analysis, complete as far as the advanced knowledge of our time permits."

Dr. Emmet's work on Vesico-Vaginal Fistula was essentially clinical in character, the history and treatment of the seventy-five cases given, representing the author's own invaluable experience. "The results of the numerous cases related are in the highest degree honorable to Dr. Emmet's skill as an operator, and also to American surgery."[4]

Of the two admirable volumes upon Ovarian Tumors referred to, one embodies the thirty years' experience of a surgeon who ranks as one of the world's most celebrated ovariotomists, and is a treatise upon the general and differential diagnosis of these tumors. The other is more ambitious in design, and wider in scope. More largely a work of com-

[1] Mark Twain's Innocents Abroad, for example, has reached a sale of two hundred thousand.

[2] London Medical Times and Gazette, 1866. British and Foreign Medico-Chirurgical Review, January, 1867.

[3] Lancet, February 3, 1866.

[4] Lancet, February 20, 1869.

pilation and of analysis, than a record of personal experience, it exhibits the indefatigable research, the patient investigation, and the accurate judgment of its scholarly and erudite author. Without being encumbered, or possibly for some enriched, with so many details, it has much of the encyclopedic character of Gallez's volume,[1] and not only is a most useful guide in the diagnosis and treatment of ovarian tumors, but is valuable for reference.

Special contributions to gynæcology, other than those just referred to, can be better considered under the heads of particular disorders. But before entering upon this division of our subject, two or three preliminary observations may be permitted.

The first is, that while in this country specialism in other departments of medicine has made rapid progress, in gynæcology few, and until quite recently only two, specialties are found. And this arises from the fact that the diseases of women must, in the nature of things, be chiefly under the charge of the general practitioner. What is the numerical relation between those diseases that are constitutional in their origin and amenable to general treatment, and those that are local and require local treatment, is a question that would have different answers, according as addressed to the general or the special practitioners. That there were extreme surgical tendencies of uterine pathologists, was the complaint a few years since of one[2] of the most eminent of British gynæcologists; and that this is true to-day, in this country, will be very generally admitted. There is something so fascinating in surgery, so demonstrative and demonstrable—that which the eye can see and the fingers touch—as to inspire the operator with ambition and the patient with hope.

Undoubtedly the danger of specialism is an undue exaltation of local disease and of local therapeutics. The general practitioner, on the other hand, is liable to depreciate the former, and then of course the latter. He does not extend himself, as Mill said of Jeremy Bentham, infinitely in one direction, but must be many-sided, undergoing a more general and complete development. Thus, other things being equal, he will be better able to detect the relations between local and general disorder, what in a given case is the antecedent and what the consequent, what may be cause or effect, unless indeed the specialist, as I believe is true of all who are devoted to gynæcology in this country, has chosen his department after large experience in general practice. The pine in our Southern forests grows stately and tall, lifting its tufted head far up to the clouds, but it is almost bare of boughs, and barren of foliage and fruit. "The unwedgeable and gnarled oak" is knit together with a stronger fibre. More firmly rooted, it spreads broadly on every side great branches, with dense foliage, ample shade, and abounding fruit.

The American development of gynæcology has been largely due to the labors of men engaged in general practice. Considering now that development, brief reference will be made first to the treatment of some of the positional disorders of the uterus.

The pessary of Hodge has already been spoken of; an instrument which, both as to form and material, has received the general and grateful acceptance of the profession everywhere. Modifications or additions, more or less valuable, of the lever pessary have been made by Drs. Albert

[1] Histoire des Kystes de l'Ovaire, etc. Brussels, 1872.
[2] Dr. E. J. Tilt. Transactions of the London Obstetrical Society.

H. Smith, Cutter, Thomas, and others. Nor must we omit, in this connection, a passing reference to the very interesting observations of Dr. H. F. Campbell, of Georgia, upon pneumatic pressure in the treatment of uterine displacement.

Turning to the greatest of all the positional disorders of the uterus—inversion—we will find that American operators may challenge comparison with those of any country in regard to ingenuity, skill, and success in its cure.

Inversion of the uterus,[1] an accident first noticed by Celsus,[2] was, until quite recently, an opprobrium of the profession, since replacement of the organ, unless efforts were made almost immediately after the accident, was regarded as impossible. The few instances where a chronic inversion was reduced, were regarded as happy accidents rather than as indicating a rule of practice. On April 13, 1858, Dr. Tyler Smith observed, in reporting to the Medico-Chirurgical Society[3] a case of uterine inversion of nearly twelve years, which he had reduced: "Hitherto inversion of the uterus has been treated either with styptics and astringents, or the inverted organ has been removed by ligature or excision. The instances in which reinversion has been accomplished have been few in number, and chiefly limited to cases of recent origin." On the 12th of March of the same year, Dr. J. P. White,[4] of Buffalo, reduced an inversion of nearly six months' duration. The one used elastic pressure and taxis; the other taxis and pressure[5] with a bougie. These successful cases were the heralds and guides of numerous similar successes at home[6] and abroad.

In this country the most important new methods of accomplishing reduction were that of Noeggerath, indenting one or other or both of the cornua, and thus starting the movement of restoration; that of Emmet, using silver sutures to hold in place a partial reduction; and those of Thomas, consisting, the one of *mediate* dilatation with a boxwood cone pressed down from the abdomen into the constriction, the other of abdominal section, and then *immediate* dilatation; and the repositor of Dr. White. Abdominal section, with immediate dilatation, has been twice performed—once with complete success, and once with a fatal result—but fails of professional endorsement. The repositor of Dr. White is an admirable instrument, as any one who has ever used it faithfully will testify: no one comparing it with the "drum-stick" repositor of Depaul

[1] Dr. Isaac E. Taylor has proposed and ably sustained the view that spontaneous inversion of the uterus sometimes commences at the os; a view which is strengthened by the cases of spontaneous reduction, the most extraordinary of which has recently been reported (American Practitioner) by Dr. Chesnut, of Lafayette, Ind. In Dr. Chesnut's case the restoration was twelve years after the accident.

[2] I am indebted to Dr. J. S. Billings for the following reference: Celsus, A. Cor., Medicinæ Libri Octo; Ed. by A. Lee, London, 1831. Lib. I. Præfatio, p. 15. "Cum aetate nostra quaedam, ex naturalibus partibus carne prolapsa et arente, intra paucas horas exspiraverit; sic ut nobilissimi medici neque genus mali, neque remedium invenerint. Quos ego nihil tentasse judico, quia nemo in splendida persona periclitari conjectura sua voluerit; ne occidisse, nisi servasset, videretur," etc. I find that several have commented on this passage, and especially Morgagni, who decides it to be a case of inversion of the uterus. Respectfully and truly yours, J. S. BILLINGS.

[3] Medico-Chirurgical Transactions, vol. xli.

[4] American Journal of the Medical Sciences, July, 1858.

[5] Dr. Thomas is in error when he states (Diseases of Women) that taxis alone was used by Dr. White, as can be seen by reference to the original account of the operation.

[6] Dr. White has been successful in twelve cases, and in one of the twelve the inversion had existed for twenty-two years.

would hesitate to give a decided preference to the former. Its value will become more and more appreciated as its use becomes more general.

Two of the most useful therapeutic agents in uterine disease, the one suggested by Dr. Sims, the other by Dr. Emmet, are among the simplest, viz.: glycerine[1] and hot water.[2] Dr. Emmet began the use of hot water injections in 1859, and observes, "I have been so thoroughly identified with this mode of practice, that it seems scarcely necessary to claim the priority. Certainly, no one in this country is on record as an advocate for the practice previous to myself; and so far as I have been able to ascertain, the same is true in regard to the practice of gynæcologists abroad." Confirmatory of Dr. Emmet's views as to the action of hot water injections in producing contraction of bloodvessels, and therefore useful not only in uterine, but also in pelvic, congestion, and in threatened cellulitis, was the practice of Trousseau[3] in menorrhagia—frequently directing two or three very hot injections daily. If Dr. Emmet had done nothing beside this, his name would deserve to be one of the most honored of the century in medicine.

Since the invention of the hysterotome by the illustrious Sir James Y. Simpson, and of its many modifications or substitutes by others, the cervix uteri, relieved from the potash-persecutions which, carried on by the ultra followers of Dr. Henry Bennet, a few years ago, threatened its integrity, has been made the subject of numerous cutting operations for the relief of dysmenorrhœa, or of sterility. Those who have contributed most to the practical study of these operations are Drs. Sims, Emmet, Pallen, Peaslee, and Worster, of New York, and Reamy of Cincinnati. There is something wonderfully fascinating both for surgeon and patient, when the former can carry salvation from dysmenorrhœa, and also a baby, upon the sharp edge of a bistoury, or between the blades of scissors! Unfortunately a conflict exists, both in theories and in experiences, as to the utility, the modes of, and the cases for, operation. It is to be hoped that since the recent publication of Dr. Peaslee, so conservative in its character, there will be elicited such discussion as will fully present the truth in this matter, and firmly establish practical rules for general professional guidance.

In amputation of the cervix uteri the flap method of Dr. Sims, and the use of the galvano-cautery of Dr. Byrne, are worthy of remembrance. Fluid applications to the lining membrane of the uterus are regarded by the profession everywhere as of great utility; and the safest and most common way of making such applications is simply one in close imitation of that originally devised and practised by the late Dr. Henry Miller,[4] of Louisville, Ky., more than a score of years ago.

Uterine fibroids in their frequency, in their exacting symptoms, in their sometimes apparently capricious history, and in the utter uncertainty of therapeutic means pursued for their relief, have long presented to the gynæcologist a field at once inviting and repellant. Electrolysis, studied in this relation by Neftel, Sims, Cutter, Kimball, and some others, cannot yet be said to have been admitted to professional confidence. Dr. Byford has ably collected most of the experience of the profession of this

[1] Sims's Uterine Surgery, pp. 21, 72.
[2] New York Medical Journal, June, 1874.
[3] Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques, vol. xxii. p. 452.
[4] Others whose names might be mentioned in connection with the study of means for making such applications are Emmet, Lente, Nott, and Reamy.

country in the hypodermic use of ergot of Hildebrandt. But this treatment frequently fails, and the knife is occasionally required.

Those whose names in this country are especially identified with operations for the removal of these tumors, are Dr. Atlee,[1] Dr. Sims, and Dr. Emmet. The most valuable original observation belonging to the last is the pedunculization of a fibroid which can be effected by traction. The important operations performed by the others, and the ingenious instruments devised for this by Dr. Sims, almost as prolific of instruments as Hudibras[2] of tropes, we pass by with this brief reference.

Removal of the uterus by abdominal section has been performed several times in this country, but most of these operations have not been successful. Probably the first case in this country, certainly one of the first, was that of Dr. Thomas G. Prioleau,[3] of Charleston, S. C., in the year 1819. The tumor, prior to the operation regarded as malignant, was very large, its removal with the uterus attended with much hemorrhage, and the patient died in twenty-four hours. On the 16th of April, 1850, Dr. Paul F. Eve removed the uterus for malignant disease, and the patient lived for between three and four months. The operation has, however, been done on account of fibroid disease, with permanent recovery, by the Atlees, Burnham, Kimball, H. R. Storer, Thomas Wood,[4] and some others.

Removal of the prolapsed uterus—the vaginal operation—has several times been successfully performed. One of the most interesting of these cases is that of Dr. S. Choppin, of New Orleans. The doctor, in 1861, removed not only the uterus, but with it the left ovary and Fallopian tube, which were also prolapsed, and the patient rapidly recovered.

Many other topics in this division are suggested, but only two can be considered as the greatest glories of American gynæcology, the operation for genito-urinary fistula, and that for the removal of ovarian tumors. Holding up these to the world, she may, with just pride, exclaim: These are my offerings to Humanity and to Medicine! Yea, these are MY offerings!

But is not the operation for genito-urinary fistula a triumph of American surgery? Up to less than a quarter of a century ago, how deplorable the condition of the unfortunate woman suffering from such a lesion! She had entered upon all the remaining sad and weary years of her life, through a portal over which surgeon and obstetrician alike had, time and again, witten: Let her who enters here dismiss hope. If here and there, now and then, a case was cured, the result was the exception, failure the rule. If Jobert, for example, cured some, his operation always failed in the hands of others. Now, how changed! An operation, known abroad as the American method, rescues these unfortunates from their infirmity, and restores them to society. Failure is now as rare as success was formerly.

Of course surgeons had come to apprehend the principles, before they had attained the method of cure.

[1] Transactions of the American Medical Association, 1853.

[2] "For rhetoric, he could not ope
His mouth, but out there flew a trope."

[3] Dr. Prioleau graduated in Philadelphia in 1808. Drs. Samuel Jackson and W. P. C. Barton were classmates. At the organization of the Medical College of the State of South Carolina, in 1824, he became Professor of Obstetrics, and filled the chair for many years. I am indebted to Prof. J. Ford Prioleau, the present occupant of the chair, for the statements given.

[4] Dr. Wood's cases are seven, with three recoveries.

For the closure of these fistulous openings two things were necessary. First, their margins must be thoroughly and uniformly freshened. Second, this vivification accomplished, the freshened surfaces must be brought and retained in accurate apposition until union took place. But how should these edges be pared so completely and accurately? The first step for the accomplishment of this end was their thorough exposure. This step was taken in the position and speculum of Marion Sims. Here we have the very foundation of the operation. This fixed, knives of various angles, forms, and sizes, or curiously curved scissors, became mere minor matters, resting with the individual operator, or determined by the exigencies of each individual case.

But how are the prepared fistulous margins to be brought and kept in contact until nature binds them fast? Here again we hear, in 1852, the voice of Marion Sims crying, and crying aloud and on, until the whole world has heard Silver Sutures. Am I told that a British surgeon, in 1834, using silver gilt sutures, cured a case of vesico-vaginal fistula? He did not follow up this one success, and confirm it by others. He established no rules of practice, he instituted no method, and his report was unnoticed, it was as the voice of one crying in the wilderness. That one success, too, was it any more than an American, Mettauer, of Virginia, had gained in 1830, with lead sutures? Was it as much as Hayward, of Boston, accomplished with silk sutures? But are the brilliant results which Simon obtains, adduced to disparage the American method? Simon's operation has become the property and the practice of the profession scarcely more than Jobert's. We gratefully record the statement of one of the most eminent of French gynæcologists, Courty,[1] that the American school has no higher claims to future celebrity than the operation for vesico-vaginal fistula.

It is needless to mention the various modifications made by Dr. Sims in his original operation, or the valuable improvements introduced by Dr. Emmet, or the peculiarities of the method of Dr. Bozeman, who has labored with such signal zeal and ability, both at home and abroad, in this department of surgery, or the additions made by Dr. Mastin, of Alabama; or by Dr. Schuppert, of New Orleans; or by Dr. Battey, of Georgia; for the American method remains essentially that of Dr. Sims. Nor is it necessary to speak of Dr. Sims's early trials, patient perseverance, and triumphant success in working out the great problem to which, more than thirty years ago, he devoted himself with all that he had. The world knows them, and he has received honors such as have been accorded to few representatives of American Medicine.

A beautiful fable of the old Greeks made Apollo, the god of healing, give to the laurel tree its evergreen leaves, and consecrate them as crowns for the brows of victors. And surely who so worthy of the present of these crowns as a son of Apollo, who, after years of bitter struggles, has gained a victory which has brought blessings to thousands, and will bring them to tens of thousands more?

Is not ovariotomy one of the rightful glories of American gynæcology? What are the facts? In the year 1809 a village doctor of Kentucky successfully removed an ovarian tumor, and from that time until his death in 1830, operated in all thirteen times with eight recoveries[2]—a success, by

[1] Traité Pratique des Maladies de l'Uterus.

[2] For the collection of Dr. McDowell's cases, the profession is indebted to Prof. S. D. Gross.

the way, which, compared with the first thirteen cases of Baker Brown, or Spencer Wells, is most extraordinary. Spencer Wells, whose operations began in 1857, lost four out of the first thirteen, and Baker Brown lost eight out of his first thirteen.

Is the priority of Ephraim McDowell as the first surgeon to remove the diseased ovary invalidated by the case of Houston, of Glasgow, in 1701? All other claims to priority have been swept away as blunders or shams, and this, too, would need no notice, had not Dr. Washington L. Atlee republished it as a proof that ovariotomy "originated with English surgery," and had not Spencer Wells asserted that it "undoubtedly strengthens the claims of British surgery to the honor of originally practising ovariotomy."[1] It is strange that this case, rejected by Boinet and Clay,[2] should be now adduced. The operator simply incised a cyst and evacuated its contents. He no more performed ovariotomy than he who lances a suppurating parotid has extirpated the gland. The claim of British surgery to the honor of originating ovariotomy is weak beyond expression if it rests upon this case.

Again we are told, "But the operation was suggested by William Hunter; its practicability and the mode of performing it were taught by John Bell." William Hunter said: "I am of opinion that excision can hardly be attempted." He adds, however: "If we could beforehand know that the circumstances would admit of such treatment, the incision should admit of only two or three fingers, and the cyst be tapped and drawn out, that the surgeon may cut the pedicle without introducing his hand." But Tozzetti,[3] in 1752, Vanderhaar and Delaporte, in 1752, and Morgagni, in 1761, gave more encouragement to ovariotomy than did William Hunter; some of them as much, indeed, as John Hunter did, in 1786. The suggestions of Hunter and the instructions of Bell doubtless had an important influence upon McDowell's mind, but this detracts nothing from the glory of his achievement. The fame of Columbus is not dimmed by the fact that others before him, others in his time, believed with him that by sailing westward a sea-way to the Indies would be found. No matter what surgeons may have believed and suggested as to removal of diseased ovaries, no matter though John Bell taught the mode of operating, their faith without works was utterly dead, and the new Columbus started upon his exploration without pilot or chart.

But then, declares Nélaton, McDowell's first patients were negresses, and Gallez follows him, adding that his effort was to save the lives of slaves, who at that time commanded a high price, so that surgery owes this brilliant conquest to the cupidity of planters. How strange that historical facts of the present century can be so misrepresented. The truth is, that Dr. McDowell's first patient[4] was not a colored woman, but one of his own race and social position.

[1] Diseases of the Ovaries.

[2] Comme le fait judicieusement remarquer Boinet, après J. Clay, il y a eu simple incision du kyste, qui n'a été lié, ni excisé: on ne peut donc voir là une opération d'ovariotomie proprement dite. Gallez, op. cit.

[3] Gallez, op. cit. p. 404.

[4] This was Mrs. Crawford, who, after the operation, resided for a time at Madison, Ind. From the late Dr. Speer, of Hanover, Ind., who, when a young man, was for a time a member of her family, I received some years since a statement as to the efforts made by Dr. Ephraim McDowell's nephew to wrest the honors of the operation from the uncle. A lawyer from Lexington, Ky., visited Mrs. C., urging her to sign a paper stating that the nephew was the operator. Her reply was that she was blindfolded, and could not positively assert that Dr. Ephraim McDowell was the operator, but that she never would have consented to the young man's operating.

Surely British surgery has glory enough in the splendid successes of Spencer Wells and Keith, and the French in those of Péan, without either abating one jot or tittle of the fame of Ephraim McDowell.

Only sixty-five years since the performance of his first ovariotomy, only fifty-eight since its publication, and then many years before general professional recognition of the operation as legitimate, and probably it is not extravagant to assert that more than two thousand women have been by it rescued from impending death! Dr. Peaslee, writing in 1872, concluded, from a careful calculation, that ovariotomy had, in the United States and Great Britain, within the last thirty years, added more than thirty thousand years to the active life of woman. We may, indeed, assert most unequivocally that the name of the father of ovariotomy is worthy of being recorded with those of the best benefactors of the race.

Dr. McDowell's third ovariotomy was in 1816. In that year, or in the one preceding, Dr. Thomas G. Prioleau, whose extirpation of the uterus has already been referred to, assisted by his nephew, Dr. Philip Prioleau, and Dr. Frost, attempted ovariotomy, but was compelled by extensive adhesions to abandon the attempt; he therefore simply evacuated the cyst, and excised a portion of it. The patient died. Dr. Nathan Smith, of Connecticut, Dr. Alban G. Smith, of Kentucky, Dr. David L. Rogers, of New York, and Dr. Billinger, of South Carolina, are among the early American ovariotomists. To give a list of those who have operated since, including as it would numbers of the profession in almost every State in the Union, and in some more than a score of them, is impossible. Those who have operated most frequently are Atlee, Kimball, Dunlap, Burnham, Peaslee, Thomas, White, Sims, and Bradford. The last named, the late Dr. J. T. Bradford, of Augusta, Ky., had a higher percentage, no less than ninety per cent., of recoveries than any other operator in the world. Those who have contributed most, by the publication of statistics of the operation, or arguments in its behalf, to its proper appreciation, are the late Dr. Pope, of St. Louis, Dr. Lyman, of Boston, Dr. Atlee, the late Dr. Miller, of Louisville, Ky., Dr. Peaslee, and Drs. Hamilton and Reeve, of Ohio. In regard to the diagnosis of cystic diseases of the ovary, no more important addition has been made than the examination with the microscope of the contained fluid by Dr. Drysdale.[1] Dr. Atlee has established important rules of diagnosis from the coagulability of the fluids obtained from uterine or ovarian cysts, or from the abdominal cavity.[2]

[1] DEAR SIR: Since reading the paper at St. Louis, in 1873, "On the Granular Cell found in Ovarian Fluid," I have continued the investigation of these and other dropsical fluids, and have now examined over a thousand specimens of them. These examinations enable me to emphasize the opinion which I then expressed, that the ovarian granular cell is pathognomonic of cystic disease of the ovary. Yours, very truly, T. M. DRYSDALE.

[2] (1) Fluids, drawn from the peritoneal cavity, or from ovarian or broad ligament cysts, do not coagulate on mere exposure to air. (2) Fluids, drawn from non-inflammatory accumulations in the peritoneal cavity, will coagulate more or less under the influence of heat and nitric acid. (3) Fluids, drawn from inflammatory accumulations, will coagulate by heat and nitric acid; and will also, by standing, produce a fibrinoid deposit in small quantities without heat and nitric acid. (4) Fluids of ovarian cysts proper, will coagulate to a greater or less extent by heat and nitric acid. Certain fluids, however, may collect in the parenchymatous structure of an ovarian tumor, which will neither coagulate by heat or nitric acid, nor on mere exposure to air. (5) Fluids of cysts of the broad ligament will not coagulate by any means. (6) Fluids, drawn from fibro-cystic tumors of the uterus, are distinguishable from all others taken from the abdominal cavity by coagulating rapidly on exposure to air, and, after standing, by separating into clot and serum. This fluid, when not stained with red blood, is very transparent and of a yellowish color, and is really blood, or liquor sanguinis *minus* the red corpuscles. WASHINGTON L. ATLEE.

In the operation itself the most important advance is the enucleation of the cyst according to the method of Dr. Miner, of Buffalo. This method is not of general, but of particular, application, and permits the safe removal of tumors that otherwise could not be dealt with in consequence of adhesions. Clamps, instruments used probably by only a minority of American operators, have been devised by Atlee, Storer, Dawson of New York, and Mears of Philadelphia. Peritoneal drainage after operation has been practically studied by Kimball, Atlee, Sims, and Thomas. Dr. Peaslee has shown the utility of intra-peritoneal injections in septicæmia, regarding them as more valuable than any other and all other means.

Vaginal ovariotomy originated with Dr. Thomas, in 1870. The same operation has since also been successfully performed by Dr. Davis, of Pennsylvania, Dr. Gilmore, of Alabama, and Dr. Battey, of Georgia.

Ovariocentesis Vaginalis has been greatly improved by Noeggerath, and the results he has had are remarkably favorable.

Removal of the ovaries in order to determine the menopause in certain cases where menstruation is attended with great suffering, and otherwise incurable disease, was the proposition of Dr. Battey. The operation has been performed sixteen times,[1] with three deaths, the operators being Dr. Battey, Dr. Sims, and Dr. Thomas. Its propriety is hardly established in the face of such statistics.

Did time permit, many other contributions to operative gynæcology might be alluded to, such as that of Nott,[2] extirpation of the coccyx for coccygeal neuralgia, in 1844; Schuppert's[3] operation for obliteration of the vagina, in 1858, "the first case of vulvar occlusion by elytro-episiorraphy without ulterior accidents;"[4] and Emmet's method of securing the restoration of the anal sphincter in operation for ruptured perineum, a method which has recently been successfully followed by a surgeon of Rouen.[5] So, too, we might mention the peculiarities of Dr. Bozeman's operation for vesico-vaginal fistula—his knee-chest support, his self-retaining speculum, his button sutures, his method of auto-plasty by gradual approaches,[6] and, above all, his past successes at home, his present abroad, might be mentioned. The merit of a method will be at once conceded when it has won the approval of such men as Simon, Braun, and Dolbeau.

One of the minor, but by no means insignificant advantages of the establishment of a method of cure for vesico-vaginal fistula, that must not be omitted, is that in consequence thereof the operation of vaginal lithotomy is relieved of its most serious objection—the resulting fistula. Now surgeons, following the example of Dr. Sims, in 1850, remove the calculus by vagino-vesical incision, and immediately close the opening with silver sutures. This operation is growing in favor; in this country forty-one cases have been collected by Dr. Mastin: "the operation,"[7] says Dr. Warren, "seems to have been done more frequently in this country

[1] Dr. Trenholme, of Montreal, and Dr. Peaslee, have reported cases of the operation since the delivery of this address.

[2] American Journal of the Medical Sciences, October, 1844.

[3] A Treatise on Vesico-Vaginal Fistula, New Orleans, 1866.

[4] Le Double, Du Kleisis Génital, et principalement de l'Occlusion Vaginale et Vulvaire dans les Fistules Uro-Génitales. Paris, 1876.

[5] Annales de Gynécologie, July, 1876.

[6] New York Medical Record, August 26, 1876.

[7] Boston Medical and Surgical Journal, July 20, 1876.

than in any other." Hybord,[1] in considering the relative advantages of lithotrity, dilatation and lithotomy, observes that the latter, especially vesico-vaginal, with immediate suture, is preferable if the calculus is hard, large, or encysted.

But turning from these and many other topics, and concluding this division of the subject, we cannot refrain from a remark or two concerning the surgical development of American Gynæcology. Of course in this country, as in others, the advance has been much greater in the surgical than in the medical department of so-called uterine therapeutics. In explanation of this fact, as it is observed here, two peculiar causes have been in operation, in addition to those which are common. One of these is in the characteristics of the American mind—its tendency is to action rather than to reflection; quick and fertile in expedients, it seeks immediate results, rather than exercises the patience for recondite investigation. And again, most of those who have made themselves especially famous, and therefore have become leaders of sentiment and of action, have acquired their celebrity and influence by brilliant operations. An inspiration comes from the graves of the dead and from the deeds of the living, kindling hope and ambition, to emulate their fame, and to do their works, or even greater works. American Gynæcology has proved its power, and recorded triumphs that cannot perish. The past is secure. The future is the child of the past, and its glories may be more numerous and grander.

"Men, my brothers, men the workers, ever reaping something new;
That which they have done but earnest of the things that they shall do."

Let us hope that, as Uterine Surgery has in this country made such great progress, and accomplished so much, the future historian will record similar progress and a corresponding development of Uterine Medicine.

The first American work on Diseases of Children, entitled, "A Treatise on the Physical and Medical Management of Children," was by Dr. Dewees, and published in 1825. Up to 1842 no less than seven editions had been issued. "To Dr. Dewees we are greatly indebted," said Dr. Hodge, "simply for fixing attention on the physical management of children, independently of the high value of his directions; for, prior to this period, the profession in this country left the details almost exclusively in the hands of nurses and midwives, with all their tormenting ignorance and officiousness." Dr. John Eberle, one of the first professors in Jefferson Medical College, then a professor in the Medical College of Ohio, and finally in the Transylvania School at Lexington, Ky., where he died in 1838, was author of a Treatise on the Diseases and Physical Management of Children, which was published in 1837. A third edition appeared in 1845. Dr. Eberle was a compiler, but was always judicious in his compilations.

In 1841, the Practical Treatise on Diseases of Children of Dr. James Stewart was published. This was, according to Dr. Condie, "certainly superior, in many points of view, to those heretofore accessible to the great body of our profession in this country." The author refers to the fact that opium is of admirable benefit in many of the inflammatory complaints of children, especially after bleeding, and was much in use by the older American practitioners. The practitioner of to-day would use the

[1] Des Calcules de la Vessie chez la Femme et les Petites Filles. Paris, 1872.

opium much oftener than he would the bleeding. The year 1844 brought another Practical Treatise on Diseases of Children, the well-known volume of Dr. D. Francis Condie. The sixth edition of it was issued in 1868.

Dr. Condie was, according to Prof. Stillé, by long and extensive experience, accurate observation and diligent study, not only of English writers, but also of the Continental, and especially of the German, well qualified to prepare such a work. The volume grew with each new edition, especially in accretions from foreign sources, and one who reads it now will, without the greatest care, occasionally get lost in conflicting pathological opinions and theories. But no one can mistake the two important characteristics of the therapeutics—great caution in the use of opium, great faith in the power of mercurials. Few American physicians have equalled Condie in medical learning, and the hundreds, possibly I might say thousands, of pages of criticism he has contributed to the American Journal of the Medical Sciences, constitute an important feature of our national medical literature. He was remarkably fluent in composition, yet no one could say—

> "His talk was like a stream which runs
> With rapid course from rocks to roses."

Rocks, big, solid rocks, enough there were in his writing, but roses fared as ill at his hands, as the poppy-heads that fell before Tarquin's angry cane.

In 1848, A Practical Treatise on Diseases of Children, by Dr. J. Forsyth Meigs, was issued by Lindsay and Blakiston. This was the third of the series of manuals which this well-known firm engaged in publishing, the first being the work on Obstetrics, by Dr. Tucker, to which reference was made in a former part of this address. Meigs on Diseases of Children has been and still is held in much esteem by the American profession. It has passed to the fifth edition, and in the preparation of the two last editions Dr. Wm. Pepper has been associated with Dr. Meigs.

A Treatise on the Diseases of Infancy and Childhood, by Dr. J. Lewis Smith, was issued in 1869; the third and latest edition appeared in 1876. "Smith" is a book of fewer pages than "Meigs and Pepper," but clearly and concisely written. As a guide for the practitioner, and as a text-book for students, it hardly has a superior.

In 1849, Dr. John B. Beck, one of the first professors in the College of Physicians and Surgeons, New York, published his work on Infant Therapeutics, which Dr. Gross[1] recently pronounced "a perfect gem in its way." The late Dr. C. R. Gilman[2] states that "it was received with the greatest favor, both at home and abroad. Few medical books of its size contain an equal amount of sound learning and practical good sense."

Observations on certain of the Diseases of Young Children, by Dr. Charles D. Meigs, was issued in 1850. Here we have, in the midst of excellent and most useful instruction as to the management of several of the diseases of children, some of the author's peculiar pathological views, views which were always ingenious, and, if not always true, never failed of the semblance of truth. Here we are taught the efficacy of a woollen cap in coryza, the "right-side treatment" of cyanosis, and are brought face to face with that "endangium" which played so important a part in his theories of disease.

[1] History of American Medical Literature. [2] American Medical Biography.

In Dr. Bedford's valuable Clinical Lectures, already referred to, some of the diseases of children are treated of plainly, practically, and wisely, but there is by no means a complete course given. Indeed, in reading this volume, and finding here and there these diseases considered, while the great body of the work is occupied with diseases of women, one is reminded of a street-car or omnibus crowded with adults, and then here and there a child interposed or superposed.

Having thus briefly noticed these different volumes, we shall next refer to a few special American contributions to Pædiatrics. Among the earlier, must be placed the letters on Angina Trachealis, by Dr. Richard Bayley, of New York, 1781, to Dr. William Hunter. Dr. John W. Francis[1] speaks highly of these letters, and states, "we are justified in giving to Dr. Bayley the merit of being the first writer who understood the nature and treatment of croup." In a letter written to Dr. Fothergill by Cadwallader Colden, New York, October 1, 1753, the writer describes diphtheria and its treatment, chiefly as observed by Dr. Douglass, of Boston. In 1736, Dr. Douglass published a pamphlet[2] entitled, The Practical History of a new Epidemical Eruptive Miliary Fever, with an Angina Ulcusculosa which prevailed in New England in the years 1735 and 1736. "The first full description of this affection published in this country," says Dr. J. F. Meigs, "was by Dr. Bard, and based on an epidemic which occurred in 1771. The views advanced in his paper have been universally recognized, even to the present day, as most clear and just." Dr. Bard's account of the disease has been recently very favorably referred to and quoted by Loraine & Lépine.[3]

Shall we mention one of the first applications of electricity as a therapeutic agent, in this country; its successful use in 1752 by the illustrious Franklin in a case of convulsions, in a patient of Dr. Cadwallader Evans, of Philadelphia, a girl of fourteen, who had been afflicted for ten years?

In regard to the treatment of croup, four important therapeutic agents have been introduced by American physicians. The mercurial treatment is attributed to Dr. George Monroe, of Delaware, who graduated at the University of Edinburgh in 1786. Dr. Hubbard, of Maine, is to be credited with first using and advising the yellow sulphate of mercury, a remedy which has received from Dr. Fordyce Barker, after using it twenty-eight years, the highest possible praise. The late Dr. Charles D. Meigs was the first to advise the common alum as an emetic, while more recently the treatment by cold has been ably advocated by Dr. Jacobi.

The most valuable contributions to the subject of umbilical hemorrhage have been by three American physicians, Drs. Minot, Stephen Smith, and J. Foster Jenkins.

The subjects of masturbation and hysteria in young children have been elaborately presented by Jacobi, whose many and valuable contributions to Pædiatrics are so well known to the profession, not only at home but abroad

Dr. R. W. Taylor[4] has produced a monograph upon Syphilitic Lesions of the Osseous System in Infants and Young Children, which has attracted much attention, both in this country and in Europe.

Dr. Busey[5] has given a faithful study of the action of certain drugs in

[1] Anniversary Discourse before the New York Academy of Medicine, 1847.
[2] An abstract of this paper will be found in the Medical Recorder, Philadelphia, 1825.
[3] Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques, vol. xi.
[4] American Journal of Obstetrics.
[5] Columbia Hospital Report.

bronchitis. And thus, name after name might be mentioned, paper after paper referred to, showing how active the American profession has been in the department of diseases of children. But this address has already transcended the time assigned it, and I must conclude.

Did time permit, I might mention the organization of societies devoted to the especial study of obstetrics, and of diseases of women and of children, in Louisville, Boston, New York, and Philadelphia, as at once the signs and means of advance. So too, the American Gynæcological Society, born this Centennial year, gives promise of high honor and great usefulness. The establishment of hospitals devoted to diseases of women, is another evidence of progress. The Woman's Hospital of New York, founded and built chiefly by the labors of Sims and Emmet, and which has been of such incalculable benefit to thousands of suffering women, and to the profession, has been the noble pioneer. May the day speedily come when every State shall have a similar institution!

The American Journal of Obstetrics, founded by Dr. Dawson, and conducted with such signal ability, the demand for the reprint of the Obstetrical Journal of Great Britain and Ireland, and the valuable additions made to it by American writers, are evidences of progress.

And now, in conclusion, have we brought the names of those who, in this country, have been prominent in advancing the knowledge of Obstetrics, Gynæcology, and Pædiatrics, for apotheosis in some new Olympus? Nay, rather, for inscription in our memorial window of the Temple of Medicine. There let them be written, Samuel Bard, William P. Dewees, Thomas C. James, Ephraim McDowell, Charles D. Meigs, Gunning S. Bedford, Hugh L. Hodge, D. Francis Condie, Henry Miller, Walter Channing—but I cannot repeat the long list.[1] There let them all be recorded, and there forever abide. It is done. Lo! the Orient sun of the Republic's second century is pouring its light upon them, is kindling in our hearts gratitude and joy, and evoking more than MEMNONIAN music, TE DEUMS and JUBILATES, from a multitude whom no man can number, who lived because *they* lived, who suffered and who were saved from their sufferings and from the sorrows of death by the interposition of *their* Divine Art. When that sun is sinking behind the flood of years, may its departing rays gild those names with a new glory, shining too on others still more illustrious.

[1] Is it presumptive to interpret the presence with us to-day of two eminent obstetric teachers from abroad, as a cheerful tribute to the value of American work in this department of Medicine? To those teachers, one of them the honored occupant of a chair made illustrious by the names of the Hamiltons and of Sir James Y. Simpson, and the other whose fame has gone out into all the earth, and whose imperial authority the professional world acknowledges, every American physician gives hearty thanks.

ADDRESS ON MEDICAL JURISPRUDENCE.

WITH

NOTES AND A BIBLIOGRAPHICAL APPENDIX.

BY

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MEDICAL JURISPRUDENCE owes its power to knowledge derived from every branch of medicine, but the law determines how far this power shall be utilized in the administration of justice. Hence, the development of Medical Jurisprudence has varied in different nations with the progress of medical science, and with the extent of its application to the protection of property, reputation, and life. Efficiency in this legal application varies with the appreciation of medical knowledge by the rulers of a nation; and (since an adequate appreciation is limited to the educated few, and is not yet disseminated among the mass of any people), it results, that laws more favorable to the culture of legal medicine are to be found in nations ruled by the educated few, than in those governed by the people. The unequal development of Medical Jurisprudence in different nations finds in these facts an explanation, in large part at least, and recalls the political axiom that "arbitrary powers well executed, are the most convenient," while "delays and inconveniences in the forms of justice are the price that all free nations must pay for their liberty in more substantial matters."[1]

The papal canon-laws,[2] originating many medico-legal questions, sowed in 1620 by the hand of Zacchias,[3] a pope's physician, the first sound seed of Medical Jurisprudence in the land of Columbus, then the home of Science and the Arts.[4]

[1] Blackstone, iv. p. 350. Blackstone's "Commentaries on the Laws of England" (four books) were published a century ago, viz. 1765 to 1769, and, hence, are frequently referred to.

[2] The "Corpus Juris Canonici," a compend of the canon-laws, is dated 1580.

[3] B. 2. This and all succeeding references to "B," refer to the Bibliography, with the numbers therein.

[4] From many facts which might be cited to prove this statement, as also that the development of medical jurisprudence in different nations has varied with their culture of medical science, the following quotation is selected from p. 142 of Russell's "History and Heroes of Medicine," London, 1861: "If we survey the social and political state of Europe from the twelfth to the sixteenth century, in its relation to the development of medical art, our attention is at once arrested by Italy, which at this period was far ahead of the rest of the world. Taking the number of Universities as an index of civilization, we find that before the year 1500, there were sixteen in Italy, while in France there were but six; in Germany . . . there were eight; and in Great Britain two; making sixteen in all—the exact number which existed in Italy alone. The Italian Universities were likewise no less superior in fame than in number to those of the North." Italy maintained this superiority during, and even after, the sixteenth century.

The new-born shoot, languishing in Italy, was transplanted in German soil, where it received such culture as nourished its growth, developed its fruit, and reproduced seed to germinate in other lands. To favoring legislation from 1532[1] to the present day, the fatherland owes its pre-eminence in Medical Jurisprudence. Germany, for two centuries, has had an organization of medico-legal officials, to whom alone it intrusts the duty, both to procure the medical facts needed by the courts, and to estimate the weight due such facts from whatever source obtained; it alone requires that these experts shall be especially educated, and provides medico-legal clinics[2] for their practical instruction. In 1650, Michiaelis[3] delivered the very first lectures on Legal Medicine, and as early as 1720 professorships of the same were founded by the state. By 1725, the celebrated works of Valentini, Teichmeyer, and Albertus,[4] had supplanted that of Zacchias; and since then a medico-legal literature more abundant than in all other languages has nourished the science of Medical Jurisprudence both at home and abroad. In fine, Germany, specially excelling in the Art, has consequently excelled in the culture of the Science.

France, from 1570 to 1692, enacted laws which, like those of Germany, favored the culture of Legal Medicine; but in 1692, medico-legal offices became hereditary and venal, and Legal Medicine languished until after the French Revolution. Since 1790, no nation has surpassed France in the culture of medical science; in addition, the judges appoint medical experts, who, since 1803, must be graduates in medicine, and must have attended one course of lectures, and have passed an examination on Legal Medicine, professional chairs of which were established by the state in 1794.[5] However, French authorities denounce their didactic instruction as insufficient for the education of experts, and declare the appointment of these by the judges, and the lack of skilled medico-legal officials to procure medical evidence, to be most unsatisfactory, and their whole system to be much inferior to the German. Still, France has at least a system, and meanly as this does apply the art, it has served to greatly stimulate the culture of the science, as has been notably illustrated since 1796[6] by French medico-legal literature. A critical appreciation of how much of this literature has been derived from Germany, and how much of medico-legal science without the art has been transported from Germany and France to Great Britain, and the United States, would, I fear, prove offensive to Gallic, and still more to Anglo-American, vanity.

Great Britain transmitted to this nation laws, barbarously conspicuous for the absence of provisions to apply medical knowledge to the administration of justice, and Anglo-American law continues to be, in large measure, hostile to Medical Jurisprudence. However, British laws have done something for the science, and a little for the art. For Great Britain has fostered medical education; did in 1803 found a chair of

[1] The Constitutio Criminalis of Charles V, 1532, (published in 1553) rendered it obligatory on the Courts to take the evidence of medical men in medico-legal cases.

[2] The first one established was at Vienna, about 1830; a second at Berlin, 1833; a third at Munich, 1865; and probably a fourth since 1870 at Strasbourg, where France had its only medico-legal clinic, 1840 to 1870. In France, Great Britain, and the United States, Medical Jurisprudence cannot be said to be practically taught, except as to Toxicology.

[3] University of Leipzig; Michiaelis was succeeded by Bohn.

[4] B. 29, 30, 31.

[5] Chaussier, in 1790, was the first lecturer, and Mahon, in 1795, (B. 115) the first professor. Foderé says the state enacted the first laws favorable to Legal Medicine in 1792.

[6] The date of the first French general treatise (B. 114).

Forensic Medicine in one University,[1] and now has such chairs in all its medical colleges (some of these conferring a special degree in State Medicine);[2] has by the Registration Act and other laws[3] greatly strengthened the medical profession; and has compelled its courts to accept expert evidence only from registered, and therefore educated, medical men. Still, "the crowned republic" remains destitute, as does its democratic American offspring, of popular, and hence of governmental, appreciation of the legal importance of medical knowledge, as is proved by the same lack of any system to secure the medical evidence of competent experts that characterized its laws when surgeons were barbers,[4] and when physicians were astrologers, sorcerers, and interpreters of dreams. What wonder that Germany and France began the study earlier, and have prosecuted it more successfully?

The States of this Union have, for the most part, left the culture of medical science to individual enterprise which supplies solely that which the private citizen demands—practitioners of medicine to heal the sick. The States have as yet made no demand for competent medical experts to aid the administration of justice, and have done nothing designedly for the culture of Medical Jurisprudence. What growth can this branch of State Medicine have as long as a State does not recognize even its existence? Before attempting to answer this question, it will be well—having now briefly examined the causes—to present as briefly some illustrations of the extent of the general progress of Legal Medicine.

To appreciate progress during any period, it is necessary to keep in mind the empire of the dead over the living; to recall some of the victories gained over superstition and ignorance during the preceding century, as well as during that now closing.

From 1620 to 1722, the authority of the father[5] of medico-legal science was supreme. He devoted chapters to Torture, Sorcery, Prophecy, Miracle, and Immaculate Conception. Admitting one hundred and fifty births at a labor, he skeptically doubted the three hundred and sixty-five brought forth by the prolific Countess of Henneberg! During this period doctors gravely discussed whether a woman could be got with child by the devil, or by a dream; and French judges legitimized an infant in a case where the husband had been separated four years from the mother, on the ground that the child owed its paternity to a dream. Doctors taught that grossly deformed infants had a bestial parentage; judges, even in 1769, declared that they had "no inheritable blood" for

[1] University of Edinburgh: Dr. Duncan, Sr., in 1801, was the first English speaking lecturer on Forensic Medicine, and his son the first professor, in 1803. In this same University (chartered in 1582) was established, in 1726, the first English-speaking Medical Faculty, which conferred degrees on less than 300 graduates from 1726 to 1776. From 1705 to 1726 twenty-one medical degrees, which would now be deemed irregular, perhaps honorary, were conferred. Institutions for medical education were established in Italy, Germany, and France, long prior to 1705 or 1726.

[2] Each of the twenty-three medical colleges reported in England and Scotland in 1875, had a regular teacher devoted to Forensic Medicine; and some of these, at least those at Cambridge, Oxford, Edinburgh, and Dublin, confer a special degree in State Medicine on those applicants only who have already graduated in medicine, and have thereafter satisfactorily pursued this special study.

[3] The Registration Act was passed in 1858. "Glenn's Manual of the Laws affecting Medical Men," London, 1871, gives a list of 36 such laws from 1800 to 1870, and of these 25 from 1850 to 1870.

[4] Surgeons were barbers in England until 1745.

[5] B. 2.

a "reason too obvious and too shocking to bear a minute discussion,"[1] and the priest, encouraging doctor and judge, cried "Si tu es homo, te baptizo."

Until 1726 (Albertus), it was taught that, in presence of the murderer, his victim's wounds did "open their congeal'd mouths and bleed afresh," and courts accepted the testimony of medical experts to this miraculous bleeding of the corpse.[2] The effect upon a suspected homicide of touching the dead body of his supposed victim, continued to be a legal expedient within the nineteenth century.[3]

Unearthed bones served to convict men of murder, and yet these accusing bones, since not even human, were not those of missing men. A cranial foramen devised by nature, yet perverted by ignorance into an assassinating awl-hole, would have hung Thomas Bowman, but for accident.

Superstition, denouncing medico-legal autopsies even more fiercely than it now does cremation, did not permit these to become frequent until about 1750; and the work of the father of morbid anatomy,[4] a foundation stone of Legal Medicine, was not published until 1761. By superstition, and by ignorance of normal and morbid anatomy, of the causes of sudden death, of diagnostics, and of chemistry, Legal Medicine was powerless, when compared with its present state. So great was its helplessness, that a horrible atmosphere of suspicion encompassed the fear of death by poison. On those even suspected, the grossest legal abuses were everywhere inflicted; while those convicted were long boiled alive by English law, and burned (as late as 1780) by the French "Chambre Ardente,"[5] which was not abolished until 1791.

The highest medico-legal authorities[6] taught belief in ghosts, witches, and possession by the devil; and united with the clergy until 1752 in denouncing all disbelievers thereof as heretics and atheists. They found demoniacs for the jailor and the stake, where we find patients for the doctor and the asylum. The distinguished medico-legist, Hoffman, commended to the barbarity of the law those who "vomited nails, hair, wax, glass, or leather," as indisputable witches.[7] The "great and good" Lord Chief Justice Hale, prompted by the medico-legal testimony of the learned physician Sir Thos. Browne,[8] illuminated the stake with witches—exemplifying in 1664 the practice of Anglo-American witch-laws till 1727, laws not repealed until 1736.[9] Thus did the legal medicine of our ancestors, only five generations removed, persecute, drown, and burn thousands of the insane, as "fire-brands of hell," who were "moved and seduced by instigation of the devil."

[1] Blackstone, II. pp. 246-7.

[2] See plea of the great scholar and lawyer, Sir George McKenzie, in the "State Trial," 1688, of Sir Philip Stansfield, executed for the murder of his father.

[3] The latest American case was in New York in 1824. (See B. 349, I. p. 807.)

[4] Morgagni, B. 18.

[5] Instituted at the close of 17th century as a special remedy for poisoning, which had become a very frequent crime in Paris.

[6] Paré, Zacchias, Hoffman, Storck, Boerner, &c., (B. 100, 2, 34, 39).

[7] "De diaboli potentiâ in corpora." Hoffman died in 1742.

[8] Author of "Religio Medici," and also of "Pseudoxia Epidemica," or Vulgar Errors.

[9] The "witch-mania" originated with a papal edict in 1484. The last judicial executions for witchcraft were in England, in 1716; Scotland, 1722; Würzburg (Germany), 1749; Glarus (Switzerland), 1780. The witch-mania prevailed in New England, 1692 to 1727. Although the witch-laws were repealed in Great Britain in 1736, yet as late as 1760 supposed witches were murdered by mobs, and there were "witch-doctors" in 1838. In France there was a legal trial for witchcraft as late as 1818.

These few examples must suffice to recall the parentage of the Medical Jurisprudence of our century, and the facts that, with the impotence of science to aid the law, it adopted miracles as explanations, suspicion as proof, confession as evidence of guilt, and "torture as the chief witness,"[1] summoning the medical expert to sustain the accused until the rack forced confession.[2]

During the hundred years now closing, the progress of medicine has been greater than in all preceding time. To detail the means acquired to aid the law, would require the record of every medical discovery: for what one of these may not contribute to the administration of justice? This occasion precludes more than bare suggestions illustrative of the general progress of medico-legal science.

(1) Innumerable precious facts have been contributed by every branch of Anatomy, and especially by Pathological Anatomy. The study of putrefaction, fractures, burns, scars, marks, stains—in fine of every change and injury to be found on the living or dead body—has given the skilled expert a power (miraculous to the ignorant) to identify the body, to distinguish real from apparent death, to approximate the date of death, to decide whether it be due to morbid, accidental, or criminal causes, and often to point unerringly to the criminal.[3] So great is this power that medico-legal autopsies have become indispensable to justice; and, since 1837,[4] the Microscope, strengthening notably Anatomy as also Toxicology, has repeatedly released the innocent from the jailor's clutch, and delivered the culprit to the hangman.

(2) Diagnostics, aided by stethoscope, thermometer, and many other instruments[5] invented or newly applied since 1776, have stripped the malingerer of power to feign disease, become the corner-stone of Life Insurance, and aided the law in many other particulars.

[1] Montesquieu, 1748.

[2] It is true that in our motherland, England, torture was abolished in 1640 (a century and a half earlier than in continental Europe); but, to such extent did suspicion replace proof, and the single penalty of death overtake every species and grade of crime, that the law had much less need than now of medical experts. Prisoners accused of a capital crime were not permitted any witnesses until 1702 (Blackstone, IV, p. 360); an accusation of infanticide sufficed for conviction, unless there was one eye-witness to the birth, until 1802 (B. 153, p. 309); and "prisoners were first allowed the assistance of counsel" about 1830 ("Science of Law" by S. Amos, p. 312). Even in 1769 there was "a dreadful list of 160 capital offences," and, not content with this liberal supply of the halter, English laws provided for criminals horrible mutilations, as branding, castration, slitting the nostrils, cropping the ears, and cutting off the hand, with a medical expert to sear the stump; death by exposure and starvation ("Peine fort et dure"), till 1772, by beheading, by drawing and quartering, and by burning alive; and brutal persecution of widow and orphan by corruption of blood and confiscation. Yet Blackstone asserts (1769), with evident pride, that this "disgusting catalogue" when compared with the criminal codes of other European nations did "honor to the English law." It is calculated to soften impatient indignation to remember that "it cannot be justly regarded as a fault in [legislators] courts or juries not to be in advance of the age in which they live"; and it is encouraging to recall that many of these barbarous laws were repealed because courts and juries did get so much in advance of them, that they could not be executed.

[3] Among errors credited by the profession and corrected within the 19th century may be mentioned here: the beliefs that the human hair could grow after death; that the wind of a canon-ball could destroy life; and that violent and fatal injuries, which at times do fail to leave any visible signs externally, might also fail to present any lesions internally.

[4] Schwann and Schleiden.

[5] Spirometer, Pneumatometer, Galvanic and Electric Batteries, Ophthalmoscope, Laryngoscope, Endoscope, Spectroscope, Sphygmograph, Cardiograph, Dynamometer, Æsthesiometer, etc.

(3) Obstetrics, until 1750 in the hands of ignorant midwives,[1] consequently remained a special nursery of mystery and credulity. During the present century, Obstetrical Jurisprudence has rescued from ignorant superstition monsters, retarded births, superfœtation, and hermaphrodism; has discovered new signs of pregnancy, and the significance of uterine moles[2] and hydatids; has appreciated the evidences of impotence, sterility, and "live-birth," and has discarded the hydrostatic test[3] as conclusive proof of the latter. In vindication of chastity, the signs of virginity[4] have been duly estimated, false have been distinguished from true corpora lutea,[5] and it has been proved that sexual connection "without consent" may be fruitful. Finally, the "jury of matrons" has been slowly despoiled of its authority to decide a question of pregnancy. It is mortifying to record that, in criminal cases, the laws of some of our States continue to regard "quickening" as proof of the very dawn of life; and yet add to this barbarism the inconsistency of admitting, in civil cases, the vitality of the embryo from the date of conception. More than a century ago, medico-legists,[6] abandoning a belief long universal, taught that life began months prior to, and was as sacred before as after, maternal sensation; but, to the encouragement of fœticide, this ancestral superstition[7] still prevails among a free people, and lingers in their laws.

(4) Chemistry, since 1789, when Lavoisier gave it a firm foundation, has enriched every science, bestowing such services on State Medicine as to necessitate the distinct department of Legal Chemistry. Two of many services may be mentioned: the murderer has been deprived of one refuge, which even professional credulity supplied—Spontaneous Combustion[8]—a mode of death yet to be witnessed by a skilled expert; but this interesting service is insignificant when compared with that rendered toxicology. Though poisons have become much more procurable and numerous, yet the skill of the medical chemist has so increased, that criminal poisoning has become, largely through this power, one of the most certainly detected, relatively infrequent, and least dreaded modes of death.

(5) By the knowledge acquired of the nervous system, medical science has influenced society and law to an extent difficult to over-estimate.

[1] A man-midwife was first employed with the greatest secrecy, in 1663. In England, men-midwives did not secure respectable professional position until 1783. Prof. T. G. Thomas writes that in the United States the subject of obstetrics has "been recognized as one of paramount importance and dignity" since 1767.

[2] The French Parliament decided in 1781 that virgins and nuns discharged moles (*i. e.* "blighted ova") without having had sexual connection. (B. 114, 2d Ed. I. p. 477.)

[3] The hydrostatic test was first practically used in legal medicine by Jan Schreyer in 1682, and was long accepted as conclusive proof of live-birth.

[4] Buffon, as also Foderé (B. 114, in the 1st Ed. of 1796, not in the 2d Ed. of 1813), Mahon (B. 115) and many others taught that there was no such thing as the Hymen.

[5] In the trial of Chas. Angus (Lancaster, England), for the murder of Miss Burns, in 1808, all the medical witnesses testified (to her dishonor) that all corpora lutea without distinction proved previous conception.

[6] Faselius, 1767; Haller, 1782; Farr, 1788, etc.

[7] "Absurd ecclesiastical canons handed this error down from one criminal code to another." B. 349, II. pp. 9 and 1076; Foderé, etc.

[8] This debt is due chiefly to Liebig and Bischoff (Case of the Countess of Goerlitz, 1850). Casper wrote: "It is afflicting to be obliged in a serious scientific work in 1861 to still speak of the fable, spontaneous combustion." A human body reduced in a few moments to a cup of ashes! To the credulous in this matter, one is prompted to recall Velpeau's attitude in reference to the "vagitus uterinus," or capacity of the fœtus (inclosed in its membranes) to cry in utero—a belief long firmly attested and universally credited. Said Velpeau: "Since learned and credible men have heard it, I will believe it; but I should not believe it if I had heard it myself!"

In 1774, England enacted the first law evincing one touch of pity for the insane; in 1792, Pinel[1] adopted the first humane treatment of their disease. Since then, civilization has been slowly taught that those upon whom our grandfathers inflicted "the pains of Hell" in order to thus drive the devil out of bodies "accursed by God," are the most pitiably diseased of all our fellow creatures, and we are enabled to point with pride to the palatial asylums with which our laws have replaced the garrets, cellars, stables, and dungeons, where starved and tortured insanity once writhed in filth and chains.[2] The history of the Medical Jurisprudence of Insanity is eloquent with the increasing number and efficiency of the laws designed to protect both society and the sufferer; and with assurances of the extension of these laws from insanity to habitual intemperance.

Is it too much to claim that the progress of Psychological Medicine has strengthened the conviction that not only mental disease, but also hereditary organization, defective education, and circumstances for which society is more accountable than any of its units, do modify criminal responsibility in *fact*, and therefore should do so in *law?* Has not this special knowledge broadened man's charity, encouraged society's efforts to redeem its outcasts, and influenced the law's amelioration of its criminal code? Is it not forcing to the front that most important social question, the problem of heredity; thus disclosing an immeasurable field for the medical research and legislative labor of our descendants? In fine, is it not true that science, stripping nature of providential caprice and disheartening chance, divinely adorning her with eternal order and omnipotent law, has gradually established that the diseases and deformities of the mind are as much as those of the body subject to nature's laws; and that the lunatic, the drunkard, the criminal, the sage, and the fool are not the products of chance, but of laws as comprehensible, though not yet as well comprehended, as those governing that thunderbolt which, once in the hand of Jove, now traverses even the depths of the sea at man's command?

The part this nation has taken in the general progress of Medical Jurisprudence must now be considered, and to test our progress five inquiries will be instituted.

I. *What have our Laws done to apply medical knowledge to the Administration of Justice?*—In the United States there are probably forty-five thousand medico-legal autopsies made annually. The service of a skilled expert at these "coroner's inquests," which have exceptional opportunity and power to detect crime, is of inestimable importance; the opportunities there presented, if once lost, can never be regained. Further, our courts have annually from twenty-five hundred to treble this number of criminal trials necessitating medical testimony; and of these a large part originate from the coroner's inquests. If to these criminal be added all the medico-legal civil trials, it would be found, I doubt not, that our courts require medical evidence in not less than twenty thousand cases annually.[3] Whatever the number may be, it would indicate inade-

[1] B. 129.

[2] In New England, say "Wharton and Stillé," the insane were sold out to the lowest bidder, who starved them, and when violent chained them in stables.

[3] I have sought in vain for full and reliable statistics to illustrate numerically the importance of legal medicine. The numbers given are only approximative estimates based partly on some meagre British statistics cited by Guy and by Taylor, and partly on the

quately the number of citizens whose welfare is involved, and the extent to which society is interested in the efficient application of medical knowledge to the administration of justice.

Now, what are the methods which Anglo-American law adopts to secure in practice that "best attainable evidence" which in theory it demands? It entrusts medico-legal autopsies, which require special medical and some legal knowledge, to those having neither the one nor the other, except by accident; for, these coroners (whose inexperience our law insures by constant "rotation in office") owe their position wholly to political popularity, a qualification which a competent expert is most unlikely to possess. Are these unqualified officials supplied with efficient aid? If so, again by accident, since the law leaves it to chance, or the coroner, or to his still less qualified jury, to provide a medical expert; and, as is usual, accident and ignorance provide inexperience and incompetence. Could ingenuity devise for medico-legal autopsies any methods more inefficient than these, which Anglo-American laws, framed before the birth of Medical Jurisprudence, have barbarously perpetuated?[1]

On this Pelion of inefficiency our legislative giants have piled an Ossa of absurdity; for, besides these fatal defects in the primary legal proceedings, Anglo-American law, in order to secure "the best attainable evidence" for its courts, where poverty and dishonor as well as the halter are administered to the free citizen, clings to a method as sadly ludicrous as it is antiquated. To plaintiff and defendant the law gives full licensé to summon such medical witnesses as each has already found reason to

following facts as to New Orleans, La., for the year 1875. The total number of coroner's views and inquests was 1026; of these there were 268 inquests, and out of these grew 47 trials. Giving New Orleans 210,000 and the United States 40,000,000 population, the New Orleans statistics would indicate for the United States annually 8952 medico-legal criminal trials, growing out of 51,047 medico-legal autopsies, or coroner's inquests.

[1] Convincing reasons could be given in proof that the duties of coroners are discharged even worse in the United States than in England. The following facts indicate how the Anglo-American method works in the latter country. An Englishman writes (1876): "The coroner is elected for life by the rate-payers of his district [a superiority over the American method], and he is paid a good salary out of the county rates. In most cases he is a medical man who has studied the arts of popularity with more success than those of medicine, or he is a small country attorney who has failed in the higher paths of his profession." Dr. Wm. Farr officially reported as to England in 1868: When all the verdicts of coroners "for the first time came under review [another great superiority over the American lack of any such system], they were not at all creditable to the intelligence of the country. They conveyed the least possible information in the vaguest possible words." Prof. A. S. Taylor wrote in 1873: "The coroner's inquest affords no certainty for the detection of crime. It, in some instances, tends to screen a criminal." "In the course of thirty years' practice, at least fifteen cases of the exhumation of dead bodies were referred to me. On some of these inquests had been held, but no inspections were made. Verdicts of death from cholera or natural causes had been returned, and at intervals of from one to twenty-two months the bodies have been disinterred, and it was then proved that the deceased persons had died from poison." (See B. 358, I. p. 12.) The British Medical Journal (Jan. 1876) reports a glaring case of poisoning, undetected by the incompetent coroner. National attention was recently attracted to the same monstrous evil in the inquest of a Mr. Bravo. An English writer asserts that "almost every day, from all parts of England tales come up of the inadequacy and absurdity of the institution. Notice has been given to-day [May 23d, 1876] in the House of Commons, by an independent member on the ministerial side, that he will call attention to the office of coroner at an early day." Other facts indicate that England recognizes this evil better than does the United States, and therefore will probably correct it sooner. July, 1876, it was reported that "the practice of electing coroners has been condemned in the House of Commons by a unanimous vote. A bill for the reform of the office of coroner is soon to be brought in." See also B. 259, 274, and 330.

believe entertain opinions the most contradictory.[1] Who are these partisan witnesses thus summoned by the law to apply the power of medical knowledge to the administration of justice? Surely these legal representatives of science must be competent experts? No.—Well, experienced and educated physicians of repute? No.—Then, of course graduates, at least some fledgling hatched in nine months, and fully feathered with the plumes of every branch of medicine, Medical Jurisprudence included? No, not indispensable, since "as a general rule" it has been adjudged that any practitioner of medicine (that is, any man who dubs himself Doctor) has sufficient knowledge of medical science to furnish justice with its "best attainable evidence."[2] "O, [this] offence is rank, it smells to Heaven!"

Common sense would presume that laws, so prodigal to ignorance and pretension, would provide means to test the value of scientific opinions by eliciting the facts upon which, if valid, they must be founded. Not so; since these opinions are replies to questions, which often by their very structure comically prove entire ignorance of the facts involved; for they are propounded by lawyers to whom these facts are unknown. Finally, it would be presumed that the decision as to the weight due such opinions would be left to a judge or jury specially chosen. No, even this last poor boon is denied by the law!

With the power of medical science thus crippled at the coroner's inquest, then prostituted by the partisan opinions of incompetent experts, then perverted by advocates, and at last when emasculated of all vigor submitted for decision to those unable to estimate its weight; what wonder that such gross misapplication of medical knowledge brings upon it that public contempt which belongs justly to methods so monstrous, and to which true medical knowledge is a helpless, pitiable, and disgusted victim!

But these legal defects, so paralyzing to the past, so discouraging to

[1] Reference is often made to the well-known facts that the sound expert-evidence of the illustrious John Hunter was in 1781 overborne by the evidence of three ignoramuses, and that the testimony of the famous Denman was in 1806 set aside by the Court in favor of one male and two female quacks. The same system is continued, and therefore the same evils persist. Prof. A. S. Taylor reports now, as to England, that a good search and good pay can always find, in abundance, the witnesses needed on either side of any medico-legal issue. This is certainly true as to the United States. Some facts may be cited in illustration. I have personal experience in a suit (unsoundness of a slave) in which the medical experts were selected by one side *because of their well-known ignorance* of the special knowledge (auscultation) which the issue involved; and the judge decided that the whole medical testimony must be set aside, because the negative evidence of the incompetent sufficed to counterbalance the very positive affirmative testimony of the competent experts. Death, with a post-mortem examination, soon after the decision, conclusively proved that the ignoramuses deserved no consideration in justice, though they did receive equiponderant consideration in law. Whenever a notorious trial attracts public attention, the results of our defective laws become disgracefully apparent, as has been illustrated in recent years by the Steinecke-Schoeppe (1868), the Wharton-Ketchum (1872), and the Stokes-Fisk (1874) trials (B. 363, 344, 370). If the medico-legal proceedings are so discreditable in cases like these, exciting great public interest and engaging the best legal and medical talent, what probably are they in ordinary trials lacking these advantages?

[2] The text will be found fully sustained by reference to B. 317, pp. 131–2, B. 349, I. pp. 283–5, and B. 363, p. 406. However, Elwell (B. 330, p. 589) refers to eight decisions to the effect that "special knowledge must be fully established before a witness can be examined as an expert;" but had he stated by what slight proofs, and by what incompetent judges, this "special knowledge must be fully established," the apparent discrepancy would have practically disappeared. An eminent lawyer assures me that in the courts of my native State (Mississippi) the competency of a medical expert rests solely on his own oath; and that, when his own interests and reputation prompt such an oath!

the future of Anglo-American Medical Jurisprudence, are not the only disadvantages against which this nation has had to contend. It inherited from Great Britain not even a page of the literature, in fact nothing of Medical Jurisprudence except laws hostile to it. So destitute was it of those indispensable promoters of science, well endowed institutions, with libraries, laboratories, and museums; so exhausted by the war for independence; so closely occupied by the pressing demands of daily life; and so profitably absorbed by glorious efforts to present to civilization a savage continent, that every science seems to have required half our century to secure the conditions necessary to fairly begin its culture.[1] Another potent, yet ill-appreciated friend to science, pressure of population, now wanting in many, was long wanting in every State. Finally, while a European nation requires but one legislative body to reform its laws, our political system now necessitates the action of thirty-eight State-legislatures to embrace the entire nation.

Just consideration of all these impediments should incline other nations not to condemn, if we have done little for Medical Jurisprudence, but rather to wonder that we have done anything at all; and to congratulate us that, so great has been the diffusion of knowledge, so ardent the love of justice, we have in the main kept pace with, and in some particulars have even outstripped, our mother-land. Fairly we can claim no more; reasonably no more should be expected.

II. *What have our Medical Colleges done to cultivate and to disseminate a knowledge of Medical Jurisprudence?*—The first chair of Medical Jurisprudence was established by the "College of Physicians and Surgeons" of New York City, and filled by Prof. Stringham,[2] in 1813. In 1815 two other Colleges[3] had chairs devoted to the usual branches with Medical Jurisprudence attached to some one of these. In 1825 there were about twenty-two medical colleges; of these only one had a full chair, and only five others had even the fraction of a chair devoted to the subject.[4] At present (1875–6) there are sixty-four regular medical colleges (four of these for women). A report[5] as to forty-six of

[1] To illustrate this as to medicine, and also the practical difficulty encountered by the courts in securing, under our laws, the evidence of competent experts, the following facts are stated: Prof. S. D. Gross reports that in 1776 the United States had about 3000 practising physicians, of whom the great majority had never received a medical education, and those who had, were educated abroad. Prof. Austin Flint, Sr., reports that in 1776 our two medical colleges (one founded in Philadelphia in 1768, the other in New York City in 1770) had not graduated even fifty doctors of medicine, and that up to 1800 the five colleges then existing had graduated only about two hundred. Thacher's "History of Medical Science in the United States" reports that it was computed that in 1826 the United States had 10,000 "very easily graduated" doctors of medicine, and more than 15,000 practitioners without diplomas. Prof. John B. Beck wrote that, "at no period in the history of this country, it may safely be asserted, has empiricism flourished to the same fearful extent as at the present time [1845], notwithstanding our boasted improvements in other respects." In 1870, the United States had 62,383 practitioners of medicine; of these there were perhaps 47,000 "very easily graduated" doctors, and at least 15,000 quacks outside of professional ranks.

[2] Dr. Stringham was also the first lecturer on Medical Jurisprudence in the United States, viz., in New York City in 1804.

[3] In 1815, "The College of Physicians and Surgeons of the Western District of New York" appointed Dr. T. R. Beck "Professor of the Institutes of Medicine, and Lecturer on Medical Jurisprudence;" and the Medical Department of Harvard University (Cambridge, Mass.) appointed Dr. Walter Channing "Professor of Midwifery and of Medical Jurisprudence."

[4] See "Thacher's History of Medical Science in the United States, 1828."

[5] Due to the courtesy of (my colleague on this occasion) Prof. N. S. Davis, M.D., of Chicago, Ill. The forty-six graduate more than nine-tenths of all our annual graduates. The twenty-five graduate fully one-half of the whole number.

the most noted, shows that twenty-one do not profess to teach the subject; of the remaining twenty-five, only fourteen (and these not the best known and attended) have professorships devoted exclusively to Medical Jurisprudence, and, by five of these fourteen, students are taught to become *medical* experts by *lawyers*;[1] while the other eleven have Medical Jurisprudence "tacked on as a caudal appendage" to some one of the usual branches. In fine, only about one-half of even our best colleges profess to pay any special attention to the subject; and many facts could be cited to prove that the true significance of the whole matter, from 1813 to the present day, is correctly represented by the following quotations from one of the most prominent of our living medico-legists:—[2]

"There are very few of the medical colleges in which it is taught, and still fewer in which it takes rank as a distinct and independent branch along with the other departments. Usually when it professedly receives any attention at all, it is tacked on, as a sort of appendix, to some other branch with which it has no natural affinity whatever, as, *e.g.*, Obstetrics or Materia Medica. This is of course done to make a show on the programme, while the subject itself is not taught systematically to the student, if taught at all." "I very much doubt whether Medical Jurisprudence is ever made a qualification for graduation, even in those colleges where it is professedly taught as one of the regular branches."[3]

From these facts it is manifest that, since 1813, our colleges (the offspring of the enterprise of individuals, and not of the State) have made ineffectual efforts to cultivate that special knowledge which, while highly beneficial to the State, would not benefit the individual members of our profession any more directly than any other citizens. In fact, the States through these citizens have failed to provide honorable and profitable employment for medico-legal experts, and, therefore, the profession has not furnished them; and, however enlightened the colleges, however praiseworthy their efforts, they will continue to contend in vain against the obstinate "demand and supply law" of political economy.

The profession recognizes the absurdity of the popular and legal presumption that every practitioner is a medical expert; but the profession does not yet recognize sufficiently that even the most skilful healers of disease are neither necessarily nor generally medico-legists; and that the experience of other nations has fully proved that merely didactic lectures can never render medical graduates competent experts. Were this feasible, any such ideal education is now impracticable; for who will deny (1) that our two short courses of lectures are insufficient for proper instruction even in the fundamental facts indispensable to the education of practitioners of medicine; (2) that the fundamental facts for the practitioner are the same as for the medical expert; and (3) that society remunerates the one, while the State finds no use for the other? Finally, there is reason to fear that, until the State demands medical experts, the colleges, dependent on the student and not on the State for their existence, will be forced by these practical students to realize continually the

[1] Casper insists, with good reason, that the medico-legist should remain a physician; and become neither a lawyer, jurisconsult, nor judge, but simply an expert witness, who does need from the law thorough instruction in the "rules of evidence."

[2] John J. Reese, M.D., Professor of Medical Jurisprudence in the University of Pennsylvania. (See B. 247 and 362.)

[3] Some only of our Law Schools have professorships of Medical Jurisprudence; and there is good reason to believe that the general facts as to the Medical are fully applicable to the Law Schools, as far as instruction in Legal Medicine is concerned.

force of the homely adage, "you may take a horse to the water, but you cannot force him to drink."

III. *What new Facts have Americans added, by original research, to the common stock of Medico-legal Science?*—Restricting the list to researches designedly and specially medico-legal, it must be borne in mind that the sum total of these in all nations has not been very large, and that few should be expected in this country for reasons already stated. Prof. John C. Dalton (though to some extent anticipated by Coste (1849) whose researches were unknown to Dalton) was the first (1851) to make a rigid comparison between the corpus luteum of menstruation and that of pregnancy; and to distinctly indicate the differences which, during a certain period, enable the expert to determine from an inspection of the ovaries whether pregnancy has or has not existed.[1] Dr. Joseph G. Richardson announced in 1869 the important medico-legal discovery that, by the proper application of high powers of the microscope, human blood-corpuscles could with certainty be discriminated from those of certain animals;[2] thereby enabling the expert to refute such statements as criminals have often offered to explain the presence of condemnatory stains of blood. Though the justice of this claim has been questioned, yet some of the highest authorities emphatically sustain it; and this discovery has been usefully applied in several criminal trials.[3] Dr. Richardson deserves the additional credit of having called attention in 1875[4] to a simple method of so treating a blood-stain, of even microscopic size, that it can be successfully examined by the spectroscope and guaiacum test,[5] as well as by the microscope.

Researches by which error is exposed, or truth more firmly established, are often as important to science as those which discover new facts. Among such researches may be mentioned those of John B. Beck, on Ploucquet's[6] and on the hydrostatic test,[7] in 1817 and subsequently; of Horner, on the mucous membranes of the stomach and intestines, in 1827; of Gross, on strangling, in 1833; of Wetherill, on adipocere,[8] in 1855; and of Fleming, on blood-stains, in 1859.[9]

Contributions by my countrymen to the progress of Medical Jurispru-

[1] See B. 257; also, Coste's second livraison, "Histoire du Développement," 1849; and the adoption of Coste's views in Longet's "Traité de Physiologie," II. p. 88, 1850.

[2] "An objective $\frac{1}{25}$ to $\frac{1}{50}$ inch distinguishes human blood-corpuscles in stains (but not in dried masses of blood) from those of the ox, pig, sheep, cat, horse, deer, and goat." (See B. 320, 373, 374, 376, 377.)

[3] Since the delivery of this address, eminent microscopists have made on the preceding periods two criticisms. First, that the word "discovery" is misused—since it has long been known that the blood-corpuscles of the "certain animals" are much smaller than those of man, as also that high powers render this difference in size more apparent. Second, that the words "with certainty" demand a modifying explanation, since able observers declare that cases do occur wherein human corpuscles in dried stains shrink, so as to be as small as those of the "certain animals": hence, that the expert when confronted with such small corpuscles cannot swear "with certainty" whether these be human; but if the specimen of corpuscles approximate those of man in size, then the expert can swear "with certainty" that they are not those of any one of the "certain animals," inasmuch as corpuscles in dried stains do shrink, but do not enlarge. The issue thus raised is as to the amount of shrinkage of human red blood-corpuscles in dried stains; Dr. Richardson asserts that this does not exceed ten per cent.; while others assert that this may be thirty per cent., the corpuscle shrinking in size to $\frac{1}{4300}$, or even to $\frac{1}{5000}$ inch.

[4] B. 375.

[5] Van Deen, 1862, and A. S. Taylor, 1868.

[6] 1782.

[7] Jan Schreyer, 1682.

[8] Fourcroy, 1785–7.

[9] See B. 205, 215, 225, 267, 287. To this list might be added Brown-Séquard's experiments in 1851 on Cadaveric Rigidity. (Am. Journ. Med. Sci., Oct. 1851.)

dence during a century, present a field for investigation so extensive, that some of these contributions may have escaped my search, or may have been inadequately appreciated. To rectify, at least in some measure, any such defects, I present, with this address, a Bibliography of American Medical Jurisprudence, to which attention is invited. The next inquiry is:—

IV. *What culture of Medico-legal Science is evinced by our Literature?*

1. GENERAL TREATISES.—The embryonic stage of medico-legal literature in our mother-tongue is attested by the following "footprints on the sands of time." Farr's "Elements of Medical Jurisprudence,"[1] an abridged translation of Faselius, was the first general treatise in the English language, and the only one from 1788 to 1815, when another worse little duodecimo was added by Bartley.[2] In 1816 Male contributed an insignificant "Epitome of Forensic Medicine,"[3] borrowed from Plenck,[4] and in 1821 John Gordon Smith, M.D., published a small book, which was the first original and meritorious treatise in our language.[5]

In 1823 appeared, in two large octavo volumes, the American, Theodric Romeyn Beck's "Elements of Medical Jurisprudence,"[6] which, in spite of the merit of Smith's book, and of the greater merit of Paris's and Fonblanque's English treatise,[7] also published in 1823, quickly supplanted these wherever the English language was spoken. From the date of its publication, which may be deemed the origin, in fact, of Anglo-American medico-legal science, its twelve successive editions have ably kept pace with the progress of legal medicine. Filling many offices of trust and honor, a member of twenty American and seven foreign scientific societies, Prof. Beck[8] lived to witness the issue of ten editions of his treatise; of these, several were published in England, and even the prolific mother of medico-legal literature issued in 1828 a translated German edition. But to Beck's merit no testimony can be more convincing or pleasing than that gracefully given by the three eminent authors whose works eventually succeeded in largely supplanting his treatise in Great Britain. Traill, the distinguished Scotch professor and author, eulogizes it as "the best work on the general subject which has appeared in the English language;"[9] Guy "acknowledges his obligations in a special manner to Beck's learned and elaborate Elements of Medical Jurisprudence;"[10] and Taylor, than whom there is no higher living authority, testifies that he, when a student, was stimulated by Beck's work to study medical jurisprudence in 1825, when no lectures were delivered in England on the subject, and this book was the leading authority for both lawyers and physicians; and that it "will carry down" the author's "name to future years as one of the most erudite and distinguished writers on medical jurisprudence."[11]

To these testimonials from abroad may be added the eloquent eulogy of that son of America whom its medical profession delights to honor as one of its noblest representatives, and as its President on this occasion. His voice, generous to all, even loving to worth, declares that "this grand book" "was in its day the most comprehensive, able, and erudite production on the subject of which it treats in any language," and that it "constitutes a lasting monument to the genius, industry, judgment,

[1] B. 150, also 207. [2] B. 151. [3] B. 152, also 207.
[4] B. 47. [5] B. 153. [6] B. 209. [7] B. 154.
[8] Born in 1791; died in 1855. For the biographies of Drs. T. R. Beck, John B. Beck, and Moreton Stillé, see Gross's "American Medical Biography," 1861.
[9] B. 158, 2d Ed. [10] B. 161, 2d Ed. [11] B. 359.

and learning of its lamented author."[1] It is pleasing to learn from the same source that, in honoring this famous author, we are honoring the memory of a good and noble man; and this pleasure is enhanced by recalling the humble estimate which he himself accorded to his "world-renowned book." For his own statements were that his "highest ambition would be gratified" if his "collection of detached essays" should "in some tolerable degree" "prove useful." Knowing that these modest words will find a generous echo in the hearts of all noble men, to them is commended the fame of America's first and greatest medico-legal author.

In 1850, Prof. Amos Dean, a lawyer, published his brief but excellent text-book, the "Principles of Medical Jurisprudence,"[2] which has passed through three editions.

A lawyer and a physician united to produce in 1855 the voluminous and admirable general treatise, "Wharton and Stillé's Medical Jurisprudence."[3] After completion, but prior to its publication, profession and country had cause to mourn the death of the young and gifted Moreton Stillé.[4] His legal associate, deriving from other able pens indispensable medical aid, has lived to issue three editions of a work which both professions accept as one of the highest standard authorities.

Evidence of our increasing appreciation of legal medicine, as also of our obligations to foreign sources, is found in the republication, since 1819, of nine books by British authors on the general subject, viz., Cooper's collection of the earliest English tracts (1819), and the numerous valuable articles in the "Cyclopædia of Practical Medicine" (1845), which deserve mention with the general treatises of Ryan (1832), Chitty (1836), Traill (1841), Guy (1845), and Woodman and Tidy (1876); and of still greater value Taylor's "Manual," as also his "Principles and Practice of Medical Jurisprudence" (1845 to 1873).[5]

2. Treatises, Essays, etc., on Special Subjects.—Besides the excellent books of Ray, Elwell, Ordronaux, Wormley, Reese, and the New York "Medico-Legal Society," the essays and articles on special topics have been too numerous to permit full examination, or more than a partial sketch of this important branch of my subject.

Infanticide and fœticide received from Prof. John B. Beck attention in 1817;[6] and this thesis, enlarged and improved for the various editions of his brother's "great work," displays a combination of erudition, original research, sound sense, and rhetorical excellence, which render it one of the most classical essays in the medical literature of the English language. Abortion has been further illustrated by the essays of Hodge (1839, 1873), Storer (1866, 1867), Heard (1868), and others;[7] as also by numerous valuable articles in our periodical literature.[8] Apparently increasing with the pressure of population, this stain on modern civilization has become one of the most serious and hopeless problems within the province of Legal Medicine, and demands the grave consideration of every enlightened citizen.

[1] See pp. 39, 65, "History of American Medical Literature" (1776–1876), by S. D. Gross, 1876.

[2] B. 254.

[3] B. 264.

[4] Born 1822; died 1855.

[5] B. 207, 245, 223, 231, 239, 246, 390, 247, 358.

[6] B. 205.

[7] B. 236, 279, 305, 309, 314, 310, 315, 325, 341.

[8] In the "Specimen Fasciculus of a Catalogue of the National Medical Library," 1876, pp. 24–5, are reported from our medical journals thirty-nine articles on this subject, since 1825, by Americans; of these, twenty-three have been published in the last ten years.

Elwell's "Malpractice and Medical Evidence"[1] (1860, 1871) and Ordronaux's "Jurisprudence of Medicine"[2] (1869) are very valuable works—legal rather than medical—and unique, it is believed, in medico-legal literature. In connection with malpractice, Hamilton's noted essay on "Deformities after Fractures"[3] (1855) and his standard treatise on "Fractures and Dislocations" (1860–1875) deserve mention, since more than half of all trials in the United States for malpractice have originated from the results of these injuries.[4]

Supplied with domestic editions of Orfila (1819, 1826), Christison (1845), and Taylor (1848–1875), on Poisons, and with Naquet's Legal Chemistry (1876),[5] native authors have produced two meritorious works on Toxicology. Wormley's "Micro-Chemistry of Poisons"[6] (1867) has received the most flattering commendation from the highest authorities, who report this book to be the result of original researches which enrich it with the treasures most valuable to science. No more will be said, because the author is not only living but present, and no praise from me can enhance the reputation of his work. The same considerations prohibit more than reference to Reese's excellent "Manual of Toxicology"[7] (1874), which has been republished in London.

On the most important specialty, America has produced one treatise, Ray's "Medical Jurisprudence of Insanity."[8] In 1838, when first published, there were few such treatises extant,[9] and it at once assumed, and throughout an English, a Scotch, and five American editions, has deservedly maintained, the first rank in Anglo-American medical literature. An able advocate of "moral mania"—the present battle ground in the Medical Jurisprudence of Insanity—Dr. Ray does not forget, while urging a larger charity for disease, to demand ample and better legal means to protect society. Besides this treatise, essays of value have been published by Hammond (1866, 1873), Fisher (1872), and Cowperthwait (1876),[10] and numerous meritorious reports and articles have richly adorned our periodical literature.[11] It would require a volume to do justice to the medico-legal labors of the Superintendents of our Insane Asylums, and to record the evidences of progress presented solely in the "American Journal of Insanity;" while some of the most valuable essays on this and other special subjects are to be found in the publications of the New York Medico-Legal Society.[12]

There are four other works which, treating incidentally of the Medical Jurisprudence of Insanity, deserve mention: the notable "Diseases of the Mind" (1812–1835), by Benj. Rush, a father of this republic and "the father of its medical literature;" Seguin's "Idiocy" (1866); Echeverria's "Epilepsy" (1870); and Hammond's "Diseases of the Nervous System" (1871–1876).[13]

Fourteen foreign books on insanity have been republished in the United States. Five treat specially of its legal relations, viz., those by

1 B. 290. 2 B. 317. 3 B. 266.
4 B. 330, pp. 55 and 587; and B. 213. 5 B. 206, 212, 244, 252, 389.
6 B. 307. 7 B. 362. 8 B. 234.
9 Viz.: Haslam's, 1807; Hoffbauer's, 1809; Georget's, 1827; Conolly's, 1830.
10 B. 304, 338, 348, 385.
11 B. 216, 265, 275, 278, 282, 283, 284, 285, 295, 296, 302, 304, 311, 316, 322, 323, 324, 327, 331, 332, 347, 353, 354, 357, 365, 366, 368, 369, 379.
In addition to the above, Dr. John S. Billings, U. S. A. (Surgeon-General's Library) has kindly furnished a selected list of 88 medico-legal articles by Americans on unsoundness of mind; of these, the first was in 1827; 79 since 1850; and 41 of the 88 since 1865.
12 B. 363. 13 B. 203, 303, 321, 326.

Highmore (1822), Blandford (1871), Maudsley (1874), Sheppard (1875), and Browne (1876);[1] and nine treat of these relations incidentally, viz., those by Combe (1834), Prichard (1837), Esquirol (1845), Brierre de Boismont (1855), Bucknill and Tuke (1858), Winslow (1860, 1866), Maudsley (1867, 1871), Tuke (1873), and Wynter (1873).[2]

This incomplete sketch of our medico-legal literature attests that it has since its origin progressively increased, and that a very large proportion has been furnished during the last ten years; thus indicating a diffusion of this knowledge to an extent which encourages the hope that such appreciation of its importance is in growth as will insure its proper use.

That our progress in this literature may be properly estimated, as far at least as increase of quantity is concerned, it is indispensable to compare it with that of other nations. As concerns *general treatises*, this nation, beginning in 1823, has produced three;[3] Germany, beginning a century earlier, published at least forty before, and more than twenty since 1823;[4] France beginning in 1796 published nine before, and seven since;[5] and Great Britain, making its first creditable effort in 1821, published its second in 1823, and has issued eight since.[6] If a comparison as to *special treatises* be instituted, a like result ensues, and a search into periodical literature proves even more unfavorable. Since 1782, Germany alone has had, and now has, several journals of great merit devoted exclusively to Legal Medicine, from which flow, to the benefit of all nations, a constant stream of medico-legal knowledge. France has had, only since 1829, one of the ablest journals[7] in the world devoted to State Medicine, but not one to Medical Jurisprudence exclusively. Great Britain has no such journal, nor has this nation except in so far as represented, since 1874, by the valuable "Psychological and Medico-Legal Journal" of Prof. Hammond.

These facts tend to prove that the culture of medico-legal literature is proportionate to the use made of it by the law; and prompt the comment that, however valuable compilations may be, these and the progress of every science depend ultimately on original research, and the labor of practical workers. Germany has produced many, France several, and Great Britain one or two, practical medico-legal experts of widespread fame. This people has never had, nor is it likely to have, one, until it provides for him honorable and lucrative employment.

V. *What illustrations of medico-legal progress are to be found in the Institutions, Laws, and Judicial Decisions of our States?*—Originated by vital statistics, nourished by medical selection, protected by medical evidence, Life Insurance, the wondrous child of Medicine and Finance, encourages the hope that it will repay our science the debt it owes. Causing numerous medico-legal suits, its vast interests are deeply involved in the legal reforms indispensable to the progress of Medical Jurisprudence; for these interests are often sacrificed by the negligence of coroners,[8] the incompetence of experts, and the inefficiency of our medico-legal methods. While Life Insurance suits have most frequently grown out of intemperance and suicide, yet there would seem to be few medical issues on which they may not depend, even on whether death was due to consump-

[1] B. 208, 334, 371, 382, 388.
[2] B. 226, 232, 243, 270, 286, 297, 313, 335, 360, 361.
[3] B. 209, 254, 264.
[4] B. 29 to 94.
[5] B. 114 to 128.
[6] B. 153 to 164.
[7] Annales d'Hygiène et de Médecine Légale, Paris.
[8] The issue has recently been raised, in reference to a case, the subject of a coroner's inquest, whether death was due to intemperance or to heart-disease. A medico-legal autopsy would have probably prevented any such issue, but this was totally neglected.

tion or to diphtheria.[1] To illustrate the progress and extent of the medico-legal interests involved, it deserves record that, in 1840, there were in the United States only three or four companies having an insignificant amount of business; that in 1874 there were seventy-seven companies insuring $2,226,000,000 upon 910,000 policies; and that the annual average number of lives insured during the past three years has exceeded 200,000.[2]

The growth of Life Insurance is one of many examples which illustrate the fact that the importance of Medical Jurisprudence has constantly increased with the progress of medicine, and the requirements of civilization. None the less, there are instances where, through legal and other causes, the bounds of Legal Medicine have been restricted.

When suicide ceased to be a felony,[3] it ceased to concern Legal Medicine, except when the suicide's life was insured; feigned diseases have under our laws little importance; and, with the abolition of imprisonment for debt, and of slavery, there disappeared from the courts many medico-legal questions. To education,[4] Forensic Medicine owes a diminution of its duties in deciding the degree of criminal responsibility of the deaf and dumb; all of whom, long held as "legal idiots" under the "perpetual pupillage of the law," were disabled from making a contract, will, or valid marriage. Possessing no institutions for their education until 1817,[5] the United States had, in 1875, forty-eight institutions, with 5309 pupils, so that "nearly all the deaf mutes of school-age within the country are now receiving instruction"; and, since "all pupils on leaving school take their places as responsible members of society, in possession of full civil rights," they cease to be of special medico-legal interest.[6]

In this, as in every civilized nation, the best examples of medico-legal progress are furnished by the institutions, laws, and decisions benefiting

[1] I have been summoned (1876), as a medical expert, to testify to my opinion as to whether a death was due to consumption, as the main issue, and, secondarily, whether due to consumption or to diphtheria.

[2] For these statistical facts I am indebted to Mr. C. C. Hine, Editor of the Insurance Monitor, No. 176 Broadway, New York City. Mr. Cornelius Walford, of London, the highest authority on this subject, has kindly furnished, for comparison with the United States, the following among other interesting facts: Life Insurance was begun in the United Kingdom (Great Britain) on a scientific basis in 1762, and reached its maximum in 1865–1868; in 1874 there were 120 companies, which issued 47,516 policies, and had in force an unstated number of total policies insuring £362,238,534. In France it was first legalized in 1787, and fettered with many legal restrictions which were removed in 1819; in 1870 there were 166,474 Life Policies, insuring £66,312,000, and 58,572 annuity contracts, securing annual annuities amounting to £1,528,600; and in 1874 there were 72 companies, having in force an unstated number of total policies insuring £49,906,400. In Germany (including German Austria and German Switzerland) Life Insurance was begun in 1827; in 1874 there were 51 companies, having in force 645,989 policies insuring a little under £100,000,000; and the new policies issued in one year (1873) were 98,692.

[3] By English law the suicide was required to be buried in a highway "with a stake driven through the body" (repealed in 1823), and his property confiscated. Juries, more humane than the law, generally decided that suicides were insane, and therefore irresponsible. Beck in 1823 (B. 209, 1st Ed.) congratulates the United States that "we do not war on the dead body in this country."

[4] The Abbé de l'Epée published in 1759 the first modern method to teach mutes, by sign-language. To him and to his disciple, the Abbé Sicard, civilization owes the initiation of the education of mutes. The first educational institution was established in France in 1760. Europe had about thirty of these institutions in 1817.

[5] The first institution founded in the United States was the American Asylum, Hartford, Conn., 1817.

[6] I am greatly indebted for the above statistical and other interesting facts to Mr. I. L. Peet, Superintendent of the New York Institution, the second one founded in the United States, viz., in 1818. (See B. 340.)

all who, no longer possessed or seduced by a legalized devil, are in fact afflicted with "unsoundness of mind," from mental disease, deformity, or debility.[1] By legislation many measures (still very incomplete in most, and not complete in any, of our States) have been adopted for the counter-protection of both the insane and the sane. The corner-stone of these measures has been the establishment of Insane Asylums. Beginning our national existence with two of these, having only eleven in 1830, we now possess about eighty public asylums, accommodating more than 30,000 of our 45,000 insane population.[2] The knowledge derived from these schools for scientific observation, is destined to benefit those without, even more than those within, their walls. Stimulated by this knowledge, the law is extending its protection, from insanity, to all who by intemperance "have lost the power of self-control." In this reform, New York, in 1854, pioneered the way with the first legislation providing for the restraint in a public asylum of habitual drunkards. In 1858, this State, as also Massachusetts, supplied the asylums indispensable for the execution of the law. With an origin thus recent, there are now at least seven asylums in five States, and four additional States have legislated to accomplish the same end.[3] The best interests of society require that this progress shall continue until the laws of every State provide for every person, whose abuse of his own liberty outrages the rights of others, ample protection both for himself and for society.[4]

Maine and New York illustrate additional progress in the jurisprudence of insanity. It is conceded that, within this century, unfortunates have been legally murdered for illegal acts, the product of disease and not of a "vicious will." To prevent these "bitter mockeries of justice," Anglo-American law, so jealous of the "liberty of the subject," fails not only to provide him, when his life is imperiled through brain-disease, with competent experts, but also to provide these with proper time and opportunity to decide a question so difficult as doubtful sanity. It is not strange that decisions reached through such defective means should cause constant dissatisfaction, nor that this should have been more serious prior to the establishment of State Lunatic Asylums; for it then occurred that he who might be acquitted of homicide, because of insanity, was freed by the law, and permitted to live a constant danger to society.[5]

Maine, in 1847, wisely enacted that "when any person is indicted for a criminal offence, or is committed to jail on a charge thereof, . . .

[1] This progress has been especially notable since 1830.

[2] For these statistics I am indebted to Dr. John P. Gray (my colleague on this occasion), who reports 54 State Asylums in 1875, accommodating 21,542 patients; 9 State Asylums in process of erection to accommodate 4600; 18 other public asylums accommodating 6064; and 10 private asylums accommodating 254 insane.

[3] Our effective Inebriate Asylums are: the Washingtonian Home, Boston, Mass., since 1858; the State Inebriate Asylum, Binghamton, N. Y., since 1858; the Inebriates' Home, King's Co., N. Y., since 1867; the Washingtonian Home, Chicago, Ill., since 1864; the Sanitarium, Media, Pa., since 1867; the Harlem Asylum for Inebriates, Baltimore, Md., which was opened in 1871; and the Franklin Reformatory Home for Inebriates, in Philadelphia, Pa., opened in 1872. California enacted the requisite laws in 1870, and Connecticut in 1874. It is believed that Texas and Kentucky have legislated on the subject. For the facts stated I am indebted to Dr. Albert Day, Sup't of the Washingtonian Home, Boston; to Dr. D. H. Dodge, Sup't of the Binghamton Asylum; and to the Annual Reports (since 1871) of the "American Association for the Cure of Inebriates."

[4] In connection with intemperance, it deserves record that only within the present century have Anglo-American Judges inclined to admit drunkenness in extenuation of crime, depriving it of premeditation. (See B. 329, p. 567.)

[5] B. 319, I. p. 759. An eminent lawyer asserts that this still occurs in some of the States, notwithstanding the State Lunatic Asylums.

any judge of the court before which he is to be tried, when a plea of insanity is made in court, or he is notified that it will be made, may . . . order such person into the care of the superintendent of the insane hospital, to be detained and observed by him till the further order of the court, that the truth or falsity of the plea may be ascertained." New York, in 1874, enacted laws which provide for "an investigation of the sanity or insanity of the accused, as a separate and independent proceeding from the trial of the indictment," and, after such preliminary investigation, "leave the question of the guilt or innocence of the accused to be tried by itself."[1] Thus have Maine and New York lessened the frequent difficulty of choosing between "inhumanity to disease, and indulgence to crime."

Five States[2] have aided the solution of this problem by progress in another direction. For, by the abolition of capital punishment, the cruel alternative between the asylum and the gallows has been obviated, and no shocking injustice can be perpetrated by confining in a penitentiary, rather than in an asylum, him proved dangerous to society. It was a great advance in civilization when the victor ceased to slaughter, and by enslaving utilized the lives of the vanquished. Will it not be proof of like progress when society has fully learned how best to utilize the lives of all its criminals?[3]

Notwithstanding that there is still great lack of uniformity in the decisions of our courts as to what are the proper tests of insanity, there has been decided progress. Until Pinel's day (1792), insanity had never been properly studied, hence physicians were little less ignorant and superstitious in regard to it than the public. In the absence of medical knowledge, what could the law do other than establish tests for itself? Until 1800, the savage test of Anglo-American criminal law was that he alone should be adjudged insane, who was "totally deprived of his understanding and memory, and did not know what he was doing any more than an infant, than a brute, or wild beast." In 1800, it was, for the first time, mercifully decided that those whose crimes were due to an "insane delusion," should not be hung, even though not yet wild beasts.[4] In 1812, it was decided that the correct test was whether the criminal alleged insane had, as to matters generally, "the power of distinguishing right from wrong;" and since 1843, in England, and to a large extent in this country, the judges' test has been, whether such criminal had, as to his special crime and at the moment it was committed, "a sufficient degree of reason to know that he was doing an act which was wrong."

But, since 1800, alienists have taught, in constantly increasing number, that a capacity to distinguish right from wrong is an inadequate test,

[1] B. 364.

[2] Michigan in 1846; Rhode Island in 1852; Wisconsin in 1853; Iowa in 1872; and Maine in 1876. Blackstone, referring in 1769 to England's "dreadful list of 160 capital offences," hoped for "such a gradual scale of punishment to be affixed to all gradations of guilt, as may in time supersede the necessity of capital punishment, except for very atrocious crimes." (Book IV. p. 371.) In the various States, other than the above, capital punishment is now inflicted for only from three to eight "very atrocious crimes."

[3] My brief allusions to the "abolition of capital punishment" and to "moral mania," intimate convictions which may cause misapprehension. For *until* our present defects in the administration of justice, in the constitution of our juries, and in the means needed to fully protect society, be remedied, I shall believe that our courts have gone generally as far as, and often much further than, the best interests of society require.

[4] In 1800, Lord Erskine initiated the first enlightened legal discussion of insanity; and the Attorney-General of England then declared that the "wild beast theory" of Lord Hale had never been contradicted, but had always been adopted.

and that, not infrequently, while the reason is apparently able to make this distinction, the will may be so enfeebled, and the morals so perverted by disease, as to deprive the sufferer of that "vicious will" necessary to constitute a crime. The first Anglo-American judges to adopt these lessons of medical science, were Shaw, of Massachusetts, in 1843, and Edmonds, of New York, in 1845. They ruled that insanity was proved if the homicide "had no power of control;" "if his moral or intellectual powers were so deficient that he had not sufficient will;" and "if he did the act from an irresistible and uncontrollable impulse."[1] The learned Edmonds took occasion to observe that "the law in its slow and cautious progress still lags far behind the advance of true knowledge," and the history of the progress of Legal Medicine proves the truth of this observation as to every medico-legal topic. How can this be otherwise while, as another legal authority asserts, our "legislatures are much better versed in the miserable tactics of party, than in the . . . laws of physiology," and while judges, as well as legislators, mindful as they should be of precedent, that friend to uniformity and stability of the law, do, in their superstitious reverence, often forget, not only that "the law of nature, being coeval with mankind and dictated by God himself, is of course superior in obligation to any other,"[2] but also that science is constantly engaged in revising and adding to our knowledge of this supreme law, thus increasing man's power to "think the thoughts of God!"

Improving on Shaw and Edmonds, Judges Bell, Perley, Ladd, and Doe, of the Supreme Court of New Hampshire, have, since 1864, announced the only principles worthy of both legal and medical science,[3] and have thereby merited the profound gratitude of the medical profession, and lasting commemoration in the annals of Anglo-American Medical Jurisprudence. None announced the true principles of the law more clearly than Judge Doe, who (in 1869) charged that "at present, precedents require the jury to be instructed by experts in new medical theories, and by judges in old medical theories;" and that, in this, "the legal profession were invading the province of medicine, and attempting to install old exploded medical theories in the place of facts established in the progress of scientific knowledge." But, he adds, "that cannot be a fact in law which is not a fact in science; that cannot be health in law which is disease in fact; and it is unfortunate that courts should maintain a contest with science and the laws of nature upon a question of fact, which is within the province of science, and outside the domain of the law." "The legal principle, however much it may formerly have been obscured by pathological darkness and confusion, is, that a product of mental disease is not a contract, a will, or a crime. It is often difficult to ascertain whether an individual has a mental disease, and whether an act was the product of that disease, but these difficulties arise from the nature of the facts to be investigated, and not from the law; they are practical difficulties to be solved by the jury, and not legal difficulties

[1] In the record of progress in the legal tests of insanity, it deserves notice that Drs. Gooch and Combe, about 1830 (B. 226), "first announced the great principle that the mind of one supposed insane should be compared with his own mind when in its natural, habitual state," and not with some ideal average mind of health. This principle has been extended by the laws of New York to homicide in self-defence, etc., modifying the interpretation of "apparency of danger," "cooling time," "intent," and "premeditation." (See Homicide; by F. Wharton, 2d Ed., 1875.)

[2] Blackstone, I. p. 41.

[3] State *v.* Wier, State *v.* Pike, State *v.* Jones, Boardman *v.* Woodman, 1864, 1869, 1870.

for the court." "If the tests of insanity are matters of law, the practice of allowing experts to testify what they are, should be discontinued; if they are matters of fact, the judge should no longer testify without being sworn as a witness, and showing himself qualified to testify as an expert." In fine, all symptoms and all tests of mental disease are purely matters of fact to be determined by the jury from the evidence of competent witnesses; and since "legal precedent was one way, legal principle the other," precedent should be abandoned, and principle be followed.

Side by side with the enlightened views of these American judges, will be recorded, for the instruction of astonished posterity, that as late as 1862 the Lord Chancellor of England did, in referring to insanity, declare in the House of Lords that "the introduction of medical opinions and medical theories into this subject has proceeded upon the vicious principle of considering insanity as a disease," and that he condemned "the evil habit which had grown up of assuming that it was a physical disease"![1] Can this fail to revive the recollection that ninety-three years anterior to this Lord Chancellor, Blackstone, an abler lawyer than he, thus fulminated (1769): "to deny the possibility, nay, actual existence of witchcraft and sorcery is at once to flatly contradict the revealed word of God," . . . "and the thing itself is a truth to which every nation in the world hath in its turn borne testimony"?[2] This Lord Chancellor affords, however, a psychological lesson much needed even now; for his opinions illustrate what profound furrows the ancestral bigotry and superstition of centuries can plough into even the best endowed brains of the descendants—as we all are—of uneducated barbarians.[3]

One more hopeful example must close my record of our progress. In 1867, Medicine and Law, uniting for the first time in history for such a purpose, founded "The Medico-Legal Society of New York;" now numbering among its members more than four hundred physicians and lawyers of influence and distinction, its brief life has been adorned with good deeds. It has established a medico-legal library, which promises to become, if it is not already, the best in existence; it is about to issue a second volume of valuable essays, the first having been published in 1874; it has, by critical vigilance, elevated the standard of medical experts, and in one noted conviction its criticisms instigated a second trial with acquittal; wisely imitating its junior, "La Société de Médecine Légale de France,"[4] in its efforts to furnish a substitute for the negligence of the State, it has (May, 1876) appointed three medical and three legal experts as a permanent commission to investigate any medico-legal issue referred to it; it has called attention to the condition and criminal responsibility of uneducated mutes, and to the laws necessary for their protection; and it has influenced the legislation of New York as to abortion, as to robbery under the influence of chloroform, and as to the trial and punishment of all homicides, whether sane or insane. Thus instructing the public, and inaugurating reforms, it seems destined

[1] Hansard, CLXV. 1297. [2] Blackstone, IV. p. 60.

[3] It deserves record, in connection with the progress of judicial decisions, that, at the present day, Anglo-American law does not regard the killing of an infant prior to its complete expulsion from the vagina (*i. e.* to its being "fully born alive") as infanticide. An English Judge recently charged a jury that, "if of opinion that the prisoner had strangled her child before wholly born, she must be acquitted of murder." (See B. 292, I. p. 542; and B. 358, II. pp. 359-61.) Ellwell (B. 290, p. 555) says, "Infanticide may be committed upon an unborn live child."

[4] Organized February, 1868, and formally recognized by a decree of the French government, January, 1874.

to render New York the pioneer of all the States in medico-legal progress; and finally, in promoting scientific intercourse and discussion between the two professions, it has forcibly taught that the invidious bigotry of the one is as misplaced as that of the other, and that the common weal loudly demands the united efforts of the best minds of both.[1]

What labor so god-like as the dispensation of justice; what efforts in its behalf can be more important than those designed to enable the law to reap promptly the full benefits of science—of that knowledge which, having done so much for human development, now proffers unappreciated boons, and by incessant progress assures mankind a higher destiny? Shall medicine and law, standing aloof, not only endure but even blindly honor ancestral evils? Is it needful in 1876 to remind two learned professions that the author of our imperishable Declaration of 1776 declared: "The Gothic idea that we are to look backwards instead of forwards for the improvement of the human mind, and to recur to the annals of our ancestors for what is most perfect in government, in religion, and learning, is worthy of those bigots in religion and government by whom it has been recommended, and whose purposes it would answer. But it is not an idea which this country will endure"?

This historical sketch requires for its completion a record of the measures proposed to correct our medico-legal evils, which, often denounced since 1823 by the medical profession, continue unappreciated by the people. To test the value of these proposed measures it is indispensable to understand the following premises, accepted by the highest medical and legal authorities.

While one would suffice to maim, our law adopts two methods to murder, Medical Jurisprudence; both so result that it is a vulgar mockery of medical knowledge and of justice to term such medical evidence as is attained, "the best attainable." In one case, the law neglects to secure competent experts to testify, as "ordinary witnesses," to facts obtained by them from inquests and all medico-legal examinations. In the other case, the law fails to provide such experts to testify, as "skilled witnesses," not to facts they have personally witnessed in the case, but to their "opinions," as to what conclusions are deducible from the medical facts sworn to by ordinary witnesses; for this marked difference between the *ordinary* and the *skilled* witness, Anglo-American law deplorably fails to make any provision, while German and French law wisely recognize that the skilled witness is not an ordinary witness, but an *arbitrator*. Further, it is manifest that the experts thus needed, for two distinct purposes, can never be supplied except by the State; that the competency of these experts can never be secured except by special and practical instruction; that the chief instruction must be given by medical men; and that the instructors alone are competent to attest the competency of their pupils.

These premises cannot be refuted, and force the general conclusions that no measures could fully correct our evils except such as, in the first place, would reform altogether the office of coroner, and supply competent experts for its duties, and, in the second place, would provide a legal status for skilled witnesses, and secure their skill; and that these two indispensable ends can never be gained except through *an organized system of medico-legal officials, specially trained as medical experts, and*

[1] B. 356, 363, 364, 380, 387.

attested as competent by competent judges. Alas! these irrefutable conclusions are most discouraging, for no American can fail to appreciate how difficult and distant is their realization. But when the remedies are not forthwith at hand to cure a disease, should we cease its study, and our efforts both to discover and to obtain all the remedies necessary? A full understanding of the indispensable remedies, will at least enable us to test the value of the seven which have been proposed. Their introduction requires two comments: first, there is no one of them which would not prove decidedly beneficial, since, fortunately in this regard, there is no change of our law which could possibly aggravate our present evils; and second, the first four to be mentioned apply to only one of our two great evils.

One proposal is that special juries be provided for special cases,[1] and another is that a medical assessor be appointed to advise and assist the courts;[2] either would provide better judges of medical testimony, but neither makes any attempt to supply "the best attainable evidence." A third measure proposes a commission of experts chosen either by mutual consent,[3] or one by each party and a third by the judge.[4] This great improvement could probably be adopted more readily than any other measure suggested; but none of those thus authorized to appoint have the knowledge necessary to enable them to select competent experts, and therefore this measure would certainly not secure "the best attainable evidence." A fourth proposal is the adoption of the French law which empowers the judge to appoint a medical commission.[5] French authorities, while urging the adoption of the German system, denounce their own with even contemptuous bitterness,[6] asserting that their judges rarely appoint competent experts, but generally their own family-physicians and practitioners of merely popular repute.[7] A fifth proposal, which would require all graduates in medicine to be competent experts, as well as practitioners,[8] has long been, and is daily becoming so much more impracticable, as to deserve no notice except as illustrating how inadequately is estimated the extent and character of the special knowledge necessary to a medico-legist.

Prof. Gross[9] urged, in 1868, that the Judges of the Supreme Court of each State should appoint a commissioner in every judicial district, to elicit and estimate medical evidence; and that he should be provided with two or more medical experts as assistants, to make all medico-legal examinations. This is the only measure, thus far considered, which aims a practical blow at both our evils. But, if French experience be applicable to us, then judges of law are not good judges of experts, and should not be authorized to appoint them, except from those whose competency has been attested by competent judges of experts; and, if German experience be applicable to us, then a medical commissioner, long ago tried

[1] J. Fitz Stephen's "Criminal Law of England, London, 1863;" and Dr. I. Ray (B. 234).
[2] A. S. Taylor and many others.
[3] Beck (B. 209) and others.
[4] Ordronaux (B. 318), etc.
[5] Urged by many.
[6] See Foderé, Orfila, Devergie (B. 114, 121, 124); and especially the Preface to Vol. I. 4th ed. of Orfila. Some at least of the French courts have made the improvement of appointing a permanent commission of experts for all medico-legal cases, instead of a special commission for each case.
[7] If the French system be satisfactory, if the State provide, as it should, "the best attainable evidence," then why should there be in Paris, as in New York City, an unofficial and gratuitous commission of volunteer experts?
[8] Chitty (B. 231), and very many others.
[9] President's Address, Trans. Am. Med. Assoc., 1868.

and abandoned in Germany,[1] is superfluous, provided the law supplies a proper system and competent experts to present to the courts "the best attainable evidence."

Finally, others advocate some such modification, as may be practicable under our form of government, of that German system which, the growth of two centuries of experience, alone gives satisfaction at home, and at the same time commands the admiration of the medico-legists of every civilized nation.[2] The Prussian system has four grades of medico-legal officials. (1) To a specially educated official expert is submitted every medico-legal case.[3] His examinations, made in presence of the judge (in some cases), and of medical men and students, insure by their publicity thoroughness and impartiality, and are the foundation of Germany's medico-legal clinics which provide the practical instruction indispensable to the education of competent experts. All the facts and proofs derived from the examination, and essential to a conclusion, are made parts of a written report, and these reports, with subsequent action thereon, are preserved in official archives, and have been the source of Germany's fruitful medico-legal literature. (2) If prosecution or defence be dissatisfied, the expert's report, with all proofs preserved, is forwarded to a Medical Court of Appeal, which, consisting of from four to six members, exists in each province. (3) If this second decision prove unsatisfactory, there is a final appeal to a Scientific Deputation, composed of experts of national reputation.[3] (4) Finally, one of the highest officers of the government, the Minister of Medical Affairs, presides over an educational and a sanitary, as well as over this medico-legal organization, and in fact rules a department of State Medicine.

Has State Medicine become necessary to a nation's progress in civilization? Can services essential to the welfare of a people be rendered by other than medical officers? Who will deny that no well-governed State can dispense with medical instructors; with physicians in charge of its hospitals and asylums; with medico-legal experts; with inspectors to watch over the execution of proper laws for prohibiting quackery and the sale of quack, fœticidal, poisonous, and adulterated drugs and food, and also to certify to every death with its cause, after personal examination;[5] with registrars of vital statistics to record not only marriages,

[1] See Preface to the French edition of Casper (B. 88).

[2] Sheldon Amos, Esq., Prof. of Jurisprudence, University College, London (see "Science of the Law," New York, 1874), in discussing the expert problem, says: "Almost all the solutions point to the public organization of bodies of skilled witnesses in each of the important departments in which they are constantly demanded, and to a special preference being given to their evidence in the administration of justice." Francis Wharton, Esq., (B. 264, II. p. 1120) urges that the German system is the only one worth imitating. In these views I thoroughly concur, notwithstanding the serious objections urged by such high authorities as J. Fitz Stephen, Esq., and I. Ray, M.D.

[3] There are circumstances, says Casper, where any doctor may be called on, as an expert, in Prussia.

[4] Formerly in Prussia (probably still in parts of Germany) the collegiate medical faculties were appealed to, whenever the primary reports of the official experts were not satisfactory to both litigants.

[5] The English system of determining and certifying to death and its cause is certainly not inferior to any such system (or lack of system) existing in the United States; and yet so inefficient is this superior system that Prof. A. S. Taylor reports that a man registered his own death, and then based a fraudulent claim on the certificate of his death and on the fact of his burial, which he himself had supervised; and he further asserts that "all that is requisite for future murderers by poison to do, is to use small doses, combine the use of various drugs, and subpœna the proper medical witnesses for the defence." (See B. 358, I. pp. 167–8 and 196.)

births, and deaths, but also prevailing diseases with their causes; and, finally, with sanitary officers to guard the public health by vaccination, quarantine, seclusion, disinfection, and all known means?

While such services to the State would now confer incalculable benefits, these are not a tithe of those which the progress of medical science assures the future. But a patchwork of ill-digested laws cannot secure these benefits, nor mere practitioners of medicine render these services. To this end a well-organized system of State Medicine, administered by specially educated medical men, is indispensable; and however discouraging the difficulties, educational, legal, and political, in our path, these must be eventually overcome, or our country prove a laggard in the triumphant march which civilization, led by the hand of science, is now treading. One of these difficulties, an increase of officials, dangerous to a republic, repugnant to the people, is more serious in appearance than in reality; for our present posts for coroners, and for sanitary and other medical officers, would suffice for, at least, the initiation of an organized system of State Medicine. Far more serious difficulties are presented by those causes which now so often fill these posts with unqualified men, by the continual elections and "rotations in office," through which the people, with suicidal folly, eliminate from public service responsible and efficient servants. If the demoralizing political principle, "to the victors belong the spoils," is to continue its mastery over the virtue and intelligence of a great people, then all hope of efficiency in any system of State Medicine, as well as in every public service which requires special skill and experience, must be abandoned. But, if the cardinal maxim of our political faith be well founded, if it be true that a republican government is better adapted than any other to secure the greatest good to the greatest number, then, though public enlightenment develop slowly, the day must come at last when all impediments will be overthrown, and an efficient system of State Medicine be organized by our laws. This progress, as all others, must pass through stages of evolution, and expediency force the acceptance, as now, of mere make-shifts; but this conviction should not deter the attempt to measure the full extent of our defects and of our needs, nor prevent us, while conscious that we are but scratching the surface of great evils, from striving to direct our efforts to their very root.

Honored Members of this International Medical Congress: Laboring on the task now completed, professional instinct prompted as sedulous a search for the symptoms of disease which skill might alleviate, as for the evidences of health which need no aid; for the instruction of my successor of 1976 (the one man who, should there be no other, will study this Address), I have deemed it as needful to record that which has been left undone, as that which has been done; and, while mindful of gratitude's debt to the honored living and illustrious dead, I have endeavored to keep in mind that the chief object of history is to teach wherein and why we have failed, that we may do better in future.

Actuated by these motives, I have thought no time more appropriate to note our failures than this Centennial Year, which gathers from far beyond our own vast domain, conclusive evidences of the might and fame of this nation. To its colossal power, how harmless is the voice of malice; to its dignity, how offensive should be the voice of flattery; to its honor, how unwelcome aught save the voice of truth! What higher evidence of true greatness, what better pledge of continued prosperity,

can this people give, than by proofs that now, in the day of its supreme vigor and renown, it is intent upon its own shortcomings; and that, beneath a flimsy veil of national vanity, there sturdily stands "the pith o' sense, and pride o' worth"! If I have dwelt unduly on my country's faults, it has not been from lack either of devotion to its form of government, or of just pride in its many noble deeds in humanity's behalf; but that I would have our Republic foremost in every good work, its progress endless, and its glory deathless.

BIBLIOGRAPHICAL APPENDIX.

Explanations and Abbreviations.

To economize space, the titles and places of publication of the general treatises in foreign languages have been omitted, except in a few instances of those of great note. For the same purpose, each author has been numbered, and is referred to by number in the notes to the Address, and in the final indices to this bibliographical appendix. Dates separated by a hyphen, *e.g.* 1621–58, indicate that the publication of the book in different parts was begun in 1621, and completed in 1658; dates separated by a comma, *e.g.* 1598, 1602, 1671, indicate the dates of the publication of different editions; and dates repeated, or closely approximating, indicate the publication of different editions in different places.

This contribution to the bibliography of American Medical Jurisprudence is preceded by a bibliographical record of the medico-legal literature of Italy, Germany, France, and Great Britain; and the whole has been arranged chronologically to illustrate the origin and progress of medico-legal science.

Wildberg's "Bibliotheca Medicinæ Forensis, 1819," records 2980 treatises, essays, etc., published from the origin of medico-legal literature (about A. D. 1600) to 1818. Orfila's Médecine Légale, 4th ed., furnishes a list of 168 treatises (to 1848), on poisons generally; of these 100 were German, 33 French, and 19 English. An English authority, the New Sydenham Society's "Year Books" for 1859 and 1860, reports the chief literary contributions of all nations to Medical Jurisprudence and Toxicology during the two years 1858 and 1859; the total number was 498, viz., 250 for Legal Medicine and 248 for Toxicology: and of the former, Germany contributed 201, and of the latter, 118. These, with other facts, indicate that Germany continues to contribute more to medico-legal literature than all other nations combined; and that a complete bibliography of the Medical Jurisprudence of all languages must report many thousands of books, essays, and articles. Hence the bibliographical sketch of foreign literature attempts no more than the record of treatises and manuals on the general subject; preceding these with a list of the books on special subjects which were published prior to the first general treatise; and following these with a reference to a few authorities of great repute on special subjects. It deserves note that in every language the number of publications on special subjects, after the issue of the first general treatise, has been very great, and proportionate to the number of general treatises.

The contribution to the bibliography of American Medical Jurisprudence attempts to report all the treatises, books, essays, and pamphlets, by native and foreign authors, published in the United States; the republication of foreign works being indicated by a †. Only a few articles from Journals are cited, and these for a special purpose. The bibliography of Medical Jurisprudence, as represented by "articles in journals, transactions, and collections," giving "the results of the examination of about 5000 volumes of such publications," will constitute a part of the "Catalogue of the National Medical Library," now in course of preparation by Dr. John S. Billings, Asst. Surgeon U. S. A., and Librarian; and will probably be published in 1877. Further, the New

York Medico-Legal Society is preparing for publication an extensive bibliography of Medical Jurisprudence. These two sources will no doubt supply all defects and omissions in this contribution.

For articles and reports of merit, the reader is referred especially to the American Jurist and Law Magazine, Boston, 1829 to 1843; American Journal of Insanity, Utica, N. Y., since 1844; Reports of the Association of Medical Superintendents of Hospitals for the Insane in North America, since 1844; Transactions of the Medical Society of the State of New York, prior to 1845; Transactions of the American Medical Association, since 1848; Albany Law Journal, since 1870; Annual Reports of the American Association for the Cure of Inebriates, since 1871; the Psychological and Medico-Legal Journal, N.Y., and the Series of Papers by the N. Y. Medico-Legal Society, both since 1874.

ITALY.

General Treatises.—1. Fidelis, Fortunatus, De Relationibus Medicorum, 1598, 1602, 1671. 2. Zacchias, Paulus, Questiones Medico-legales, &c., Books I. to X., 1621–58, 1651, 1655–61, 1657, 1666, 1688, 1674–1701; Nuremberg, 3 vs. 1720; Venise, 1737. 3. Tortosa, "best book to its date," 2 vs. 1802, 1809. 4. Sidoti, 1806. 5. Barzelotti, "very excellent," 2 vs. 1818, 1823–4, 1839. 6. Martini, 3 vs. 1825. 7. Taurini, 4 vs. 1832, 1841–4. 8. Speranza, 1833. 9. Grotanelli, 1834. 10. Clédillot, 1836. 11. Riscica, 1836. 12. Gianelli, 1836. 13. Perrone, 2d ed. 1840. 14. Presutti, 1841–4. 15. Treschi, 3 vs. 1846. 16. Lazzaretti, 1859. 17. Gandolfi, 1854, 1863.

Special Treatises.—18. Morgagni, De Sedibus et Causis Morborum, 1761. 19. Beccaria, Scriptura Medico-Legalis, 1764; and "Crimes and Punishments;" republished in Italian, 1821; French, 1821; English, 5th ed. 1804; Philadelphia, 1778 and 1793.

GERMANY.

Earliest Treatises on Special Subjects.—20. Condronchus, Methodus Testificandi, 1597. 21. Libavius, De Cruentatione Cadaverum, 1599. 22. Ammann, Irenicum, &c., 1619, and Medicina Critica, 1677. 23. Behrens, Medicus Legalis, 1696. 24. Wolf, Cogitat. Med.-Leg. 1697. 25. Valentini, Pandectæ Med.-Leg. 1701, and Novellæ Med.-Leg. 1718. 26. Fischer, Consilia Medic., &c., 1703–6, and Responsa Pract. et Forens. Select. 1719. 27. Bohn, De Officio Medici, 1704, and De Renunciatione Vulnerum, 1689, 1710 (both of note). 28. Zittman, Medicina Forensis, 1706.

General Treatises.—29. Valentini, Corpus Juris. Med.-Leg. ("best book to its date"), 2 vs. 1722. 30. Teichmeyer, Institutiones Med.-Leg., &c. ("long the Manual of Students"), 1722, 1731, 1740, 1762, 1769. 31. Albertus, Systema Jurisp. Medicæ, &c. ("very excellent"), 1725, 1733–47, 1756. 32. Goelicke, 1723. 33. Loew, 1725. 34. Storck, 1730. 35. Tropanneger, 1733. 36. Eschenbach, 1746, 1775. 37. Hebenstreit, Anthropologia Forensis, &c. ("much valued by lawyers"), 1751. 38. Furstenau, 1752. 39. Boerner, 1756. 40. Bernhold, 1760. 41. Ludwig, 1765, 1774. 42. Faselius, Elementa Medicinæ Forensis, 1767, 1775 ("*foundation*" of Farr's Elements of Med Jurisp., 1788, the first treatise in the English language). 43. Braendel, 1768, 1789. 44. Kannegiesser, 1768, 1772, 1777, 1778. 45. Baumer, 1778. 46. Sikora, 1780, 1792. 47. Plenck, Elementa Med. et Chir. Forensis, 1781, 1786, 1802 (foundation of Male's Epitome of Forensic Medicine, London, 1816). 48. Haller, 1782–4. 49. Schlegel, Collect. Opusc. Select. ad Med. For. Spect., 6 vs. 1785–90 ("best book to its date"). 49. Loder, 1793. 50. Metzger, System. der gerichtl. Arzney. ("best book to its date"), 1793, 1794, 1798, 1803, 1814, 1820, and two French editions: Autun, 1812; Paris, 1813. 51. John, 1795. 52. Fahner,

3 vs. 1795–1800. 53. Müller, 4 vs. 1796–1802. 54. Erhard, 1800. 55. Schrand, 1802. 56. Schmidtmuller, 1804. 57. Masius, 1810, 1812, 1821–3. 58. Bene, 1811. 59. Wildberg, 1812, 1824. 60. Henke, Lehrb. der gerichtl. Med., 1812, and Bergmann's 13th ed., 1859. 61. Niemann, 2 vs. 1813. 62. Bernt, 1813, and 5th ed. 1846. 63. Dorn, 1813. 64. Klose, 1814. 65. Sprengel, 1816. 66. Mende, Handb. der gerichtl. Med. ("a remarkable book"), 6 vs. 1819–32. 67. Meckel, 1821. 68. Platner, 1824. 69. Niemann, 3 vs. 1827–9. 70. Wagner, 1833–40. 71. Sporer, 1837. 72. Siebenhaar, 2 vs. 1838. 73. Freidreich, 2 vs. 1839, 1843, 1848, 1855. 74. Rolff, 1840. 75. Nicolai, 1841. 76. Ney, 1845. 77. Bergmann, 1846. 78. Brach, 1846, 1850. 79. Siebold, 1847. 80. Meckel, 1848–53. 81. Schürmayer, Lehrbuch der gerichtl. Med., 1848, 4th ed., 1874. 82. Quentner, 1851. 83. Güntner, 1851. 84. Krahmer, Handb. der gerichtl. Med., 1851, 1857; and 85. Handb. der Staatsarzneik., 1874. 86. Böcker, 2d ed., 1857. 87. Hauska, 1857–69. 88. Casper, Handb. der gerichtl. Med., 1857, 1858; 3d was the first complete ed., 1860, 1864; Liman's 5th ed., 1871; French ed., 2 vs. 1862; English ed., 4 vs. 1861–5; and translated into several other languages. 89. Hofman, 1860. 90. Pichler, 1861, 1867. 91. Lion, 1861, 1867. 92. Schauenstein, 1867. 93. Büchner, 1867; 2d ed., 1872. 94. Kraus und Pichler, Encyc. Wörterbuch d. Staatsarzneik., 2 vs. 1873.

N. B.—Five of the above sixty-five German general treatises have been published since 1870, viz.: Casper (Liman's 5th ed.), 1871; Büchner, 2d ed., 1872; Kraus & Pichler, 1873; Krahmer, 1874; Schürmayer, 4th ed., 1874.

Special Treatises.—Among the most distinguished German authors on special subjects have been: 95. Roose, Grund. Med.-gericht Vorles., 1798, 1800, 1802; translated into French by Marc, Manuel d'Autops. Cadav. Méd.-Lég., Paris, 8vo. 1808. 96. Hoffbauer, 1802, 1807, 1809; translated into French, Méd. Lég. relative aux Aliénés, &c., Paris, 1827. 97. Heinroth, 1812, 1825. 98. Mittermaier, 1856, 1858, 1865. 99. Krafft-Ebing, 1868, 1872, 1875. All of these in reference to the Medical Jurisprudence of Insanity or Medico-Legal Psychology.

France.

Earliest Treatises on Special Subjects.—100. Paré, Ambroise, Medical Reports, Monstrous Births, Feigned Diseases, 1575, 1582, and Œuvres Complètes, Paris, 3 vs. 1840. 101. Pineau, Integ. et Corrup. Virgin., 1598. 102. Riolan, Les Monstres, 1605. 103. Tagereau, L'Impuissance, 1611. 104. Duval, L'Hermaphrodisme, 1612. 105. Sebitz, La Virginité et L'Hymen, 1630, 1638. 106. Gendri, Moyens de bien rapporter à justice, 1650. 107. Blegni, La Doct. des Rapports de Chirurgie, 1684. 108. Devaux, L'Art de faire des Rapports en Chirurgie, 1693, 1701, 1730, 1746. 109. Bruhier, L'Incertitude des Signes de la Mort, &c. (180 cases of living, treated as if dead), 1742. 110. Verdier, La Jurisp. de la Méd., &c., 1752. 111. Prévost, Principes de Jurisp., &c., Méd.-Lég., 1763. 112. Louis, A. Signes de la Mort, &c.; Exps. sur les Noyés; Recueil d'Observations, &c.; Causes Celèbres (first 43 vols.), 1752–88. 113. Chaussier, Obs. Chirur.-Légales, &c., 1790, 1824, 1838.

N. B.—The most noted of these were Paré, Gendri, Blegni, Devaux, Louis, and Chaussier.

General Treatises.—114. Foderé, Traité de Méd. Lég. et d'Hyg. Pub., 3 vs. 1796; 2d ed., 6 vs. 1813. 115. Mahon, 1801, 1807, 1811. 116. Belloc, 1801, 1811, 1819. 117. Vigné, 1805. 118. Bertrand, 1817. 119. Lecieux, &c., 1805. 120. Biessy, 1821. 121. Orfila, Leçons de Médecine Légale, 3 vs. 1821–5, 1828, 1839; 4th ed., 1848. 122. Briand, Manuel de Médecine Légale, 1821, 1828; Briand et Bresson, 3d ed., 1830; Briand et Chaudé, 1846, 1852, 1858, 1864, 1869; Paris, 9th ed., pp. 1102, 8vo. fort., 1874. 123. Sédillot, 1830, 1833. 124. Trinquier, 1836–8. 124*a*. Devergie, Médecine Légale, 3 vs. 1837, 1840, 1852. 125. Bayard, 1844. 126. Legrand du Saulle, Ortolan, et Naquez, 1870.

127. Legrand du Saulle, Traité de Médecine Légale et de Jurisp. Méd., Paris, pp. 1268, 1874. 128. Bergeron, 1876.

Special Treatises.—Among the most distinguished French authors on special subjects may be mentioned: 129. Pinel, Ph., 1800, 1805, 1809. 130. Georget, 1820, 1826, 1828. 131. Esquirol, 1832, 1838. 132. Marc, 1840. 133. Brierre de Boismont, 1827–1860. (All on Medico-Legal Psychology.) 134. Capuron, Obstetrical Medical Jurisprudence, 1821. 135. Trebuchet, Jurisp. de la Médecine, &c., 1834. 136. Poilroux, Manuel de Méd.-Lég. Criminelle, 1834, 1837. 137. Tardieu, A., numerous experimental and literary contributions, entitling him to the first rank among the medico-legists of the present time, 1845 to 1876.

GREAT BRITAIN, ETC.

Earliest Treatises on Special Subjects.—138. Percival, Thos., Essays Med. and Exp., &c., 1778–90. 139. Wm. Hunter, Uncertainty of Signs of Murder in Bastard Children, 1783. 140. Goodwyn, Submersion, Strangulation, &c., 1788. 141. Crichton, Mental Derangements, 1798. 142. Haslam, Insanity, and Medical Jurisp. of Insanity, 1798, 1809, 1817, 1819. 143. Johnstone, Madness, Med. Jurisp. of, 1800. 144. Perfect, Insanity, 2d ed., 1801. 145. Collinson, Lunacy, Law of Idiots, Lunatics, &c., 1812. 146. Johnson, C., Signs of Murder in New-Born Children (from the French of Mahon), 1813. 147. Hill, Insanity, 1814. 148. Hutchinson, Infanticide, 1820. 149. Burrows, Insanity, 1820, 1828.

General Treatises.—150. Farr, Saml., Elements of Medical Jurisprudence (founded on the German of Faselius, 1767, see 42), London, 12mo. 1788; 2d ed., pp. 139, 12mo. 1814. 151. Bartley, Treatise on Forensic Med. (said to be very insignificant), Bristol, 12mo. 1815. 152. Male, Epitome of Forensic Medicine (largely from the German of Plenck, see 47), London, 12mo. 1816; 2d ed., 1818. 153. Smith, John Gordon, Principles of Forensic Medicine (the first book in the English language of any original merit), London, pp. 503, 8vo. 1821; 2d ed., 1824; 3d ed., 1827. 154. Paris and Fonblanque, Medical Jurisprudence (best book to its date), London, 3 vs. 1823. 155. Forsyth, Synopsis of Modern Medical Jurisprudence (denounced by Beck as a wholesale plagiarism), London, pp. 600, 1829. 156. Ryan, Manual of Med. Jurisp., London, pp. 309, 8vo. 1831; 2d ed., 1836. 157. Chitty, Med. Jurisp. (never completed), London, Part I. 8vo. 1834. 158. Traill, Outlines of Med. Jurisp., Edinb. 12mo. 1836; 2d ed., 1840. 159. Brady, Med. Jurisp., Dublin, 1839. 160. Taylor, A. S., Elements of Med. Jurisp., Vol. I., pp. 511, 8vo., London, 1836 (a second volume never published). Manual of Med. Jurisp., 1st ed., 1844; 8th ed., 1866; 9th ed., London, pp. 772, 8vo. 1874. N. B.—Seven American and one German ed., 1858. 161. Guy, W. A., Principles of Forensic Medicine, London, 1843; 2d ed., pp. 534, 1861; 3d ed., 1868. 162. Taylor, A. S., Principles and Practice of Medical Jurisprudence, London, 1865; 2d ed., 2 vols. pp. 723, 672, 8vo. 1873. 163. Husband, Forensic Medicine and Med. Police, Edinb. 1873. 164. Woodman and Tidy, Handy-book of For. Med. and Toxicology, London, 1875.

Special Treatises.—Among the most distinguished British authors on special subjects, and whose books have not been republished in America (see 201 to 390), may be mentioned—165. Conolly, 1830. 166. Pagan, 1839, 1843—both on the Med. Jurisp. of Insanity. 167. Watson, Alex., Homicide, 1837; 2d ed., 1842. 168. Gavin, H., Feigned and Factitious Diseases, 1843.

CONTRIBUTION TO THE BIBLIOGRAPHY OF AMERICAN MEDICAL JURISPRUDENCE.

Republications of foreign works are indicated by a †, and pamphlets by a *; the latter are for the most part reprints from Journals, etc. Some works not strictly medico-legal are given, because often referred to by medico-legists.

201. †Cox, J. M. Observations on Insanity, Med. Jurisp. of, from the 2d London ed. Phila., 1811.
202. Rush, Benj. Introductory Lectures (16th). 8vo. Phila., 1811.
203. Rush, Benj. Diseases of the Mind. 8vo. pp. 367. Phila., 1812. 2d and 3d ed.? 4th ed. 1830, 5th ed. 1835.
204. *Ducachet. Dissertation on Action of Poisons. New York, 1817.
205. *Beck, John B. Infanticide. 8vo. pp. 95. New York, 1817. (*Republished with additions, etc., in the various editions of* "*Beck's Med. Jurisp.*")
206. †Orfila. Poisons, by R. H. Black. 12mo. pp. 240. Baltimore, 1819.
207. †Cooper, Thos. Tracts on Med. Jurisp., viz., of Farr, Dease, Male, and Haslam (British). 8vo. pp. 456. Phila., 1819.
208. †Highmore. Law of Idiocy and Lunacy. 8vo. Exeter, N. H., 1822. (1*st Am. from last English ed.*)
209. Beck, T. R. Elements of Medical Jurisprudence, with the Chapters on Infanticide and Abortion, by J. B. Beck. (*First American General Treatise.*) 2 vols., 8vo. pp. 418, 471. Albany, N. Y., 1823.
    2d ed. with notes by Dr. W. Dunlop. London, 1825.
    3d and 4th eds. dates unknown. One at least was a London ed. with notes by Dunlop and Darwell.
    German edition (translated). Weimar, 1828.
    5th ed. (2d American ed.) by T. R. and J. B. Beck. 2 vols., pp. 661, 694. Albany, 1835. 5th ed. London, 1836.
    6th ed. (3d American) pp. 670, 743. Phila., 1838.
    7th ed., with notes by Dunlop and Darwell. pp. 1138. London, 1842.
    8th and 9th editions, dates and places of publication unknown.
    10th ed. 2 vols. Albany, 1850–51.
    11th ed., revised by Dr. C. R. Gilman, with Drs. D. T. Brown, R. H. Coolidge, A. Flint, Sr., B. W. McCready, S. St. John, Jno. Watson, and J. P. White, as Collaborators. 8vo. 2 vols., pp. 884, 1003. Phila., 1860.
    12th ed. Same Editor and Collaborators. 8vo. 2 vols., pp. 894, 995. Phila., 1863.
210. *Webster, J. Essay on Med. Jurisp. 8vo. pp. 52. Phila., 1824.
211. †Morgagni. Seats and Causes of Disease. (*Cooke's English Translation.*) 8vo. 2 vols. Boston and Phila., 1824.
212. †Orfila. Poisons (translated by J. G. Stevenson). 12mo. Boston, 1826.
213. Warren, J. C. Dislocations of Hip-Joint (Malpractice). 8vo. pp. 142. Cambridge, Mass., 1826.
214. *Dunglison, R. Syllabus of Lectures on Med. Jurisp. 8vo. pp. 142. Univ. Va., 1827.
215. Horner, W. E. Healthy and Diseased Appearances of the Mucous Membranes of the Stomach and Intestines. [With experiments.] Am. Journ. Med. Sci., pp. 1–40, Nov. 1827.
216. *Simons, T. Y. Mental Alienation, Med. Jurisp. of. 8vo. Charleston, S. C., 1828.
217. Horner, W. E. Pathological Anatomy. 8vo. pp. 460. Phila., 1829.
218. Lobstein, J. F. D. Semeiology of Countenance, etc., in Med. Jurisp. 8vo. pp. 150. N. Y., 1830.
219. *Hance, W. Appeal to Citizens of Ohio. . . . Medical Law, etc. 8vo. pp. 20. Columbus, O., 1830.

220. Rush, B. Diseases of the Mind. (See 203.) 4th ed. 1830.
221. Dubois, W. E. Trial of Lucretia Chapman (Poison). 8vo. pp. 213. Phila., 1832.
222. Coley, H. (of London). Treatise on Med. Jurisp. Part I. Poisons and Asphyxia. (*Never completed.*) 8vo. pp. 72. N. Y., 1832.
223. †Ryan. Manual of Med. Jurisp., with notes, etc., by Dr. R. E. Griffith. 1st Am. ed. 8vo. pp. 327. Phila., 1832.
224. Ducatel. Toxicology (*from Christison, and for practitioners rather than for medico-legists*). 12mo. Balt., 1833.
225. Gross, S. D. Experiments on Strangling. Western Journ. Med. and Surg. Vol. 9, 1833.
226. †Combe, And. Mental Derangements (1st Am. ed). 8vo. pp. 336. Boston, 1834.
227. Beck, J. B. Researches in Medicine and Med. Jurisp. 8vo., 2d ed., pp. 258. N. Y., 1835.
228. Williams, S. W. Catechism of Med. Jurisp. (Very severely criticized.) 18mo. pp. 205. Northampton, Mass., 1835.
229. Beck, T. R. and J. B. Med. Jurisp. (See 209.) 5th ed., 1835.
230. Rush, B. Diseases of the Mind. (See 203.) 5th ed. 1835.
231. †Chitty. Med. Jurisp. Part I. Am. ed. 8vo. pp. 509. (*Never completed.*) Phila., 1836.
232. †Prichard, J. C. Insanity. Am. ed. 8vo. pp. 111. Phila., 1837.
233. †Taylor, A. S. Observations and Experiments on Lungs of New-Born Children. Phila., 1838.
234. Ray, Isaac. Medical Jurisprudence of Insanity. ("Best book in English language to date," reprinted in England and Scotland.) 8vo. Boston, 1838.
   2d ed., pp. 490, 1844; 3d ed., 1853; 4th ed., 1860; 5th ed., pp. 658, 1871.
235. Beck, T. R. & J. B. Med. Jurisp. (See 209.) 6th ed., 1838.
236. *Hodge, H. L. Criminal Abortion. Phila., 1839; Phila., 1854; 12mo. pp. 44, Phila., 1869; 4th ed., 1873.
237. Gross, S. D. Pathological Anatomy. 2 vols., 1839. 2d ed., 8vo. pp. 822, Phila., 1845; 3d ed., pp. 771, 1857.
238. †Montgomery. Signs and Symptoms of Pregnancy. pp. 220, Phila., 1839; from London ed., 1837. 2d Am. from 2d London ed., 8vo. pp. 568, Phila., 1857.
239. †Traill. Outlines of Med. Jurisp.; (from 2d Edinb. ed.) 8vo. pp. 234. Phila., 1841.
240. †Winslow, F. Plea of Insanity in Criminal Cases. 8vo. pp. 111. Boston, 1843.
241. Ray, I. Med. Jurisp. of Insanity. (See 234.) 2d ed., 1844.
242. Gross, S. D. Path. Anatomy. (See 237.) 2d ed., 1845.
243. †Esquirol. Mental Maladies. Notes, etc., by E. K. Hunt. 8vo. pp. 496. Phila., 1845.
244. †Christison. Poisons. Edited by Jno. Bell. 8vo. pp. 756. Phila., 1845. (Reprint 4th Edinb. ed.)
245. †Forbes, Tweedie, and Conolly. Cyclopædia of Practical Medicine, with additions, etc., by R. Dunglison. (Contains many valuable articles on Med. Jurisp.) 4 vols. 8vo. Phila., 1845.
246. †Guy, W. A. Forensic Medicine, with Notes, etc., by Chas. A. Lee. 8vo. pp. 771. New York, 1845.
247. †Taylor, A. S. Manual of Med. Jurisp., with Notes, etc., by R. E. Griffith, M.D. 8vo. pp. 539. Phila., 1845.
   Same by Griffith. 2d Am. from 3d London ed., 1850.
   Same by E. Hartshorne, M.D. 3d Am. from 4th London ed., 1853.
   Same by Hartshorne. 4th Am. from 5th London ed., 1856.
   Same by Hartshorne. 5th Am. from 7th London ed., 1861.
   Same by C. C. Penrose, Esq. 6th Am. from 8th London ed., 1866.
   Same by Jno. J. Reese, M.D. 7th Am. from 9th London ed., 1873.

248. Crozier. Life and Trial of Dr. Abner Baker. 8vo. pp. 152. Louisville, Ky., 1846.
249. *Purple, S. S. Corpus Luteum. (Med.-Legal, from N. Y. Jour. Med.) 8vo. pp. 13. N. Y., 1846.
250. Hall, B. F., Esq. Freeman Trial (Van Nest Murder). 1847.
251. Costill, O. H. Pract. Treatise on Poisons. 12mo. pp. 160. Phila., 1848.
252. *Taylor, A. S. Poisons, with Notes, etc., by R. E. Griffith. 8vo. pp. 687. Phila., 1848. 2d Am. from 2d London ed., 1859; 3d Am. from 3d London ed., pp. 788, Phila., 1875.
253. Beck, T. R. & J. B. Medical Jurisp. (See 209.) 10th ed., 1850.
254. Dean, Amos, Esq., Prof. Med. Jurisp., etc. Medical Jurisprudence. 8vo. pp. 664. Albany, N. Y., 1850. 2d ed., 1854; 3d ed., 1873.
255. Bemis, G., Esq. Parkman-Webster Murder. Boston, 1850.
256. †Taylor, A. S. Manl. of Med. Jurisp. (See 247.) 2d Am. ed., 1850.
257. *Dalton, J. C. Corpus Luteum. Trans. Am. Med. Asso., vol. 4, 1851.
258. *Storer, D. H. Address on Med. Jurisp. Boston, 1851.
259. *Binney, H., Esq. Coroner, Jurisdict. of. Phila., 1853.
260. Ray, I. Med. Jurisp. of Insanity. (See 234.) 3d ed., 1853.
261. *Taylor, A. S. Manl. Med. Jurisp. (See 247.) 3d ed., 1853.
262. Dean, Amos, Esq. Medical Jurisp. (See 254.) 2d ed., 1854.
263. Hodge, H. L. Criminal Abortion. (See 236.) 2d ed., 1854.
264. Wharton, Francis, Esq., and Stillé, Moreton, M.D. Medical Jurisprudence. 8vo. pp. 815. Phila., 1855. 2d ed., "Wharton and Stillé," and Prof. Alfred Stillé, M.D., pp. 1031, 1860; 3d ed., "Wharton and Stillé," and Drs. S. Ashhurst, Sinkler, and Amory, two vols. in three, Phila., 1873.
265. Wharton, F., Esq. Mental Unsoundness. Phila., 1855.
266. Hamilton, F. H. Deformities after Fractures (Malpractice). 8vo. pp. 99, Phila., 1855, and Tr. Am. Med. Asso., vols. 8, 9, 10. Treatise on Fractures and Dislocations, 8vo. pp. 757, Phila., 1860; 5th ed., 1875.
267. *Wetherill. Adipocere and its Formation. Folio, pp. 25. Phila., 1855, and in Tr. Am. Philosoph. Society, vol. 11, Phila.
268. *Lee, C. A. Case of John Hendrickson, Jr., for Wife-murder (Poison). 8vo. pp. 41. N. Y., 1855.
269. Purple, S. S. Wounds of Heart, and their Relations to For. Med., with 42 Cases. N. Y. Med. Times, May, 1855.
270. †Brierre de Boismont. Hallucinations. (Am. from 2d Paris ed.) 8vo. pp. 553. Phila., 1855.
271. *Gilman, C. R. Relations of Medical to Legal Profession. 8vo. pp. 26. N. Y., 1856.
272. †Taylor, A. S. Manual Med. Jurisp. (See 247.) 4th Am. ed., 1856.
273. Gross, S. D. Path. Anat. (See 237.) 3d ed., 1857.
274. *Semmes, A. J. Med.-Legal Duties of Coroners. 8vo. pp. 16, and Tr. Am. Med. Asso. Phila., 1857.
275. *Jarvis, E. Criminal Insane. 8vo. pp. 39. Utica, N. Y., 1857.
276. *Earle, P. Parish Will Case (Apoplexy and Paralysis). 8vo. pp. 50. N. Y., 1857.
277. *Watson, Browne, etc. Parish Will Case. N. Y., 1857.
278. *Gilman, C. R. Case of C. B. Huntington (Insanity). 8vo. pp. 24. N. Y., 1857.
279. *Suffolk Dist. Med. Society. Criminal Abortion. 8vo. pp. 13. Boston, 1857.
280. †Otto, J. Poisons, by W. Elderhorst. N. Y., 1857.
281. †Montgomery. Signs and Symptoms of Pregnancy. (See 238.) 2d Am. ed., 1857.
282. *Reese, D. M. Moral Insanity, Med. Jurisp. of. 8vo. pp. 26, and Tr. Am. Med. Asso. Phila., 1858.
283. *Coventry, C. B. Med. Jurisp. of Insanity. 8vo. pp. 56, and Tr. Am. Med. Asso. Phila., 1858.

284. *Perkins, R. G. Insanity in its Legal Relations. 8vo. pp. 24. N. Y., 1858.
285. *Davis, T. G. C. Plea of Insanity in Case of R. C. Sloo. 8vo. pp. 53. St. Louis, Mo., 1858.
286. †Bucknill and Tuke. Manual of Psycholog. Med. Phila., 1858.
287. *Fleming, A. Blood Stains. Pittsburg, Pa., 1861, and Am. Journ. Med. Sci., 1859.
288. *Wright, M.D. Drunkenness and Inebriate Asylums. 8vo. pp. 22. Columbus, O., 1859.
289. †Taylor, A. S. Poisons. (See 252.) 2d Am. ed., 1859.
290. Elwell, J. J., M.D., Esq. Malpractice and Medical Evidence. 8vo. pp. 588. N. Y., 1860. 2d ed., 1866 (?); 3d ed., 8vo., pp. 600, 1871.
291. Hall, A. G. Institutes of Med. Jurisp. ("*A senseless monthly serial of a quack, having no reference, except in name, to Med. Jurisp.*") 8vo. pp. 576. Boston, 1860.
292. Beck, T. R. and J. B. Med. Jurisp. (See 209.) 11th ed., 1860.
293. Ray, I. Med. Jurisp. of Insanity. (See 234.) 4th ed., 1860.
294. Wharton and Stillé. Med. Jurisp. (See 264.) 2d ed., 1860.
295. *Wharton, F. Involuntary Confessions. 8vo. pp. 36. Phila., 1860.
296. *Wright, T. L. Med. Leg., Inquiry on Influence of Chloroform. 8vo. pp. 32. Columbus, O., 1860.
297. †Winslow, F. Brain and Mind. 1st Am. ed., Phila., 1860.

N.B. "*War between the States,*" 1861–1865.

298. †Taylor, A. S. Manl. Med. Jurisp. (See 247.) 5th ed., 1861.
299. *Swinburne, John. Case of Rev. H. Budge (Wife-murder). 8vo. pp. 94. Albany, 1862, and Tr. Med. Soc. State of N. Y., 1862.
300. *Swinburne, J. Trial of Hendrickson (Wife-murder. Aconite). 8vo. pp. 16. Phila., 1862.
301. Beck, T. R. and J. B. Med. Jurisp. (See 209.) 12th ed., 1863.
302. *Parigot, J. Legislation on Insanity. 8vo. pp. 14. N. Y., 1865.

302*a*. Elwell's Malpractice and Med. Ev. (See 290.) 2d ed., 1866 (?).
303. Seguin. Idiocy. 8vo. pp. 457. N. Y., 1866.
304. Hammond, W. A. Insanity in its Medico-legal Relations. 8vo. pp. 72. N. Y., 1866.
305. Storer, H. R. Why Not? (Abortion.) 12mo. pp. 91. Boston, 1866. [2d ed., 1871?]
306. †Winslow, F. Obscure Dis. Brain and Mind, 2d Am. from 3d Lond. ed. 8vo. pp. 483. Phila., 1866.
307. Wormley, Theo. G. Micro-Chemisty of Poisons. 8vo. pp. 668. N. Y., 1867.
308. Day, A. Methomania. 16mo. Boston, 1867.
309. Storer, H. R. Is it I? (Abortion.) 12mo. pp. 154. Boston, 1867.
310. *Le Prohon. Voluntary Abortion. 8vo. pp. 24. Portland, Me., 1867.
311. *Wetmore, E. Law of New York regarding the Insane. 8vo. pp. 35. Utica, 1867.
312. *Forbes, W. S. History of Anatomy Act of Pa. 8vo. Phila., 1867.
313. †Maudsley. Mind, Physiol. and Pathol. of. 8vo. pp. 442. N. Y., 1867. 2d ed., 1871.
314. Storer and Heard. Criminal Abortion. 8vo. pp. 215. Boston, 1868.
315. *Todd, J. Serpents in the Dove's Nest (Abortion). 8vo. pp. 28. Boston, 1868.
316. *Lee, C. A. Report on Insanity. 8vo. pp. 32. Phila., 1868.
317. Ordronaux, John. Jurisprudence of Medicine in its Relations to the Laws of Contracts, Torts, and Evidence. 8vo. pp. 310. Phila., 1869.
318. Ordronaux, J. Appointment of a Commissioner to examine Witnesses in Med.-legal Trials. Tr. Am. Med. Asso., 1869.
319. Hodge. Criminal Abortion. (See 236.) 4th ed., 1869.

320. Richardson, Joseph G. Blood Stains. Am. Journ. Med. Sci., July, 1869.
321. Echeverria. Epilepsy. 8vo. pp. 386. N. Y., 1870.
322. *Hammond, W. A. Case of Daniel McFarland (Insanity). 8vo. pp. 35, N. Y., 1870, and in Quart. Journ. Psychol. Med.
323. *Lee, Charles A. Case of Carlton Gates (Insanity). 8vo. pp. 30. N. Y., 1870, and in First Series of Papers of N. Y. Med.-Legal Soc.
324. *Ray, I. Trial of John Reynolds. N. Y., 1870.
325. *Nebinger. Criminal Abortion. 8vo. Phila., 1870.
326. Hammond, W. A. Diseases of the Nervous System. 8vo. pp. 754. N. Y., 1871. (6th ed., 1876.)
327. Storer, H. R. Reflex Insanity in Women. 8vo. pp. 236. Boston, 1871.
328. Calkins, A. Opium and the Opium Appetite. 8vo. pp. 390. Phila., 1871.
329. Ray, I. Med. Jurisp. of Insanity. (See 234.) 5th ed., 1871.
330. Elwell, J. J. Malpr. and Med. Evidence. (See 290.) 3d ed., 1871.
331. *Gray, J. P. Insanity, Dependence on Physical Dis. 8vo. Utica, N. Y., 1871.
332. *Conklin, W. J. Relations of Epilepsy to Insanity and Med. Jurisp. 8vo. pp. 43. Dayton, O., 1871.
333. *Dimon, D. Freeman Trial (Murder). Auburn, N. Y., 1871.
334. †Blandford. Insanity, Medical and Legal, with a Synopsis of the laws of the several States of the Union respecting the confinement of the Insane, by I. Ray. 8vo. pp. 472. Phila., 1871.
335. †Maudsley. Body and Mind. 12mo. pp. 155. N. Y., 1871.
336. †Maudsley. Mind, Physiology and Pathol. of. (See 313.) 2d ed., 1871.
337. Delafield, F. Post-mortem Examinations. 8vo. pp. 376. N. Y., 1872.
338. Fisher, T. W. Plain Talk about Insanity (Med.-legal). 8vo. pp. 97. Boston, 1872.
339. Day, A. The Opium Habit. N. Y., 1872.
340. *Peet, I. L. Physical Status and Criminal Responsibility of the totally uneducated Deaf and Dumb. 8vo. pp. 31. N. Y., 1872, and in First Ser. Papers N. Y. Med.-Legal Soc.
341. *Van de Warker. Detection of Criminal Abortion. 8vo. pp. 88. Boston, 1872.
342. *New York Med.-legal Society. Report on Criminal Abortion. 8vo. pp. 15. N. Y., 1872.
343. *Crothers, T. D. Trial of Mrs. Dr. E. Burleigh (Abortion). 8vo. Albany, 1872.
344. *Williams, P. C. Ketchum-Wharton Trial (Antimony). 8vo. pp. 31. Baltimore, 1872, and Med. and Surg. Report., Phila., vol. xxvi. (See also Am. Journ. Med. Sci., April, 1872.)
345. *Wey, W. C. Medical Responsibility and Malpractice. 8vo. pp. 35. Albany, 1872.
346. *Stevens, T. M. Med.-legal Science. 8vo. Trans. Ind. State Med. Soc. Indianapolis, 1872.
346*a*. Dean, Amos, Esq. Med. Jurisp. (See 254.) 3d ed., 1873.
347. Ray, I. Contributions to Mental Pathology. 8vo. pp. 558. Boston, 1873.
348. Hammond, W. A. Insanity in its Relations to Crime. 8vo. pp. 77. N. Y., 1873.
349. Wharton and Stillé. Med. Jurisp. (See 264.) 3d ed., 1873.
350. Hodge. Criminal Abortion. (See 236.) 4th ed., 1873.
351. *Stevens, T. M. Expert Testimony. 8vo. Indianapolis, 1873, and in Ind. Med. Journ.
352. *McClelland. Civil Malpractice. 8vo. pp. 74. Chicago, 1873.
353. *Field, D. D., Esq. Emotional Insanity. 8vo. N. Y., 1873.
354. *Clinton, H. L., Esq. Defence of Insanity in Criminal Cases. N. Y., 1873.
355. *Morse, D. A. Dipsomania and Drunkenness. pp. 52, and in Ohio State Med. Soc. Trans., 1873.

356. *Bell, Clark, Esq. Inaug. Address N. Y. Med.-Legal Soc. 8vo. pp. 11. N. Y., 1873.
357. Echeverria. Epilepsy in relation to Med. Jurisp. and Epileptic Insanity. Am. Journ. Insanity, Utica, April and July, 1873.
358. †Taylor, A. S. Principles and Practice of Med. Jurisp. (Reprint of 2d London ed.) 8vo. 2 vols. Phila., 1873.
359. †Taylor, A. S. Manual Med. Jurisp. (See 247.) 7th Am ed., 1873.
360. †Tuke, D. H. Influence of Mind on Body. 8vo. pp. 415. Phila., 1873.
361. †Wynter, A. Borderlands of Insanity. Am. ed. 12mo. pp. 314. N. Y., 1873.
362. Reese, Jno. J. Manual of Toxicology. 8vo. pp. 507. Phila., 1874.
363. N. Y. Med.-Legal Soc. "First Series of Papers." 8vo. pp. 552. N. Y., 1874. (By Judge Campbell, Wm. and Jacob Shrady, and Tillou, Esq'rs, Mr. I. L. Peet, and Drs. O'Dea, Rogers, Lee, Hammond, Burr, Clymer, De Marmon, Parsons, and Peters.)
364. *Bell, C., Esq. Inaug. Address N. Y. Med.-Legal Soc. 8vo. pp. 23. N. Y., 1874.
365. *Beard, G. M. Legal Responsibility in Old Age. 8vo. pp. 42. N. Y., 1874.
366. *Morse, D. A. General Paralysis and Blackburn Case. pp. 120. 1874.
367. *Morse, D. A. Monomania. pp. 46. This and 366 in Ohio State Med. Soc. Trans., 1874.
368. *Clymer, M. Influence of Epilepsy on Criminal Responsibility. 8vo. pp. 25. Phila., 1874.
369. *Howard, E. L. Legal Relations of Emotional Insanity. 8vo., and Trans. Am. Med. Assoc. Phila., 1874.
370. *Peugnet, E. Gunshot Wounds of Abdomen (Stokes-Fisk Murder). N. Y., 1874.
371. †Maudsley. Responsibility in Mental Disease. 8vo. pp. 313. N. Y., 1874.
372. †Bucknill and Tuke. Manual of Psycholog. Med. 8vo. Phila., 1874.
373. *Richardson, Jos. G. Value of High Powers in Blood-Stains. 8vo. pp. 12, and in Am. Journ. Med. Sci., July, 1874, and London Mic. Journ., Sept. 1874.
374. *Richardson, J. G. Diagnosis of Blood-Stains. pp. 5. Am. Journ. Med. Sci., April, 1875.
375. Richardson, J. G. Improved Method for Micro-Spectrosc. Test for Blood-Stains. Phila. Med. Times, Nov. 1875.
376. *Woodward, J. J. Blood-Stains in Criminal Cases. pp. 12, and in Am. Journ. Med. Sci., January, 1875.
377. Smith, J. E. Diagnosis of Blood Stains. Cinn. Med. Times, April, 1875.
378. *Ray, I. Duncan Will Case, 1875.
379. *Allen, N. State Med. in its relations to Insanity. Boston, 1875.
380. *Bell, C., Esq. Inaug. Ad. N. Y. Med.-Legal Soc. 8vo. pp. 16. N. Y., 1875.
381. †Taylor, A. S. Poisons. (See 252.) 3d ed., 1875.
382. †Sheppard. Madness—Med., Social, and Legal, from London ed. of 1873. 8vo. pp. 186. Phila., 1875.
383. †Poppesis, B. Hermaphrodism (Med.-legal), Translated from the French by Dr. E. W. Sawyer, Chicago, 1875.
384. †Sieveking. Med. Adviser in Life Insurance, from London ed. of 1874. 12mo. Phila., 1875.
385. Cowperthwait, A. C. Insanity in its Med.-legal Relations. 8vo. pp. 80. Phila., 1876.
386. Hammond, W. A. Diseases of the Nervous System. (See 326.) 6th. ed., 1876.
387. *Bell, C., Esq. Valedict. Ad. N. Y. Med.-Legal Soc. 8vo. pp. 26. N. Y., 1876.
388. †Brown, J. H. B. Jurisp. of Insanity (from the 2d English ed.). 8vo. pp. 713. Phila., 1876.

389. †Naquet. Legal Chemistry (English translation from French). [It has 16 pp. Bibliog. of Toxicology, etc.] 12mo. pp. 175. N. Y., 1876.
390. †Woodman and Tidy. Handy-book of For. Med. and Toxicol. Phila., 1876. (In press, August, 1876.)

Index to Subjects in the American Bibliography, 201 to 390.

General Alphabetical Index.

N. B.—Under each letter, the authors are recorded in the following national order: Italy, Germany, France, Great Britain, and the United States. Foreign authors, recorded among the American, are designated by a †.

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ADDRESS ON MENTAL HYGIENE.

BY

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THE classical phrase, "*Mens Sana in Corpore Sano*," is a general and true expression of the related condition of the mind and body for the best functions of human life. Health of body has, in all times, been regarded as essential to the best balance and culture of mind, and to the most effective application of its activities to the concerns of the world, whether educational or practical, whether in the realm of Philosophy or in the lower plane of Manual Labor. This is true notwithstanding that there are notable exceptions where great intellectual activity and application have been conjoined with feeble physical structure, and even disease.[1]

Indeed we must start with the proposition that what is now denominated Mental Hygiene, is practically inseparable from Physical Hygiene. It is comparatively a new application of the word hygiene. Dr. Maudsley, in his Gulstonian Lectures for 1870, says: "The time has come when the immediate business which lies before any one who would advance our knowledge of mind, unquestionably is a clear and searching scrutiny of the bodily conditions, of its manifestations in health and disease." Again, "as physicians, we cannot afford to lose sight of the physical aspects of mental states, if we would truly comprehend the nature of mental disease, and learn to treat it with success. The metaphysician may, for purposes of speculation, separate mind from body, and evoke the laws of its operation out of the depths of self-consciousness; but the physician, who has to deal practically with the thoughts, feelings, and conduct of men, who has to do with the mind, not as an abstract entity concerning which he may be content to speculate, but as a force in nature, the operations of which he must patiently observe and anxiously labor to influence, must recognize how entirely the integrity of the mental functions depends on the bodily organization, must acknowledge the essential unity of mind and body."[2]

And the great change which has taken place in the views regarding insanity, within the memory of men now living, transferring it from the domain of mere metaphysics, to the jurisdiction of medical science, as a recognized physical disease, witnesses to the same thing. But notwithstanding this intimate connection of physical with psychological conditions, in the study of mental hygiene, it will not be expected that I should go into the former, or into the field immediately related, of Pre-

[1] Dr. Godman, Dr. Robert Hall, the historian Prescott, and numerous other instances might be cited.

[2] Body and Mind, pp. 1, 108.

ventive Medicine, as that whole subject has been assigned to my learned *confrère*, Dr. Bowditch, who has already explored and expounded it in the most exhaustive and satisfactory manner. My range of thought must, therefore, rather be general, and less limited by professional metes and bounds than if it were a strictly medical or a purely psychological one, and may, therefore, not prove as satisfactory to my professional brethren. I shall also be pardoned, I trust, on this National Centennial, if I refer to our own nation largely in elucidation of principles, and for practical illustrations.

This whole subject was formerly confined within the range of philosophy alone. In the days of ancient civilization, before real science was born, when the oracle "know thyself," had only a subjective metaphysical meaning, men like Plato and Cicero placed all mental hygiene in the delights of literary conversation and philosophizing. The Academic Groves were the resorts of dreamy contemplation of a State and a World that never could be realized. The Tusculan Villa was a refuge from the clamor of Senates and the wrangles of the Forum, where Cicero and his friends sought their *otium cum dignitate*, secure from the jealousies and conspiracies of politics, whether Cæsar or Pompey should triumph. They knew little of the physiological functions of the body, while they indulged in their acute and poetical, and sometimes touching, speculations upon the immortality of the soul; but modern science, in studying and dignifying the visible temple of the immortal spirit, has only confirmed and followed in the track of Christianity, which first promised to the human body an equal dowry of immortality with the human soul. The Resurrection staggered the Stoics and Epicureans; but it is at least the best warrant in the diploma of modern medical science, now perhaps somewhat disposed to spiritualize even matter itself, to endow it with the "promise and potency of every form and quality of life." On the other hand, the expression "mental physiology" has been coined to identify the study of mind with that of body, and some would even attempt to resolve all psychology into physiology.

Mental hygiene may be variously classified, but, as a whole, it embraces all that relates to the development, exercise, and maintenance of mental activity in individuals, communities and nations, and must, therefore, be considered from an individual, social, and national point of view. It involves education, social culture, religion and national life. With the individual, it begins at birth, and takes cognizance of even the constitutional tendencies, under laws of heredity, as well as of all the circumstances of subsequent life. To the individual then, in a general way, it consists in that general and special training which will most perfectly and harmoniously develop the body and the mental faculties for the duties of life. At the outset, therefore, on this point, we are met with the questions, who is the individual? What has he to do as an occupation of life? However we may settle on general principles, the rules for individuals must be special. Practically, we must consider classes and conditions. The occupation and pecuniary condition of parents; their culture, social status, and surroundings; clothing, food, and climate; proximity to schools, churches, and places of amusement; density or sparseness of population; laws, government, etc., must all be considered as among the efficient and constantly modifying conditions of any system of hygiene proposed.

Thus, it will be seen, it is a many sided and all-embracing subject, evidently too vast for one, in an address of an hour, to do more than simply

glance at its main features. Though it may be looked at from various stand-points, as men contemplate life, its duties, objects, and ends, from the work they are doing in the world, the fundamental ideas must be the same, to lead to any practical results. The views which the minister, the lawyer, the teacher, the scholar, the physician, the scientist, and the statesman, may take, will be directed, or colored somewhat, by their field of observation, and their bias of education; but on general principles they must be in accord. Mental hygiene covers all the broad field of human energy, embracing all the professions and every branch of industrial life. It looks after man's moral as well as his intellectual nature, for the two cannot be separated. It enters into his domestic and social conditions, and follows him in his duties as a citizen.

I. It first looks at *human nature*—at man as he is born—his utter helplessness, his passions, his needs, physical and mental, and his social, moral, and spiritual wants, and then must devise a scheme for his development. Is this an easy task? The science that would do this perfectly, must, from the realm of physiology, single out the laws written on his members, and harmonize them, in their development and action, with the laws of the spirit written on the psychological side of his nature, and put these in harmony with the laws of nature around him, in whose realm he dwells, and bring all in obedience and loving accord with the eternal truths of God. Mankind has been working upon this problem ever since Adam first contemplated the tree of knowledge, in the Garden of Eden, and was sent forth to his toil and to his own reflections. It will continue to be a problem, for human nature will not change; the human passions, in all their wide scope, are not to be obliterated under culture, but regulated and controlled. Indeed, the discipline of the passions is, in a large measure, the moral side of this great question. Except for speculative study, there is no separation of the intellectual and moral elements, and the regulation of the passions and their proper development become a part of education.

A fundamental principle, therefore, in mental hygiene, is harmonious intellectual and moral culture, under the recognition of man's essential nature as a spiritual being. This cultivation is imperative, in order that man may be able to recognize not only his own needs, but his relations to others; that he may, while taking care of himself, be able to see that he forms an integral part of a social organization absolutely essential for his own welfare. The machinery for the preservative harmony and elevation of social life, must reach to every class and condition; otherwise, the antagonistic elements would impede, if not prevent, all progress, and engender discord.

Common customs, common schools, and common laws, are the most fundamental, and also the most powerful, equalizing agencies in the great machinery of the politico-social life of a people. Through these means the rich and poor meet on common ground, and acquire a certain degree of unity in physical and mental training, and a certain likeness of character and harmony of thought on the great questions of education, morals, politics, and religion, highly favorable to stability, both social and national. Thus great general principles of government become more readily and universally received, and there are left for difference and discussion only the methods and forms of development and application. Still, these means only partially reach into the home life of individuals, where influences are constantly limiting and modifying development, both physical and mental. Indeed, however distinctly the laws of

physical and mental hygiene may be stated, like fundamental propositions in law and morals, their individual application may be difficult. The circumstances surrounding and controlling, may not only modify, but entirely forbid, their application; and persons, families, and whole communities, do thus drift away from any regulated plan of life, or any reasonable principles of action, as the history of moral epidemics, not only in the past but the present, would abundantly show. Poverty and ignorance, the breeders of vices and crimes, and the enemies of culture, may render all principles nugatory, except as they are enforced by law. And in this direction political economy is a most important element in the study of mental hygiene, in its broad application to the interests of mankind.

Thus we see in hygienic science the ideas which lead to the study of pauperism and crime, and the care of the dependent classes, whether made so by disease or misfortune; to the origination of means for preventing social evils of every character; to the enactment of laws for the regulation of morals by limiting vices, as in licensing prostitution and the sale of intoxicating drinks; laws for the suppression of gambling, and providing punishment for the publication of obscene literature; laws to prevent the spread of contagious and infectious diseases; and also that higher and grander step, laws for compulsory education, thus compelling the elevation of the masses by undermining ignorance and superstition, the prolific sources of human misery and degradation. This is itself a wide field of inquiry; a field where Christianity and moral hygiene, social and medical science, and sanitary police, must join hands with education and law, to lift into social order the victims of evil and hereditary influences which they are powerless in themselves either to avoid or to conquer.

That mental activity is highly favorable to physical health and development, when systematically directed into useful channels, need hardly be argued in this day. Intellectual labor seems, as a rule, to contribute to longevity. It was formerly a sort of proverb, that "one of the rewards of philosophy is long life." The ancient philosophers, especially the Peripatetics, pursued their studies and imparted their instructions largely out of doors, amid the varied objects of nature. The amount of physical exercise taken in the open air was a remarkable feature in the life of antiquity, even among its scholars; and we have among them numerous examples of great longevity in men of intellect. Homer, Pythagoras, Plutarch, Thales, Galen, Xenophon, Carneades, Sophocles, Zeno, Hippocrates, Xenophanes, Democritus, and others, reached the age of ninety and upwards, while the majority of such men passed the limit of threescore and ten.

In the early days of this Republic, with simplicity and plain living, we find the same rule holding good. Chief Justice Marshall and Thomas Jefferson reached eighty-four; Benjamin Franklin and John Jay, eighty-five; James Madison, eighty-seven; and John Adams ninety-one. All these men certainly had not only vast intellectual labors to perform, but great burdens of care, anxiety, and responsibility to carry, and even serious privations to encounter; and we might greatly lengthen this list. Bearing on this point I have recently been informed by Governor Seymour, of New York, who has given great attention to the history and character of the North American Indians, that the more intelligent tribes were not only the dominant and conquering ones, but that they afterwards met the struggle with civilization more successfully than

those giving themselves up wholly to physical exercises; that they also maintained a higher physical and mental standard, and lived to a greater age; that these tribes, this day, number nearly as many as in the early history of this country; that they maintain their language, and largely their simplicity of life, in the midst of civilization.[1]

The ancient gymnasia, as well as the modern signification of the word gymnasium, bear witness to the sense of the importance of combining mental and physical culture. Mental culture is a powerful influence in developing the symmetry of the bodily organism, the tone and expression of the face, the organs of special sense, and the harmony of co-ordinating movements of the whole body. One can see this illustrated in our common schools at any time. The power of attention in the majority of young children, in any community, is not much aroused in ordinary life, and they often look dull and stupid on this account. These children enter school, and the direction of the attention to a few simple exercises, in common, awakens the power of attention; and soon, at the tap of a rule, the sound of a musical note, or the word of the teacher, the whole school instantly responds. Now, under such simple but common and systematic exercises and study, the whole expression of the school changes. The bodily organism soon conforms to the habit of attention and to the systematic mental training, and awkwardness and dulness are soon transformed into gracefulness, courtesy, and intelligence.

[1] Extract from Appendix to Address by Gov. Horatio Seymour, of New York, at the dedication of the Kirkland Monument, Clinton, N. Y., June 25, 1873.—The superiority of the Indians of New York, over those of adjoining States, is proved, not only by contemporaneous history, but by striking facts within our own observation. Their pride, heroism, and victories, through a long series of years, affected not only their mental and moral characters, but even their physical organizations. . . . . Beyond the evidences of their superiority to be found in history and science, we have living proofs of the vitality and vigor of the Iroquois. All others of the Indians, who once lived in the States lying east of the Mississippi, have been swept away, except a few who linger in the wild regions south of Lake Superior. None of them have withstood the power and influence of the whites, except the Iroquois. The Mohawks went to Canada, during the Revolutionary War, and most of the Oneidas removed to Wisconsin about forty years since. The other tribes still live in New York. In all their homes they are surrounded by the whites, and by high civilization, yet their numbers do not diminish at this time. For a while after the whites went among them, they fell off about one-third in population, but they now hold their number, with a slight increase in some cases. But the strength of their character is more strikingly shown by another fact. Although the Indians of New York, for three generations, have lived in the centres of civilization surrounded by a dense white population, with whom they are in constant contact, as their reservations are small, yet they retain their own language, their own customs, and about one-half hold their old religious faith; yet they have white teachers and preachers who live among them. In sermons or addresses, they must be spoken to in their own tongue, or through interpreters. At the celebration of the Kirkland Monument, a deputation of Oneidas was present. They belonged to a small remnant of their tribe, numbering less than one hundred and fifty, who did not go with their people to Wisconsin. They and their fathers and grandfathers always lived in the heart of New York, in the vicinity of large cities and villages. The tract of land they own contains but a few hundred acres, yet those present at Clinton, who were well-dressed men and women, could only speak to the assembly through an interpreter. In private conversation, with a few exceptions, they speak the English with hesitancy, as their thoughts are all conceived in the Indian language. The whole world is sending representatives of every lineage, language, and nationality to our country: all of these in a few years speak our tongue and adopt our customs, and in a little time are assimilated in all respects with our people. Even the most stubborn races of Asia yield to our phases of civilization. There is not in ethnology a more extraordinary fact than the resistance for more than a hundred years of our influences by this little band of natives. The continued existence of the Iroquois, while their kindred tribes have been swept away, and their resistance to our language and mode of thought, while all other lineages in our land have been assimilated, give proof of the vigor and marked peculiarities of their race.

Another common illustration is the change we mark in servants under the training of intelligent masters; under simple example and the stimulation of mind in the direction of systematic attention to duties, how quickly they are transformed, if they have any reasonable degree of capacity. The same may also be said of soldiers. Hume, in his Essay on National Character, says: "The human mind is of a very imitative nature; nor is it possible for any set of men to converse often together without acquiring a similitude of manners, and communicating to each other their vices as well as their virtues."

The great power of mental activity and attention, in modifying expression, bodily habits and movements, as well as general manners, may not only be seen in individuals and schools, but in whole communities and even nations. This is far more than imitation, it is substantial individual culture, the development of all the faculties in more or less symmetry. If we were asked the secret of the physical prowess and conquering power of the Roman people for a thousand years, we should answer it was the military education and discipline of the *whole* population from seventeen years of age, with its stern system of self-restraint and self-regulation. It was a civilization based upon the rigorous principles of the Lacedæmonians, rather than the lighter and more artistic life of Athens. The modern meaning of the word *virtue* was the *cause* of its Latin meaning, as confined to the behavior of men in battle, a stern temperance and self-control behind unflinching courage and endurance. It was once said by an American orator[1] that Rome was thrice mistress of the world, by her arms, her religion, and her law. It is in the last only that she retains supremacy, for there is hardly a civilized nation in which the maxims of Roman civil law do not form the basis of equity. And this is all the result of the primitive training of that great people both physical and mental. The same principle was illustrated in the Puritan Cromwell's troops, the soldiers of William the Conqueror, the German armies of Frederick William in the Franco-Prussian War, and conspicuously in the Revolutionary patriots of America and in their descendants, the soldiers on both sides of the contest in the recent war of Rebellion, in this country.

The more we examine into this question of mental hygiene, the more it seems to resolve itself, on the mental side, into a statement of the best methods of education and training to secure the highest and truest culture. And this is the line of thought which forces itself upon the mind as the true exposition of the words mental hygiene—a system of culture embracing all the interests of man in all his relations of life; education in its highest expression and broadest application; education to secure not simply a knowledge of man, and of nature and her laws, and the awakening of the faculties to a deep obedience which will make man reverence *her* in all *her* works and ways, or, in the beautiful language of Prof. Huxley, "the instruction of the intellect in the laws of nature, under which name I include not merely things and their forces, but men and their ways, and the fashioning of the affections and the will into an earnest and loving desire to move in harmony with these laws," adding "for me education means neither more nor less than this"—clear emphatic words of which no one can mistake the meaning: but far more than this, we should demand a broad and deep culture of man which would do all this, and which would also awaken in the soul a full conscious-

[1] Hon. Hugh S. Legaré, of South Carolina; N. Y. Review, 1841.

ness of its responsibility to One by whom all things exist; an education which would not only raise man to harmony with the laws of nature, but which would also raise him to obedience to the laws of God; which would make his life real, earnest, pure, and useful. To accomplish this, mental hygiene must include, therefore, not simply the mental and moral training in a general but also in a particular way; must as well include social restraints and duties, as I have heretofore indicated, and embrace a supreme regard for the welfare of the country—a true patriotism.

It is not out of place in this centenary year of the nation, to remind ourselves that this element of moral and religious restraint and discipline, this spirit of subjection to an overruling power above nature, was not left out of the practical life and training of its original founders. The first settlers of this continent began their conflict with wild nature, and still wilder savages, under a system of self-discipline and sense of religious responsibility, sterner even than that of the old Romans. It was indeed their *virtue* that brought them through successfully in their contest with the mightiest empire of Europe, and enabled them to lay the foundations of a governmental fabric which has been the astonishment and the study of European statesmen.

But to follow up the subject of mental hygiene in relation to individual life, it must be evident that I should have to enter into details not possible in such an address as this; I should have to take into consideration, not only the whole scope of what I have denominated, in a general way, educational means and influences, including domestic life, but also age, sex, civil condition, heredity, passions, the influence of climate, social customs, religion, etc., in all their varied relations as found in action in individuals, as also the questions of food, water, exercise, air, sleep, etc., all essential conditions and vital questions in the study of individual hygiene.

II. When we come to the question of mental hygiene in communities, we include all hygiene as to individual life, and at the same time enter the wide domain of sociology or social science. Here again we meet the great problems of education, social customs and laws, intermarriage, amusements, and indeed all the conditions of social and civil life, together with religious culture, which, though I have named it last, is really first and fundamental. In this age of books and thinking, no man can ignore this latter point if he would. While I have never been able to see any conflict between Science and Religion, certainly the tendencies of the times are rather to question closely their relations and their respective domains. Christianity has been challenged by science, in some quarters, and the old questions of faith, freewill, responsibility, necessity, etc., are again discussed in the light of increased physical knowledge and physiological investigations, and the more advanced views of psychology.

The literature of the age itself illustrates the subject of mental hygiene. Bulwer, in his "Caxtons," gives an admirable chapter on the hygiene of books. He suggests that reading should be governed by the mental state; that it should be suited to the morbid drift of thought, or the malady of the individual. As an illustration, he speaks of the folly of attempting to amuse a man in the midst of a great sorrow; and nothing could be more in accord with sound psychology. Referring to Goethe taking up the study of a science, after the death of his son, he says, "Ah! Goethe was a physician who knew what he was about. In a great grief like that you cannot tickle and divert the mind." He re-

commends for the sorrows of middle life and old age, "bringing the brain to act upon the heart." He would thus have philosophy lead and temper the emotions and the will, and enrich the true life by contemplating the lessons of experience. He says: "For that vice of the mind, which I will call sectarianism, not in the religious sense of the word, but little narrow prejudices that make you hate your next-door neighbor because he has his eggs roasted, when you have yours boiled, and gossiping and prying into people's affairs, and backbiting, and thinking heaven and earth are coming together if some broom touch a cobweb that you have let grow over the window-sill of your brains—what like a large and generous, mildly aperient dose of history! How it clears away all the fumes of the head . . . how your mind enlarges beyond that little feverish animosity to John Styles!" Here we have a strong hint, not only against the cultivation of a narrow range of thought, but also of the great value of that wide mental scope which takes in the interests of others, and occupies the mind in public and social interests and affairs of State.

Again he says: "I remember to have cured a disconsolate widower, who obstinately refused every medicament, by a strict course of geology. I dipped him deep into gneiss and mica-schist. Amidst the first strata, I suffered the watery action to expend itself upon cooling crystallized masses; and by the time I had got him into the tertiary period, amongst the transition chalks of Maestricht and the conchiferous marls of Gosan, he was ready for a new wife." But all men and women cannot grasp geology. No, but all can find, in morbid states, some serious mental occupation, if it be only their own sphere of labor, or some useful course of reading which will occupy them *out of themselves;* this is the secret, *out of themselves.* This would be substituting action for mere sentiment, a most important principle in mental culture. No lesson is more important than this, and it gives emphasis to what all experience teaches, that in the substantial realities of life are to be found the true sources of healthful mental discipline and growth; that while amusements are useful as recreation *after* toil and responsibility, they are not to be looked upon as the daily pabulum of the individual or of society; that, in excess, amusements become a kind of dissipation which creates a distaste for systematic and useful labor, and for the quiet of home life and its practical duties, and for all rational social intercourse. Thus real life grows tame and insipid; a constant, restless desire for excitement is substituted for the deep, solid stimulus of duty, of domestic responsibility, and of that substantial mental culture which alone can give to society strength and virtue.

Sentimentalism is a tendency of the age, and has a most important place, but it should not become so dominant in social life and in education as to exhaust the mental energy in trivialities, or in mere expression of the feelings, instead of leading it to action and duty. Mere sentimentalism, whether in social life, religion, or politics, demoralizes and emasculates all life and action; it is but a sensual indulgence at the expense of all vigor and energy in the practical advancement of the individual or of society. What the school of sentiment, as headed by Rousseau, did for France—the extravagances, the follies, and the fanaticisms to which it led—is patent enough from French history since the Revolution of 1789. By sentimentalism is here meant that exaltation of the feelings and sympathies of the mind which is always expending itself upon unworthy objects or causes, or in the mere contemplation of those

that are worthy; which Butler, in his Analogy, has pointed out with great psychological penetration, as mere emotional impulse, with no practical object, and no ontlet for reasonable action, and which is, therefore, always enervating and destructive to mental character.

This tendency is not found simply in the immense indulgence in novel-reading in these days; in the graphic and picturesque portrayal of crimes and social vices; in newspaper serials, which flood the country, and which are brought before people at every book-stand and in all the avenues of travel; in the publication, broadcast, of the minute details of crimes, suicides, and court trials, where sickly sentiment, and social vices, and the passions of mankind, become the staple and the snm; but this tendency is also seen in science and in much quasi-religious teaching, as well as in a multitude of so-called social and humanitarian schemes which promise to lift man into a realm of thought and feeling where he will necessarily, as by a law of his being, drift to the good. This spirit of sentimentalism and restless love of novelty is only a form of scepticism, and drifts into sensualism in morals, into useless and vague speculations in science, and into transcendentalism and infidelity in religion, and in all its tendencies leads to morbid, unhealthful, and impracticable mental action.

Carlyle has said that "the proper task of literature lies in the domain of Belief, within which, poetic fiction, as it is charitably named, will have to take a quite new figure, if allowed a settlement there," and that "the exceeding great multitude of novel writers and such like, must do one of two things, either retire into nurseries, and work for children, minors, and semi-fatuous persons, or sweep their whole novel-fabric into the dust cart, and betake them with such faculty as they have, to understand and record what is true, of which there is, and ever will be, a whole infinitude unknown to us;" and he adds, "poetry will more and more come to be understood as nothing but higher knowledge, and the only genuine Romance for grown persons, Reality."

Bulwer, with a sort of prophetic fore-glimpse over the field of these developing tendencies, speaks in the chapter already mentioned, of "curing a young scholar, at Cambridge, who was meant for the Church, when he suddenly caught a cold fit of free-thinking, with great shiverings, from wading out of his depth in Spinosa." He failed when he tried him on the divines, but succeeded by dosing him first with chapters of faith, in Abraham Tucker's book; then strong doses of Fichte; after these the Scotch metaphysicians; ending up with a plunge bath into certain German trancendentalists. He adds, "having convinced him that faith is not an unphilosophical state of mind, and that he might believe without compromising his understanding, for he was mightily conceited on that score, I threw in my divines, which he was now fit to digest, and his theological constitution since then has become so robust that he has eaten up two livings and a deanery." Now "wading out of his depth in Spinosa" is admirable. These youths are now grown quite numerous, and count among them some of the ablest scientists, who seem to be carried away by "winds" or by "doctrine" into what St. Paul styled, in his day, "science falsely so-called." They drift so far out as to get into what they call the "unknowable" and the "unthinkable," which phrases, if the veil of time was removed, would disclose underneath the inscription of the Athenian philosophers on a certain altar, noticed by St. Paul, as he entered that learned city eighteen hundred years ago.

If we look through the history of such mental drifting, we cannot but recognize it as productive of vast evil. Certainly such speculations have, as yet, made no scholar wiser or better, judging by what they have imparted to the world on the subjects of Christianity or Revelation. To be sure, we are aware that doubt has been even dignified as an essential condition of mind for the highest perception of Truth! Now the mental hygiene suggested by Bulwer was as admirable as his diagnosis. He did not strand his patient, at the start, by discussion and dialectics, but led him by a line of thought, natural to the patient, through the regions of apparent contradictions and doubt, according to his powers of mental digestion, and finally cultured him to the full truth, and at length made a Dean of him, where he had the widest scope for faith and works, as well as a field for mental action in the highest range of human duty.

A most notable instance of this "wading out" is the case of a late President of the British Association. A brilliant man, with rare powers of analysis of physical facts and phenomena, his mind trained in this direction, he launches out on the track of the great spiritual ocean, to which he applies his physical tests, and fails. He is followed instantly by another brilliant mind in physical science, who takes down his system of astronomy to follow the wake of a new leader. These are conspicuous cases, and in other times the former might have become the founder of a sect; but instead of that, his structure falls to pieces as he contemplates it. Besides, he confesses that he has *moods*, which is equivalent to unsoundness, when moods are confessed in explanation of statements and opinions. This is encouraging, for it shows that the great and constantly increasing body of truth, extended in all directions, is not only unmoved by scepticism, but is gradually undermining it by explaining the phenomena on which it rests for the title of its existence, and is also revealing principles so much mightier and broader than man's theories of Nature and of God, that Scepticism dies in the light of Truth.

Newton, after reaching far into the arcana of nature, and finding his vast knowledge so little of the whole that he compared it to a few pebbles on the shore of the ocean, is the modest but grand symbol of true progress. He illustrates the majesty of science, and her respectful homage at the feet of the Creator. That science which is "puffed up," and which stands ready to unveil the sanctuary, and to enter into the holy of holies and reveal the Almighty to man by chemistry and telescope, and thus find out the secret of his creation and existence, and discover the government of the moral world in a series of correlated forces, is not the science of religion which can captivate the world of to-day. However man may move in a cycle of the same truths and deceptions, in kaleidoscopic variation from age to age, God is unchangeable, and has declared Himself to be "past finding out."

So principles are eternal, though our methods change. The ideas of God, in ancient philosophy, which Pythagoras and Socrates, Plato and Cicero, saw dimly through the body of science and truth then developed, cannot, at this day, be accepted as religion, though their faith and loyalty to an invisible Creative power stand unquestioned, and like a rock in the desert of centuries. Though Plato is perhaps the grandest figure standing forth in the history of human mind, his theories are but as the dawning light to the mid-day sun, in the abstract truths, developed science, and revelation of to-day. Much less can we accept Democritus and his followers. When Horace sang of the "*Integer vitæ, scelerisque purus*," he but struck a glimpse of the same truth which belongs to the

province of religious culture, and which had long before been uttered by the Royal Psalmist, "Blessed is the man who hath not walked in the counsel of the ungodly, nor stood in the way of sinners, nor sat in the seat of the scornful, but his delight is in the law of the Lord." The psalm of Horace and the psalm of David are alike devout confessions of the blessedness of purity, and are tributes to the importance of moral culture as an essential to the full development of mind and mental balance.

Indeed, without this moral culture, we may say, it is well nigh impossible to understand even Nature herself. Certainly this is so, as far as the ideas of purpose and design are concerned. As Baring Gould has well put it, the world is a visible exhibition of the ideas of God, a mighty book to be read. But who is to spell out this Created speech and comprehend its significance? Those who are to catch and understand the ideas of God, must have a spiritual nature capable of perceiving such truths. "Therefore, he who is to read Creation, must be neither mere spirit nor mere body, but must have a spiritual nature combined with a corporeal nature, so that, through the things revealed to the mind by the bodily senses, the thoughts of God may be perceived." Mental hygiene or culture, from this standpoint, recognizes the essential nature of man as a spiritual being, and points to the necessity of educating his moral nature in harmony with his intellectual, to bring forth the full man.

In this view the very wastes and solitudes of nature come to have their moral and spiritual uses. In a burst of enthusiasm, Baring Gould exclaims: "The time of Alpine snow has come; age after age has seen it powdered on the mountain peaks, slide down the flanks in ice, and flow away in rivers to the sea, unesteemed save for the water it yielded. But its time has come, its value is known. There is no medicine to a weary brain, like the golden light on a distant bank of Alpine snow. . . . I remember a mountain scramble, leading me suddenly, from rough rocks and sear grass, upon a dell of rich greensward girt about with pines. Set in the turf was here and there a fallen star—a yellow anemone; on the rocks the carmine Alpine rhododendron was in full blaze of blossom, and over all the sward was a tender bloom of forget-me-not. Over head *burnt* a glacier in the summer sun, and a thread of silver fell in powder from it, waving in the soft air. I am not ashamed to tell you that that vision filled my heart to overflowing. God spake through that scene, through every flower, out of the mountain, out of the ice. The voice of God, walking in that garden, was as audible as of old in Paradise, when Adam heard it in the cool of the day."

No reflecting man can fail to see that the attention given to education, all over the world, is a most significant fact in the history of this period. That education, in some degree, shall be universal, seems to be a common sentiment. That this is essential to the progress of civilization, needs only to be stated. The great questions that arise are, what shall education be, and what shall it include? Shall it include religious instruction in all fundamental training? is the real problem, however the question may be stated. And reaching higher in the scale, the same great question intrudes itself only in a different form. The scientific theory of culture, set forth by the school of which Professors Huxley and Tyndall are exponents, as Principal Shairp has so well pointed out, gives little account of and makes no provision for the moral elements of human nature, and this would seem to be its deep defect. As he truly says, "the knowledge of the highest things, those which most deeply concern

us, is not attained by mere intellect, but by the harmonious action of understanding, imagination, feeling, conscience, will—that is, of the whole man—reason in its highest exercise, intelligence raised to its highest power." And this for the single reason that no science can call our *whole* nature into play at once. This can only be done by religion, which alone calls upon the whole of man. When Prof. Huxley presents life as a game of chess, with an invisible player, he endeavors to save the idea that a man *must* respect the rights of others; but there is no more room for such an idea in his scheme than there is in a shipwreck, or in Darwin's doctrine of the survival of the fittest. It is *not* a *natural* impulse of man to respect the rights of others, or "to love one's neighbor as one's self;" as Principal Shairp has well said, it requires the whole weight of Christian motive to do either.

III. When we come to examine mental hygiene from a national point of view, we see that it comprises all that gives intelligence, character, dignity, progress, and stability to national life. In this greater field the lesser are included. That hygiene which tends to elevate a people, both mentally and physically, by a true and rational culture, is in fact mental, moral, and physical training, resting on definite principles, and these so accepted as to become the prevailing and growing habit of the people; or, in other words, culture extending itself into national habits, thoughts, and pursuits. Whatever theories we may adopt as to the equality of man, the best practical result of national training must be to give to each class of minds that bias which will serve to develop useful individual tendencies, and at the same time, in the main, correspond with its social status. For whatever general education or fundamental culture we may rest upon as a prescribed system for general application, the professional man, the merchant and business man, the mechanic, the farmer, the clerk, and the laboring man, need different training to fit them for practical life. The question of mental hygiene is therefore not simply how we may best train men so as to cultivate mental health and physical vigor, but also how we are to bring about the use and application of all the principles and agencies which are best adapted to develop, expand, and maintain in balance the mental and spiritual life of individuals, communities, and nations, so as to insure progress in civilization, a healthy state of general and domestic morals, and, at the same time, the advancement of culture, arts, and industries.

It is only within a comparatively short period, that the study of general and mental hygiene has been demanded, under the progress of science. This study has been stimulated by the developing necessities of civilization, the greater attention to sociology, and the progressive ideas of personal liberty and responsibility. All the ancient civilizations were but little concerned in the welfare of the individual, as a unit going to make up national life. Knowledge and the exercise of governmental functions were confined to the few. The great masses were only so much brute force, or mere physical elements, in the hands of rulers and leaders, to carry out their own ends; and to all this the masses gave almost absolute assent. The lives of the people were held cheap by the rulers, as well as by the people themselves. Even the great revolutions, from time to time, were not movements of the people, but were simply brought about by kindred though antagonistic governing families, and the people were used in their respective interests, being aroused under the temporary stimulation of the passions of the hour. They had no appreciation of the ultimate tendencies of public movements, and no hope or desire

for personal elevation or advancement. In such a state of national life, mental culture could have no wide significance, and no place except among the learned, as a mere subject of contemplation. Therefore for many ages we have little on the subject of mental hygiene, in any direction, beyond the philosophic declarations and fables of learned men. The priestly orders, indeed, in ancient times combined and exercised largely the professions of minister, teacher, and physician, and were also the trusted advisers of kings. The Mosaic law laid down the rules of hygiene, as it did those of morals, intermarriage, and worship, in a single code.

The aphorisms of Hippocrates embrace about all that is valuable in medical literature of the pre-Christian centuries. The Code of Health of the School of Salernum, for hundreds of years a medical classic, contains the recorded knowledge on the subject of health down to the sixteenth century. A recent translator, Prof. John Ordronaux, says of it, "it was for ages the Medical Bible of all Western Europe, and held undisputed sway over the teachings of its schools, next to the writings of Hippocrates and Galen." It contains a great many precepts and dietetic rules, but deals little with mental hygiene; and this is the sum:—

"Salerno's School, in conclave high, unites
To counsel England's King, and thus indites:

If thou to health and vigor wouldst attain,
Shun weighty cares—all anger deem profane,
From heavy suppers and much wine abstain.
Nor trivial count it, after pompous fare,
To rise from table and to take the air.
Shun idle, noon-day slumber, nor delay
The urgent calls of Nature to obey.
These rules, if thou wilt follow to the end,
Thy life to greater length thou mayest extend.

Shouldst Doctors need? be this in Doctors' stead—
Rest, cheerfulness, and table thinly-spread."

The author closes with a valedictory which would indicate that he felt he had accomplished a great work:—

"The Flower of Physic endeth here its strain;
The Author, happy o'er his garnered grain,
Prays that in Heaven there be prepared for him
A seat near Christ, and His blest Seraphim.
Amen!"

We must not forget, however, the great services rendered, during the dark ages, to Science and Literature by the Monks and Religieux while buried in their cloisters, as well as their work in preserving the treasures of learning from the all-surrounding devastation. It is admitted by all historians that, in days of violence and anarchy, the Church was a defence and refuge of the poor and the oppressed, as well as of the learned, against the hand of tyranny and rapacity. To the Benedictine orders, at least in their constant and systematic attention to the cultivation of the soil, and the implements and improvements of agriculture, we owe a great lesson of that primitive truth, that in the sweat of man's face shall he eat bread. It may even be said that the motto of this order, *laborare est orare*, has become the watchword of modern civilization, for in no period of the world has labor been so dignified as in these times, and the machinery of labor so multiplied for the uses of man.

Mental hygiene, from a national point of view, would also cultivate in the people a harmonious and universal aim towards elevated and yet practical ideas. A national sentiment, fostered and dignified by government, in favor of education, mechanics, agriculture, arts, becomes a most powerful mental stimulant to individual effort, and seems to give breadth, tone, and vigor to national mind and character. No people, perhaps, ever gave more earnest and practical attention to educational power and the value of morality, as elements in government, than the Puritan stock of New England. From the first, the church and the school-house rose side by side, and whatever economy and frugality they exercised in affairs, and they were marvels in these virtues, they never stinted the head and the heart. They had lofty ideals, and they practised stern virtues, and when national oppression came they had stout hearts, willing hands, and clear heads, to offer in the struggle for liberty and the founding of a nation. The sentiments of Union and Liberty, early and deeply rooted in the mental soil of the early inhabitants of the American Republic, have propagated their influences and spread their roots and fibres through the blood-soil of children's children, and we see the result in millions of active intelligent minds, carrying forward with united and persistent purpose the vast interests of this great nation, and subduing this mighty continent, in its multiplied physical resources, to the utilities of mankind, as though governed by a single national impulse.

The founders of the Republic, in every part of the land, seemed to have been thoroughly permeated with the spirit of personal and public duty. With them, Liberty meant Law and obedience to principles of Justice—an obedience, as beautifully expressed by Ruskin, "chastisement of the passions, discipline of the intellect, subjection of the will, fear of inflicting and shame of committing a wrong; respect for all who are in authority, consideration for all who are in dependence; veneration for the good, mercy to the evil, sympathy with the weak; watchfulness over all thoughts, temperance in all pleasures, and perseverance in all toils."

Had our ancestors cultivated the softer graces, and given themselves up to games, sports, and ease of life, and the government contented itself with hereditary dignities, leaving the mass of the people in ignorance, and to think and act only in the narrow sphere of providing daily bread by daily toil, how different would have been the mental status of this nation to-day! If, on the other hand, the inhabitants of this country should ever become so demoralized and degraded as to find their contentment, like the people in the latter ages of Rome, in material comforts alone—*panem et circenses*—mere bread and amusements—then, too, like the later Romans, they would soon become the prey of family feuds and contending factions, ending in the despotism of a swift succession of flagitious rulers, till the whole political system would sink into final disintegration and ruin.

This Centennial, a part of the movement of which this International Medical Congress represents, is a great national thought, and a most powerful influence in stimulating national mind, as well as individual, in the direction of healthful mental activity. Indeed, it is itself a vast and far-reaching means of culture, which touches a responsive chord in nationalties of the most diverse social and political character, but nevertheless in harmony in the one direction of progress. To impress men by such magnificent displays of wealth, mechanism, and art; to show them

that life is more than meat and drink; that a nation is great and powerful in proportion as its citizens are cultured to refinement, utility, morality, and personal responsibility; that these constitute the foundation stones of national greatness and prosperity, is in itself a great national hygienic measure. As the accumulation of patrimonial treasures, learning, and office, give dignity to families, and stimulate to higher culture, so the accumulation of treasures of all kinds, and the recognition and cultivation of art and learning by nations, tend to dignify national reputation, and to stimulate citizens of all classes to higher efforts and more patriotic lives.

Thus a nation secures mental and moral growth and breadth of enterprise. No one can look at the wonderful Exposition, now held in this city, without realizing this fact. The world seems, indeed, to be here assembled. The Egyptian, the oldest civilization, stands before us today as it stood in the days of the Pharaohs. The march of progress and the attrition of nations may have modified her national life in some outward things, but the central ideas remain the same; her escutcheon is unchanged, and she sends to this Centennial, as an essential treasure, the head of Rameses, thus typifying her original and perpetual dignity, unbroken through the long tide and flow of centuries. And so down through the roll-call of nations, to our own, each has its own grand representative idea. At the end of a hundred years we stand at the statue of Washington, and relate his virtues, as embodying the central ideas out of which grew, and on which rests, the deep, broad, and sure foundation of this Republic. Egypt may come to us; she may take our ploughs and reapers, our engines and printing presses; but she will only enthrone Washington when she accepts our ideas.

The true greatness and dignity of any nation will always be measured by the standard of its mental and moral culture, not simply by the intellectual standard it presents in its military power, its science and arts, and its dynamic forces, but also by its will and capacity for morally elevating its citizens, without clash of caste; maintaining universal freedom, with all men equal before the law. The present Emperor of Russia, realizing such a sentiment as essential to the dignity, prosperity, and permanence of the government, transformed, in a day, millions of serfs into freemen, the grandest ukase in the history of time. This is the substance of Magna Charta, the glory of England. This was the ostensible aim of Cæsar. His memory is quite as much dependent on his philosophic culture, and his assimilation to the people, as on his conquests. He shed lustre on Rome, and on mankind, by his amazing combination of simplicity, learning and statesmanship, with the greatest capabilities of a soldier, all of which he illustrated in the midst of a galaxy of the most magnificent minds in the annals of the world. His name fitly represents the power of mental culture in the direction of definite ideas, in a ruler looking to the elevation of a people as the true source of national power.

Rome lost in prestige when she accustomed her people to ideas of conquest and personal ease, above moral culture, in its wide meaning. Indeed, no nation has ever maintained permanent elevation and power, which has encouraged or permitted public opinion to act outside of the pre-ordained boundaries of religious truth. The belief in a God must be the corner-stone on which a nation rests. Both Greece and Rome flourished in power, arts, and arms, so long as they clung to a belief in a supreme Providence, above Nature. But when the speculative

philosophy of Epicurus and Lucretius, with its absorbing sensualism, usurped the ancient worship, they perished beneath the blight of a cold scepticism. The glory of Egypt was clouded with Cleopatra, who represented deified sensuality on the throne of a Nation. The great Assyrian Empire, in like manner, fell under Sardanapalus, the gilded monarch of Asiatic licentiousness. France well nigh perished when she installed the Goddess of Reason in the seat of Worship, and her Chief Assembly voted Death to be an Eternal Sleep.

Under the cultivation of the ideas, and the practice of the principles, to which I have referred, by the founders of this Republic, we have the national fruit, not only in a great and well-established nation, but conspicuously in the wonderful development of the resources of high civilization all over this continent. It is a truth, well worthy to bear in mind always, that education, with them, embraced ideas of religious freedom, which were cultivated together, no matter what the calling in life. And it is not too much to say that in the rigid spirit of utility and the high sense of responsibility of the early fathers, we have the seed from which has germinated, over this broad land, the personal independence of character, the inventive genius, the subjection to law, and the matchless energy, which have made us equal in power to the older nations of the world, which have also given us an individual national character—stamped us as Americans—notwithstanding that we represent all the nationalities of Europe; which have developed a national mental hygiene which reduces and conforms the cosmopolitan ideas of the vast and constant drift to our shores, to the national standard, which prevents anything antagonistic to the fundamental principles of the government from taking root, and which assimilates and harmonizes all the seeming antagonisms to the genius and spirit of the constitution, the moment they are subjected to its dominating idea, "government of the people, by the people, for the people."

I might properly allude to the great influence exerted on the national mind by such men as Franklin, Rush, William Penn, Robert Morris, Richard Henry Lee, John Jay, the Adamses, Hamilton, Jefferson, Madison, and others, if there were time; but such influence, however special and potent, was, after all, only the projection and happy presentation of principles which, when thoroughly impressed, acted on the mind of the people in moulding national thought. The work of these men was done through the reason and judgment, and not by popular display and glamour, and it was abiding. Economics and population, education, statesmanship, finance, constitutional law, Political Economy in all its wide bearings, received from them the most earnest and profound discussion. However great their attention to religion, they did not confound it with state morals; but, on the one hand, they maintained Christianity and the highest responsibility to God, and, on the other, they sought to work out, under laws, the mutual rights and relations of men under their new social and political conditions of government.

Their lives were illustrations of the principles they advocated; William Penn exercised a wide and mighty influence in securing not only the mutual co-operation of savages, but also of classes of men bred in other traditions, and in bringing all to the formation of national habits and character. Yet he represented no victor with temporary plaudits, no sensational or dramatic phase of social life or regeneration. His power illustrates what education, aided by elevation of character and equi-

librium of the intellectual life and passions, may do in a man who is controlled by truth and directed by spiritual light. Wilberforce, in England, illustrated the same great influence on national mind. He showed how a strong mind, panoplied in its convictions of universal justice, might gradually undermine historic precedents, against all the forces of conservatism arrayed in opposition, as well as against the apparent interests of the nation. Slavery was then a part of the national wealth, but it died through the influence of this one peaceful mind, breathing condemnation upon it, and this in the very presence of those whose material relations to it were of the closest character. Thus a whole nation was transformed by a mental revolution, wrought solely in the name of universal philanthropy, justice, freedom, and religion.

Such are some of the higher triumphs of national culture, when it embraces the moral and spiritual elements of Christianity. Though this age may be characterized as one of liberal tendencies of thought, in all directions, it has been permeated by the principles of Christianity, and to-day there is more respect for religious truth, and a firmer belief in the necessity, for both man and nations, of faith in a God, than when the century commenced. When, in the recent French Revolution, the Archbishop of Paris, Monsiegneur Darboy, was struck down by the Commune, the nation turned from the act with horror. Yet the Bishop was only a man, and one among the hundreds of noble men who thus perished. But he represented Religion, and millions of people, alike Protestant and Catholic, condemned the deed as one of infamy, and as a diabolical defiance of the very instincts of humanity, as well as of the traditional sentiment of Christendom.

The lesson of mental hygiene, for nations, which we learn from all example, is, not that education and wealth, nor the refining influences of æsthetic art, will suffice for the highest development of national mind, but that, if underneath and through all these are not interwoven the great truths of moral responsibility to the author and upholder of all governments, lifting man above the dominion of the baser passions, the nation dies as an individual dies; for "unless the Lord build the house, they labor in vain who build it."

In the Convention at Philadelphia, in 1787, for forming a constitution for the United States, after some weeks had passed in fruitless debate, a proposition having been made for daily prayers, Dr. Franklin rose and said: "In the beginning of the contest with Britain, when we were sensible of danger, we had daily prayers in this room for Divine protection. Our prayers were heard and graciously answered. All of us who were engaged in the struggle must have observed frequent instances of a superintending Providence in our favor. To that kind Providence we owe this happy opportunity of consulting in peace on the means of establishing our future national felicity. And have we forgotten this powerful friend; or do we no longer need His assistance? I have lived a long time, and the longer I live the more convincing proof I see of this truth, that God governs in the affairs of men. And if a sparrow cannot fall to the ground without His notice, is it probable that an empire can rise without His aid? We have been assured in the sacred writings that, except the Lord build the house, they labor in vain that build it. I firmly believe this, and I also believe that without His concurring aid we shall succeed no better, in this political building, than did the builders of Babel."[1] The motion was carried.

[1] Debates on the Constitution.

This illustrates the sentiment and temper of those who founded this nation, and may we not say, standing where we do, that the influence of this illustrious example has had some share in determining the tone and the practice, in that respect, of this renowned University from its foundation, whose successive Provosts have been eminent examples of the essential harmony between the different qualities of Faith and Science? These latter thoughts have come into my mind since entering this hall, while looking round upon the long line of Reverend Provosts speaking out from the canvas, and then reading over the door of entrance the grand inscription, "In Honorem Dei." An institution, like a State, which writes over its portals, "in honor of God," cannot fail of success and power, before the people, as more than a century has here demonstrated. And this is my Alma Mater.

For individuals and communities, the quaint lines of George Herbert, with which I close this Address, are a suggestive and pregnant summary:—

Slight those who say amidst their sickly healths,
Thou livest by rule. What doth not so but man?
Houses are built by rule, and Commonwealths.
Entice the trusty sun, if that you can,
From his ecliptic line; beckon the sky.
Who lives by rule then, keeps good company.

Who keeps no guard upon himself, is slack,
And rots to nothing at the next great thaw.
Man is a shop of rules, a well-trussed pack,
Whose every parcel underwrites a law.
Lose not thyself, nor give thy humors way;
God gave them to thee under lock and key.

ADDRESS ON AMERICAN MEDICAL LITERATURE.

BY

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I HAVE attempted in this Address to give some account of the books which have been written by the physicians of the new world, since the settlement of America by white men. The history embraces a period of nearly two hundred years, and includes the names of as many authors, and the titles of more than as many publications. Restricted as I am, both in space and time, my notices are necessarily brief. As to most of them, they are nothing more, indeed, than a catalogue of names and titles. But it occurred to me that I could not in any other way so well meet the wants of the profession in an address like this, as by presenting a full list of our medical works in the order in which they have appeared, so that we may see, and that those who shall come after us may learn, what the medical men of America have done in the first century of our national existence. My references are confined almost exclusively to books, but occasionally I have noticed pamphlets and articles in Transactions of Societies, which appeared to me to possess unusual value. To do justice to the medical literature of America, would require something more than a single discourse. All that is proposed in this, is to give an outline of the subject; and, with all the pains I have taken to make my list complete, I can hardly hope that it will not be found chargeable with many omissions.

The Literature of Medicine had acquired but little extension, and was still expanding slowly in Great Britain, when the first emigrants for America left her shores. Harvey was just then announcing his grand discovery of the circulation of the blood. Sydenham had not been born, and it was half a century afterward that his works, conceived in the true spirit of the Baconian philosophy, and the standing glory of English medical literature, made their appearance to put a new countenance upon the Science of Medicine. Mead and Cheselden were born more than half a century afterward. The writings of the Hunters, and of Cullen, appeared only as the Colonies were ready to set up in the world for themselves; and the discoveries in chemistry which imparted to it the character of a science, were made by Black and Priestley on the eve of our great political revolution.

It is not surprising, then, that hardly anything was accomplished in the way of authorship by the physicians of the Colonies during the first century of their settlement. For fifty years nothing at all was published by them, of which we have any record. The first essay came out in 1677, bearing the title "A Brief Guide in the Smallpox and Measles." It was published in the province of Massachusetts, thirty years before the first newspaper was issued in America, and more than forty years before one was established in Philadelphia. Its author, Dr. Thomas Thacher, as

was not uncommon in the new settlements at that day, united in his person the two professions of divine and practitioner of medicine. He was the first minister of the Old South Church, in Boston, and was accounted "a great divine" as well as "a learned physician," who, preaching and practising "to general approbation" as long as he lived, had his name worthily perpetuated in the *Magnalia* of the famous Cotton Mather. His unpretending essay, composed especially for the instruction of the people, seems to have remained more than half a century without a successor. It was not until 1720 that the next tract relating to medicine appeared. Dr. Cadwallader Colden, in that year, published an account of the climate and diseases of New York, founded on his own observations and experience; and from that time medical publications followed in quicker succession.

Three essays respecting inoculation for smallpox were published within a few years of each other, by three Boston physicians. Dr. Nathaniel Williams, one of the three, was, like Thacher, both physician and preacher; another was Dr. Benjamin Colman; and the third was the celebrated Zabdiel Boylston, who was the first physician to make trial of the hazardous practice in the new world, and who narrowly escaped falling a martyr to it. Pharmacy and the Medical Botany of the new territory, attracted the attention of its physicians at an early day; and treatises on the preparation of drugs, the polygala senega, and the great water dock, were written by Dr. Howard, Dr. Tennent, and Dr. Colden. Dr. John Clayton, of Virginia, produced, in 1743, a *Flora Virginica*, so highly esteemed that an edition of his work was issued some years afterward in Leyden. An essay on fevers was written by Dr. Walton in 1732; one on scarlatina, by Dr. Douglas in 1736; one on the iliac passion, by Dr. Cadwalader in 1740; and one on yellow fever, and a second on the causes of the variety of colors in people of different climates, by Dr. John Mitchell in 1743. Dr. Lining also wrote an account of yellow fever, in which he started the idea, never before advanced, that persons who have suffered with the fever are not subject to a second attack.

Douglas, in his paper, was the first to recommend calomel in scarlet fever. Cadwalader had the good sense to condemn the use of quicksilver and drastic purgatives in lead colic, which he described as the "iliac passion," and advised opiates as preparatory remedies in that affection. The letters of Mitchell on yellow fever are of peculiar interest, as having led to the heroic practice in the disease for which Dr. Rush was noted at a subsequent day. Dr. Chalmers, of South Carolina, published a paper on tetanus in 1754. Dr. Bond, of Philadelphia, published, in 1759, an essay on the use of bark in scrofula; and in 1760 Dr. John Bard, of New York, made public the facts in a case of extra-uterine fœtation which had come under his notice. Dr. Bard, the same year, also published a valuable essay on the nature and causes of "Malignant Pleurisy," as it prevailed on Long Island in the winter of 1749, a disease since recognized in the epidemics of typhoid pneumonia which have more than once swept over our continent. In 1769 some letters on malignant sore throat were published by Dr. Ogden, of New York; and Dr. Kearsly, of the same place, sent the same year a paper to the Gentleman's Magazine, at London, on angina maligna. The thesis of Dr. John Moultrie, of Charleston, the first graduate in medicine from South Carolina at Edinburgh, was printed in 1749. It described yellow fever as he had seen it in his native city. The theses of Morgan, Shippen, Kuhn, Rush, Bard, Kissam, and other American students, were printed at Edinburgh from 1749 to 1776.

Some of these essays, treating of the diseases incident to the new world, deserve to be rescued from the oblivion that has overtaken them, and if collected would form a volume acceptable to the profession. But it is to be remarked that they were all issued in a fugitive form; many of them treated of subjects the interest of which has long since passed away; most of them were brief papers, such as would now make articles in our medical journals, and nearly all are now wholly inaccessible. At most, they can be regarded as nothing more than an introduction to the medical literature which in the course of the century succeeding them has become so voluminous.

The medical literature of America may with truth be said to have commenced with the century whose years we have just numbered. Just as it was about to open, Dr. John Jones, of New York, foreseeing the need of surgical knowledge likely soon to arise, prepared, in the autumn of 1775, and in anticipation of the impending struggle, a little volume on wounds and fractures. It is the first, in time, of American works on surgery. Its claims to merit were small, if regarded as an original work, since it was little more than a compilation; but the compiler was a surgeon familiar with the art about which he was writing, and his volume proved highly serviceable to our inexperienced and young surgeons during the war. In 1776, an able work was published in two volumes, on the Climate and Diseases of South Carolina, by Dr. Lionel Chalmers, of Charleston.

But ten years before this time, an event occurred which marks an epoch in the medical history of the country. I refer to the creation of a public school of medicine. This, more than any other movement recorded in our history, has shaped our medical literature and contributed to its growth and development. The school was inaugurated under circumstances that invest it with uncommon interest. The address delivered on the occasion was published, and is extant in our libraries. Among the Trustees of the College, who weighed the arguments urged for the erection in it of a Medical Department, was Benjamin Franklin, President of the Board. Dr. John Morgan and Dr. William Shippen, its founders and earliest professors, were medical scholars such as might rightfully aspire to lay the foundations of medical education in a new empire. They had prepared themselves for teaching by long and laborious study. An imposing assemblage of the citizens of Philadelphia was present to witness the commencement in their college, and to hear Dr. Morgan's address on the institution of medical schools in America.

"It is now," he said, "more than fifteen years since I began the study of medicine, and I have arrived at the middle age of life endeavoring to lay up treasures of knowledge." And he continued, in words that proved prophetic, "Perhaps this medical institution, the first of its kind in America, though small in its beginning, may receive constant increase of strength, and annually exert new vigor. It may collect a number of young persons of more than ordinary abilities, and so improve their knowledge as to spread its reputation to distant parts. By sending those abroad duly qualified, it may give birth to other institutions of a similar nature calculated to spread the light of knowledge through the whole American continent wherever inhabited."

In the great concourse of "respectable citizens" gathered to hear this discourse, was a young student of medicine who was destined to become not only the great luminary of the school, but the foremost of American medical teachers and writers. Benjamin Rush had been for three years

a student under Dr. John Redman, in Philadelphia, translating Hippocrates, and making memoranda on yellow fever and other current epidemics of the time. Three years later he was admitted to the degree of Doctor of Medicine in the University of Edinburgh, having defended a thesis *De Concoctione Ciborum in Ventriculo.* Returning home he was elected, in August, 1769, when under twenty-four years of age, Professor of Chemistry in the Philadelphia Medical College, now the University of Pennsylvania.

With Rush, to teach was to write. The motto on which he was in the habit of insisting most strongly to his pupils was, *legere sine calamo est somnere;* and he studied his profession with pen in hand, recording observations which are still of value to writers on yellow fever. The year after his election he appeared before the public as author of what he styled "Sermons" to young men on temperance and health. Three years later he was invited to deliver the annual oration before the American Philosophical Society, of which Franklin and Jefferson were members. He chose as his subject "The Natural History of Medicine among the Indians of North America, and a comparative view of their diseases and remedies with those of Civilized Nations." This oration makes the first paper in his "Inquiries," the first volume of which was issued in 1788. No one can read it without a feeling of profound respect for the talents and scholarship of its author. It is philosophical in tone, rich in original observations, and written in a simple, graceful style. The volume to which it forms the introduction raised its author at once to a position among the leading medical writers of the time. Of the other papers in the volume, one is devoted to pulmonary consumption, and it is interesting to remark that in his treatment of that disease Dr. Rush anticipates the therapeutics of the present day. He gives a graphic description of "breakbone fever," in which he was the first to note the unwonted depression of spirits which attends the disorder. For scarlet fever, on which he has a chapter, he thought that he had found a specific in calomel, which he believed to be as sure a remedy for croup, also, as bark is for intermittent fever.

A second volume of "Inquiries" followed on the 4th of July, 1793. Another oration before the American Philosophical Society forms the first paper in this volume. Its subject is "The Influence of Physical Causes on the Moral Faculties." The titles of his orations indicate the decided turn of his mind to philosophical disquisitions; the matter composing them proves how closely and widely he had observed, and how profoundly he had reflected on, the subjects of discussion. A second paper of great interest in this volume is an inquiry into the causes of the increase of bilious and intermittent fevers in Pennsylvania. A third relates to pulmonary consumption, to which he returns in his second volume to insist, with greater emphasis, on the utility of iron, cold baths, and horseback exercise, in the early stage of the disease. Another chapter is given to an inquiry into the state of the body and mind in old age, with observations on its diseases and their remedies. It is, perhaps, the best in the volume. It is a charming article, and will compare most favorably with any essay on the subject to be found in any literature.

Dr. Rush's third volume was issued in 1794, and is taken up with a history of the yellow fever, which ravaged Philadelphia the year before. It will be read, it is safe to say, while yellow fever continues to scourge the human race. No more graphic account of a pestilence was ever

written by a medical man. It possesses all the charm of a personal narrative. The author reveals the state of his feelings under the varying aspects of the epidemic. "Heaven alone," he says, "bore witness to the anguish of my soul in this awful situation," baffled in every attempt to stop the ravages of the disease. And when, after reading the manuscript account of the fever by Dr. Mitchell, he became successful, he says: "Never before did I experience such sublime joy as I now felt in contemplating the success of my remedies. It repaid me for the toils and studies of my life. The conquest of this formidable disease was not the effect of accident, nor of the application of a single remedy; but it was the triumph of a principle in medicine. The reader will not wonder at this joyful state of my mind when I add a short extract from my notebook, dated the 10th of September: 'Thank God, out of a hundred patients whom I have visited or prescribed for this day, I have lost none.'"

The principle by which Dr. Rush was conducted to these happy results, was that the debility attending this malignant fever is apparent merely—an *indirect* debility—in which the abstraction of blood and evacuation by purgatives tend to raise the pulse and increase the strength of the patient. His practice consequently was to bleed again and again, and give calomel and jalap in active doses, reducing heat by cold water. Finding the dose of ten grains of each drug too slow in its operation, he increased the quantity of jalap to fifteen grains. But the pleasure derived from his great triumph was attended by a serious drawback; his practice was denounced in unmeasured and bitter terms by many of his professional brethren. The effect of this was to excite such a prejudice in the public mind against his mode of treatment that he found it difficult to carry it out. Two of his colleagues, Kuhn and Wistar, united in its condemnation. Currie, a practitioner and writer of note and influence, declared in the public prints of Philadelphia that it "could not fail of being certain death." The report was started by some enemy that Dr. Rush was insane, and he relates that one of his former patients, a lady, "who had believed the report, expressed her surprise at perceiving no deviation from his ordinary manner in a sick room." It was even proposed by some of his excited fellow citizens to "drum him out of the city."

The terms in which Dr. Rush referred to the conduct of his enemies, under these trying circumstances, are worthy to be reproduced in this narrative, as illustrating the magnanimity of his nature. He says: "I allude to these slanders now only for the sake of declaring, in this public manner, that I most heartily forgive them; and that if I discovered at any time an undue sense of their unkindness and cruelty, it was not because I felt myself injured by them, but because I was sure they would irreparably injure my fellow citizens, by lessening their confidence in the only remedies that I believed to be effectual in the reigning epidemic. I commit the calumnies which have followed my opinions and practices in this fever to the dust."

The fourth and last volume of the "Inquiries" was issued in 1796; and in this the author took occasion to announce that he was engaged in preparing a work on Diseases of the Mind. This volume opens with an account of another epidemic of yellow fever, by which Philadelphia was visited in 1794. Dr. Rush contends earnestly for the doctrine, so unpalatable to the citizens, that the fever was of local origin, and depended upon accumulations of filth which it was in their power to

remove, even going so far as to say that he believed the time would come when municipal authorities would be held responsible for the spread of all such fevers. "Seasons and climates," he continues, "are not necessarily sickly. The sun would seldom strike by day, nor the moon by night, were pains taken to prevent the accumulation and putrefaction of those matters which occasion malignant fevers." He believed, when first asserting this opinion, that it was new, but, discovering his error, he was careful to state that it had been advanced by Dr. Thomas Bond, as early as 1766, in a clinical lecture at the Pennsylvania Hospital. At one time Dr. Rush regarded the fever as contagious, but he candidly admits that he was convinced by the arguments of his pupil, Dr. Charles Caldwell, that "the pestilence is devoid of any such power."

One of the papers in this volume is an inquiry into "the proximate cause of fever," which he found in spasm of the extreme bloodvessels; but the leading paper is a defence of bloodletting, which brings out all his power and eloquence as an advocate. It is written with the earnestness and spirit of one who felt himself engaged in establishing a great truth; who believed that he was contending for a principle that involved the interests of his race. While candid in its statements, his defence is heroic in spirit, and its influence in popularizing the use of the lancet was felt in every part of our country. The practice is now spoken of as "atrocious." There can be no doubt that bloodletting was carried to excess by Rush and his followers, but it admits of a doubt whether the practitioners of the present day have not run to the other extreme.

In this volume, and in connection with bloodletting, Dr. Rush repeats a belief which he had before expressed, that the pains of labor would be annulled by the discovery of an anæsthetic. This was no mere guess, but an opinion reached by a process of induction. "I was encouraged to cherish this hope," he says, "by having known delivery to take place in one instance during a paroxysm of epilepsy; and having heard of another during a fit of drunkenness in a woman attended by Dr. Church, in both of which there was neither consciousness nor recollection of pain."[1]

In 1798, Dr. Rush published a volume of "Essays; Literary, Moral, and Philosophical," which had previously appeared in the magazines of the day. It is in one of these that he contends against the ancient languages as a necessary part of the education of students of medicine. In 1801 he added to his other works a volume of Introductory Lectures, in one of which he unfolds his views on the unity of disease. Nosology appeared to him mischievous, as directing the attention of practitioners to the name rather than the character of the disease; and in his lectures he inveighed warmly against it. Rising from his chair, to give greater emphasis to his words, he would exclaim, *Nosologia delenda est!* But this question, in which he was able to create a lively interest while he was the popular teacher of the period, has long since been put to rest. His great work on Diseases of the Mind appeared in 1812, only a short time before the death of the author, which took place in 1813.

This work, the result of a lifetime of careful observation and honest inquiry, is, of all his writings, the one now most read and most frequently quoted by medical authors. Nearly all who have followed him on psychological medicine, refer to it as one of the most instructive of the treatises on that subject, and as especially rich in facts. Adopting the enlightened views which Pinel had just promulgated in France, Dr.

[1] Inquiries, vol. iv. p. 376, 3d ed.

Rush gave an impetus to the revolution which this country has witnessed in the management of the insane; and it is his noble eulogy that he aided "in opening the prison doors of the maniac, unbarred his noisome dungeon, and knocked the shackles from his limbs, substituting moral treatment for brute force, and love for fear."[1] He saw, as the great French alienist had pointed out, that the law of kindness is the true one in the management of lunatics; his work gave currency to the novel idea, and, as a result of the light diffused abroad by him, the condition of the insane in all our asylums has been one of steady improvement from his day to our own.

In the century that has passed away since Rush appeared as an author, no one of all the medical writers of America has attained to the popularity which he enjoyed, nor exerted so wide and lasting an influence on the professional mind of his country. During the generation in which he lived he was more generally read and followed, not only than any other medical author of this country, but than all our other writers on medicine put together. Nor is it likely that his name or his works will be forgotten. His writings form, as a whole, a body of philosophical medicine—defective, indeed, in many points—but exhibiting a breadth of view, an originality of thought and conception, an accuracy and extent of observation, and a terseness, vivacity, and clearness of style, that compare well with the best medical works of their period.[2]

A host of minor writers on yellow fever was brought out by the epidemics, as they appeared toward the close of the last century in our Atlantic cities. Among these was Dr. William Currie, of Philadelphia, whose writings on that subject were only less voluminous than those of Rush. He was an independent thinker, a careful investigator of the diseases of the country, and a faithful observer and recorder of facts; but he is likely to be longest remembered for the violence with which he assailed Rush's doctrines and practice. It is, however, due to his memory to record that he retracted publicly, soon after making the harsh strictures on his great contemporary.[3]

[1] Dr. Edward Jarvis.

[2] Before the first volume of Rush's Inquiries was written, Samuel Stanhope Smith, D.D., President of Princeton College, had published a small volume on "The Causes of the Variety of Complexion and Figure in the Human Species," which was republished, with notes, in Edinburgh, by a professor of that university. Dr. Caldwell reviewed it with great acrimony, urging the insufficiency of the causes assigned to produce the existing varieties. The question has been much discussed since that day, but remains still in an unsatisfactory position. In 1781, Dr. James Tilton published a short treatise on the Diseases and Management of Military Hospitals. Dr. Samuel Tenney published, in 1783, an Account of the Saratoga Springs. In 1786, Dr. Matthew Wilson produced an essay on the diseases arising from the air, contending that most of them are generated by miasmata. The same year he wrote for Atkins's American Magazine an account of a malignant fever which prevailed in Sussex County, Delaware, in 1774; and also an account of the severe winter of 1779, which appeared in the Transactions of the American Philosophical Society. In 1788, the Medical Society of the County of New Haven, Conn., published a volume of Transactions, which was much quoted by British writers. In 1793, part of a volume of Transactions was published by the Philadelphia College of Physicians, which, after many years, began to issue a quarterly summary, and now publishes annual volumes. Dr. Charles McLean wrote a work, in 1797, to prove that pestilential diseases were dependent, in all cases, upon certain altered conditions of the atmosphere, and were never communicated by contagion. An oration delivered at the University of Virginia, by M. Coste, Medical Director of the French forces, in 1782, forming a volume of 103 pages, 8vo., was printed at Leyden in 1783. Dr. John Leigh, of Virginia, obtained in 1785 the Harveian prize for an essay on the properties of opium, which was printed at Edinburgh in 1786.

[3] Dr. Currie published, in 1792, a historical account of the climate of the United States. In 1798 he wrote on the causes and cure of remitting or bilious fevers. In 1800 he wrote

Few of our authors on yellow fever are entitled to more honorable mention than Dr. Richard Bayley, of New York, who before writing on this subject had acquired a reputation abroad by his researches in membranous croup. His history of yellow fever in New York is one of the most graphic and instructive produced in our country, and affords evidence of powers which might have placed him in the front rank of our writers, had he lived to develop them; but his fearlessness in visiting the localities in which the epidemic raged, brought on an attack of the fever, which terminated his life just as his labors promised the greatest usefulness to his profession. Another writer, who afterwards rose to great distinction, was Dr. Charles Caldwell. He began to write on yellow fever while yet a student of medicine, having seen much of the epidemic in Philadelphia, in 1793. Of those who contended against its contagiousness, and for its domestic origin, he was among the first and the most earnest. He also wrote against the expediency of quarantine. Dr. John Beale Davidge, at that time a young physician in Baltimore, was also an early writer on yellow fever. He published a paper on that subject in 1798, in which he contended for the doctrine that the fever originated in the places where it prevailed, believing, when he asserted it, that it had not before been taught in this country.[1]

Our medical journalism commenced in New York towards the close of the last century. The first journal was a reprint, in 1790, of the Journal de Médecine Militaire, annotated from the French by Joseph Brown. The Medical Repository made its appearance on the 26th of July, 1797, under the editorial care of Dr. Elihu H. Smith, Dr. Edward Miller, and Dr. Samuel L. Mitchill. Its origin marks an important era in our medical history. Its editors were men of marked ability. Dr. Mitchill was especially noted as a theorist, but he was admirable for the extent and diversity of his attainments, and in versatility of powers had no superior among the medical men of his day. Dr. Miller was an original thinker, an acute observer, of solid judgment and learning, and a pleasing writer. Dr. Smith, the projector of the work, and the most gifted of the remarkable triumvirate, fell a victim to yellow fever in the twenty-seventh year of his age, but not until after he had achieved a national reputation. Among the productions

a sketch of the rise and progress of the yellow fever of 1798 in Philadelphia. In 1811 he wrote a treatise on the diseases most prevalent in the United States at different seasons of the year; and in 1815 he published a synopsis of the theories and doctrines of disease. Other writers were the following: Deveze, who has the distinction of having been the first American writer to declare against the contagiousness of yellow fever; Cathrall, who wrote, in 1796, on the synocha maligna of Philadelphia; Dr. J. O'Reilly, who, in 1798, wrote on the "Contagious Epidemic Yellow Fever" of that city; Seaman, who wrote, in 1796, on the yellow fever of New York; and Dr. Samuel Brown, who, in 1797 and 1800, wrote on the yellow fever of Boston. Noah Webster also published, in 1796, two volumes on Pestilential Diseases, forming a valuable work of reference. Among other writers at that period were Hardie, Tytler, Pfaff, Hosack, and Davis. J. Henry C. Helmuth, of Philadelphia, wrote a tract in German "for the reflecting Christian," in which he presented the subject in its religious bearings. Ffirth, a few years later, published a number of experiments made to determine the question whether the disease could be communicated from person to person. One of these experiments was to swallow the black vomit.

[1] To the list of our early writers on yellow fever, the following names are to be added: Matthew Carey, Pascalis, Condie and Folwell, Chadwell, Manly, Rushton, Addoms, Martin, Monson, and Monson, Jr. The various publications relating to the early epidemics of yellow fever on our continent would form a curious body of medical literature, which it would be interesting to study. While the discussions were going on about the nature and treatment of yellow fever, Drs. Yates and McLean found time to write a volume of a hundred and fifty pages on the Brunonian theory of Life.

left behind him to testify to his genius, is a poetical introduction to Darwin's "Botanic Garden." The Repository, under these gentlemen's guidance, acquired great popularity and influence, reaching every part of the country, and receiving contributions from its best writers. Among other articles were Physick's report of his autopsies in yellow fever, and the papers of Stearns and Prescott announcing the oxytocic properties of ergot. In the hands of numerous editors the Repository continued until it reached its twenty-third volume.[1]

In 1805, Dr. Caldwell, who had become widely and favorably known by a translation of Blumenbach's physiology, and by his writings on yellow fever and quarantine, issued a volume of Select Theses, written by graduates of the University of Pennsylvania, with a preliminary discourse and appendix from his own pen. A second volume followed in the succeeding year, the appendix containing a paper by the editor on the "vitality of the blood," one of his favorite doctrines. It was his purpose to continue the work annually, but the trustees, by the advice of the Medical Faculty of the University, saw fit to relieve candidates of the burden of printing their theses, and in consequence Caldwell's publication fell through. Strangely enough, he looked upon the action of the faculty in this matter as "a measure of vengeance and mischief" to himself.

Dr. Caldwell was one of the most prolific of American medical authors. His writings were fragmentary, consisting of essays, reviews, and discourses, scattered through the literary magazines and medical journals of the day, but if collected would make not less than ten octavo volumes of a thousand pages each. Almost always in a controversy on some point in medicine or medico-theology, he was seldom lost to the public eye from the time he entered his profession till near the close of his life. He was a man of varied attainments, but his learning was remarkable for extension of surface, rather than accuracy or depth; and, while he wrote on a great variety of subjects, it cannot be said that he added much to the stock of medical science. It was a peculiarity of his mental constitution that he continually set himself to advocate opinions and systems generally rejected, and to assail the most cherished beliefs of men. "Better occasionally broach startling error, than deal continually in time-beaten truisms," was one of his maxims; and it may be said that he wasted his fine powers, as another great scholar declared he had wasted a life, "in levities and strenuous inanities." Many of his best years were devoted to the exposition and defence of phrenology, which, towards the close of his career, was superseded by mesmerism and spiritualism. Such was

[1] The New York Medical Repository was followed, in 1803, by a periodical at Philadelphia, the Philadelphia Medical and Physical Journal, edited by Dr. Benjamin Smith Barton; but this contained too much botany and zoology to suit the profession, and it expired at the end of the third volume. It was succeeded, in 1806, by the Medical Museum, under the editorial care of Dr. John R. Coxe, which was continued seven years. In 1808 a journal was started at Baltimore, by Dr. Tobias Watkins, the Baltimore Medical and Physical Recorder, which had a brief career, and gave place to another in that city, of still shorter duration, edited by Dr. Nathaniel Potter, under the name of the Medical and Philosophical Lyceum. In 1810 the American Philosophical Register was set on foot by Dr. Hosack and Dr. J. W. Francis, at New York, its fourth volume containing the famous letters of Dr. Mitchell to Dr. Franklin on yellow fever, communicated by Dr. Rush to Dr. Hosack. The Eclectic Repository and Analytical Review was started at Philadelphia in 1811, by Drs. Otto, Hewson, James, and other physicians of that city, and reached its tenth volume, when it was discontinued. Among its notable papers is a brief account of McDowell's renowned operations for diseased ovaria. In 1812 the New England Journal of Medicine and Surgery was established by a number of the physicians of Boston.

his assurance of the truth of organology, that he advised the use of the trephine to the head in mania, over the part of the brain indicated by phrenology as affected. He wrote an analysis of fever which would have possessed value if it had been the fruit of clinical observation; but it was purely a work of the closet. As a writer of critical reviews, he was one of the readiest, most affluent, and most formidable of his day; in tone somewhat too domineering and dogmatic, and in language often unnecessarily bitter, but always vigorous and independent. His most elaborate work was an autobiography published since his death, which has added nothing to his fame. Exhibiting, throughout, the temper which caused him to suspect his old friends and teachers in the University of Pennsylvania of framing "a measure of vengeance" against him, he is everywhere unjust to the memory of his contemporaries.

About the beginning of the present century, the first works on systematic medicine began to appear, nearly all up to this time having been monographs. In 1801, Dr. B. S. Barton published some contributions for an essay towards a Materia Medica of the United States, having commenced in 1798 to make collections for such a work. In 1803, he issued a work on the Elements of Botany, with some references of a desultory character to vegetable physiology. Dr. J. R. Coxe, in 1806, compiled a Dispensatory, which in the scarcity of medical books at the time proved of much service to practitioners.

In 1807, Dr. Samuel Bard, of New York, gave to the profession a small but admirably written volume on the Science and Art of Midwifery. His "compendium" was the *vade-mecum* of our accoucheurs for more than a quarter of a century, and is still quoted with respect by writers on obstetrics. Its sound judgment and good sense made it a safe guide, and the clearness and simplicity of its language adapted it to the capacity of midwives, in whose hands, when it appeared, was most of the obstetrical practice of the country.[1] In 1810, Dr. James Thacher prepared a valuable Dispensatory, on a plan which had been proposed by the Medical Society of Massachusetts for securing uniformity in the Pharmacopœias of the United States; this work remained long in use by the profession.

In 1811 appeared the System of Anatomy, by Dr. Caspar Wistar, of the University of Pennsylvania, which was the popular text-book in American medical schools for more than thirty years. It passed through nine editions, enriched and enlarged by successive editors, its author having died shortly after the publication of the second volume. Wistar was the first writer on anatomy to describe accurately the extremities of the ethmoid bone, which previously had been supposed to belong to the sphenoid, and hence they have ever since been known as "the pyramids

[1] Dr. George C. Shattuck, in 1808, published three dissertations on Boylston prize-questions for 1806 and 1807. They treated of Diseases of the Skin, Mortification, and Biliary Concretions. Dr. Edward Cutbush, of the U. S. Navy, wrote in 1808, a work on Preserving the Health of Sailors and Soldiers. In 1808 and 1811, the monograph of Dr. Bayley on Membranous Croup appeared in the Medical Repository. The true nature of the complaint had been pointed out by him as early as 1781 in a letter to Dr. Hunter. In 1809 Dr. J. C. Warren published a valuable paper on Organic Diseases of the Heart. Dr. Nathan Strong, in 1810, wrote a treatise on Spotted Fever, which was followed the next year by papers on the same subject by Dr. North, Dr. Woodward, Dr. Bester, and Dr. Fish. Dr. Hugh Williamson, in 1811, published Observations on the Climate of different parts of America, to which he added a history of North Carolina, including an account of its diseases in 1812.

of Wistar."[1] His volumes contained little that was original, but the matter was presented in a shape highly convenient to students, and it is not too much to say that their publication marked an era in the history of American authorship.

Of scarcely inferior popularity at the time, and of the greatest value to the profession of our country, was another work which proceeded from the same institution a few years later. The "Elements of Surgery," by Dr. John Syng Dorsey, in two volumes, appeared in 1813. The author was a young man, and could not claim to have had much personal experience in surgery, but that of his distinguished uncle, Physick, then, as long afterward, the leading surgeon of the United States, was at his command with which to enrich his work. It was written in a simple, graceful, flowing style, with special reference to the needs of students, and so well did the author succeed in his design that his book was at one time adopted in the University of Edinburgh as a text-book. That place it assumed at once in our schools, and maintained until later systems supplanted it. Nor was it less prized by the profession as a work of reference. It passed through four editions, two after the author's death, which occurred while he was still young. Had he lived to discipline and mature the high gifts with which he was endowed by Nature, he would have attained undoubtedly to great eminence as an author. He indulged occasionally in poetical effusions, some of which, says Professor Gross, his biographer, "embody uncommon vigor of thought and power of description." Something of the glow of poetry may be detected in his professional writings.

At all times, while acknowledging his indebtedness to foreign surgeons, Dorsey does not hesitate to criticize their prejudices and practice. Thus, in the preface to his Elements he says: "Great Britain and France have been foremost in the cultivation of modern surgery, but their deficiency in philosophical courtesy and candor has in some instances greatly retarded its progress. To illustrate this remark it will be sufficient to state that the doctrine of adhesion, so ably developed in England, has been shamefully neglected in France; and that French surgery in fractures finds no advocates in Britain. Some of the best writings of Desault have never been translated into the English language, and those of Hunter are unknown or disregarded throughout the continent of Europe. This spirit of hostile rivalship, extending from the field of battle to that of science, cannot fail to exert a pernicious influence on practical surgery; a truth too palpable to escape the observation of any foreigner who visits a European hospital. An American, in walking their wards, sees with surprise in London a fractured thigh rudely bound in bundles of straw, and the patient discharged limping with a crooked limb. In the French capital he witnesses an amputation, and is disgusted with the officious zeal with which the surgeon crams a handful of lint between the stump and the flap which covers it, with an express design to prevent their adhesion."

While the University of Pennsylvania was contributing thus liberally to our literature, the University of Maryland was not idle. Dr. Davidge, its honored founder, in 1812, essayed another System of Nosology, more simple and therefore more eligible than that of Cullen then prevailing. In 1814 he republished his memoir on yellow fever, and with it his inaugural thesis on the Catamenia, in a volume entitled "Physical Sketches,

[1] Gross, Introductory Lecture on American Medicine.

or outlines of correctives applied to certain errours in Physick," which, besides these, contains an elaborate history of the various methods of amputation, including one of his own. The introduction to his Nosology contains a powerful argument against Rush's theory of "the unity of disease," which at that time excited a good deal of interest. His thesis, written in Latin, and first printed at Glasgow in 1793, maintains the doctrine that the menstrual flux is a true secretion. Not the least pungent article in his sketches is the review of a case related by Caldwell in his paper on the vitality of the blood. The case was as follows: The writer, after the extraction of a tooth, had some trouble with hemorrhage, but, keeping the blood in the alveolus by pressure, at last arrested its flow. The weather being warm, he examined the coagulum several times a day, but instead of its becoming "putrid and offensive, he was surprised to observe it on the fourth or fifth day after coagulation beginning to assume the appearance of flesh." And this incarnation commenced, he relates, not at the circumference, "but in the centre of the coagulum, at the greatest possible distance from any vessels that might, by elongation, have been protruded from the adjacent gums;" so that "what had been at first nothing but congealed blood, became a piece of perfect flesh, similar in texture and appearance to that of the gums." Upon this remarkable case Davidge makes the following comments: First, that it is "solitary of its kind;" then, that the clot is singular "from its transparency," all other clots of blood being opaque; and, finally, he is bold to say, that "the asserted fact is bottomed upon the broad basis of human credulity."

As a controversialist, Davidge displayed undue acerbity, as the reader will conclude from the foregoing quotation. His style, at the same time, was stiff, involved and affected, entirely unlike that in which he lectured. His lectures, indeed, afforded a model of simple elegance, while the moment he took pen in hand he seemed to forget the English idiom. Ten years after the appearance of his "sketches," he issued the first number of a journal, The Baltimore Philosophical Journal and Review, which he proposed to continue, but was obliged to give up for want of encouragement. The number published was written almost entirely by himself, in his peculiar, crabbed style, and in a temper so acrimonious that it was hardly read at all when it came out, and might soon after have been bought almost as waste paper.[1]

In 1816, a syllabus of the lectures of Dr. James Jackson in the Massachusetts Medical College, was published, and only increased the general feeling of regret that the learned and able author did not prepare a systematic treatise on the principles and practice of medicine.

In 1817 was published a volume of lectures on the "Elements of Therapeutics and Materia Medica," delivered by Dr. Nathaniel Chap-

[1] Dr. J. W. Francis in 1811 published an able paper on Mercury. It was in 1813 that Dr. Prescott wrote his article on Ergot, which was inserted in the thirteenth volume of the Dictionnaire des Sciences Médicales. The same year Dr. John Warren wrote ably on Calomel, arguing against its utility in hydrocephalus. Dr. Charles Wells, a native of South Carolina, also published that year, in London, his philosophical theory of dew, which was crowned by the Royal Society in 1814 with the Rumford medal. Dr. E. Hale, Jr., in 1814, published an instructive volume on spotted fever; and in that year Dr. J. Dyckman produced an elaborate dissertation on the pathology of the human fluids. Dr. Joseph Gallup, in 1815, contributed a practical work on the epidemic diseases of Vermont from its first settlement. In the same year Dr. Job Wilson wrote an inquiry into the nature of spotted fever, and Dr. Ennalls Martin produced a history of the epidemics of the winter of 1813 and 1814 in Talbot and Queen Anne's counties, Maryland.

man in the University of Pennsylvania, to which a second volume succeeded in 1819. Nothing so fresh in style, or so original in theory, had been contributed to our literature since the days of Rush. These lectures were received by the profession with abundant favor. As "discourses" they were characterized by some redundancy of expression, and a severe critic would have termed their style florid; but they were, perhaps, more pleasing to youthful readers on that account. The writer remembers well the feeling of relief, not to say delight, with which he turned to them from the dry treatises on Materia Medica, and the drier dispensatories which they came to supplant. Chapman's theory of the operation of medicines was of modern date, as he expressed it, and of captivating simplicity. It was this: "That all such agents act by exciting a local impression, which is extended through the medium of sympathy." No article, he held, "ever enters the circulation as a medicine." "It cannot, indeed, be credited," he says, "that any substance, after a subjection to the digestive and assimilative process, retains in the slightest degree its original properties." Besides this, everything that acts upon the system "is a stimulant." Such was his philosophy in a few words, which one of his critics hardly ventured to controvert when it was announced, feeling himself *tibi miles impar.*[1] How successfully it has since been opposed need not be related here.

With this popular work came out some numbers in quarto of a treatise on the "Vegetable Materia Medica of the United States," by Dr. W. P. C. Barton, of the same University; and a treatise on Medical Botany, of much research and substantial value, in three volumes octavo, by Dr. Jacob Bigelow, of Harvard University. The work of Barton was subsequently issued in a form magnificently illustrated, under the name of "Flora of North America."

In 1817 was originated a Journal of Science, which, though not medical in character, exerted a decided influence on medicine in the United States. This was the "Journal of Science and Art," projected at New Haven by Prof. Benjamin Silliman, of Yale College. The work remained under his editorial care until it reached its fiftieth volume. No American serial, it may be safely said, has contributed more to the fame of our country, and none has done so much to develop its science in the direction of natural history. Its volumes form a magazine of facts and observations indispensable to writers on any branch of natural science, and contain many articles relating directly to medicine. Among the authors of our country it would be difficult to name a writer more polished and graceful, or more variously gifted and informed, than the elder Silliman, who devoted a long life assiduously to the advancement of useful knowledge among his countrymen, and who, on retiring from his work as editor, had the good fortune to find in his own family successors qualified to sustain the high reputation of his journal. Under the direction of Prof. B. Silliman, Jr., and Prof. James D. Dana, the American Journal of Science has maintained its rank as one of the leading periodicals of the world.

Dr. James Thacher, in 1817, published a work styled the "Modern Practice of Physic," the reception of which by the profession was so favorable that a second edition was called for in a few years. Dr. Thacher was at the same time a scholar and a practical physician, equally at home in the sick-room, in writing on the nature and treat-

[1] See Med. Recorder, vol. i. p. 189.

ment of disease, or in drawing up his well-known biographical sketches of noted American physicians. His is one of the names which will be long held by the profession in grateful remembrance.[1]

In 1818, Dr. Nathaniel Potter, the learned Professor of Theory and Practice of Medicine in the University of Maryland, published an elaborate memoir on Contagion, more especially as respects the yellow fever. In the same year the Medical Recorder, which for a time was the most influential journal of medicine in the country, was set on foot by several respectable physicians of Philadelphia. Drs. Eberle, McClellan, and Calhoun were subsequently announced as its editors. In 1829 it was merged in the American Journal of the Medical Sciences. Among other interesting papers it contains a report of the famous operation of Deaderick, of Tennessee, for removal of the lower jaw, the first on record. Many other papers by the ablest medical writers of our country found their way into the Recorder.

In 1819, the first American works on Chemistry appeared. Dr. John Gorham published his Elements of Chemical Science in two volumes; and Dr. Franklin Bache, of Philadelphia, prepared an elementary treatise on chemistry about the same time. During this year, also, was announced a new theory of Galvanism, with an account of the calorimotor, a new galvanic instrument, by Dr. Robert Hare, the gifted Professor of Chemistry in the University of Pennsylvania.[2]

The United States Pharmacopœia, proposed in 1808 by the Massachusetts Medical Society, was issued in 1820, and having been repeatedly revised at intervals of ten years, retains its place among our standard works as a book of reference. In 1820 also was established the medical journal which has done more than any other of our serials to advance and shape the medical literature of the United States. Dr. Nathaniel Chapman, Professor of Theory and Practice in the University of Pennsylvania, started that year the Philadelphia Journal of the Medical and Physical Sciences. In the inception of the enterprise he was alone, but in a short time he secured the valuable assistance of Drs. Dewees and Godman as associate editors. From the beginning, the Journal evinced the vigor to have been expected from the editorial talent engaged upon it. Its editor in chief was then in the prime of his great intellectual powers, and Godman had already given proof of a genius rarely equalled in our profession.[3] In 1827, the name of the Philadelphia Journal was changed,

[1] Dr. James Mann, of New York, contributed an interesting History of the Campaigns of 1812, 1813, and 1814, to which are added surgical cases, and observations on military hospitals and flying hospitals attached to a moving army. Dr. Jabez Heustis published, in 1817, Some Physical Observations and Medical Facts and Researches on the Topography and Diseases of Louisiana. In the same year, Dr. W. C. P. Barton issued a work on the Internal Organization and Government of Marine Hospitals, based on his own experience and observation. The year following, Dr. John King published, at Norwich, a treatise on Extra-uterine Fœtation and Retroversion of the Uterus. Dr. J. C. Shecut, of Charleston, the same year, produced an Essay on Contagions and Infections. Dr. A. H. Stevens published some cases of Fungus Hæmatodes of the Eye, in 1818.

[2] This year appeared also a work by Dr. L. Spalding, of New York, entitled Reflections on Yellow Fever Periods; one by Dr. Felix Pascalis, on the Malignant Yellow Fever of New York in 1819; and one by Dr. Shecut, of Charleston, containing a history of Yellow Fever.

[3] The first account of that strange disease, "Milk sickness," so far as is known, was given by Messrs. Lea and McCall, students of medicine from Tennessee, in one of the earlier numbers of this journal, and it is proper to record that the editor was disposed to reject their history of this disease as apocryphal, for the reason that the facts stated seemed wholly irreconcilable with his hypothesis that poisons in a formal state never entered the circulation.

and it passed into the hands of Dr. Isaac Hays, who, with a judgment, perseverance, and industry, seldom witnessed in journalism, has continued it down to the present day, as the American Journal of the Medical Sciences.

This is the medical journal of our country to which the American physician abroad will point with greatest satisfaction, as reflecting the state of professional culture in his country. For a great many years it has been the medium through which our ablest writers have made known their discoveries and observations.

In 1821, Dr. Hosack's Nosology, which had been issued some years before, reached a second edition, and the Journal of Foreign Medical Literature was set on foot, by Drs. Emlen and Price, and afterwards continued for a time by Drs. Godman and Littell. Dr. Donaldson, in 1821, published a treatise on the present system of medicine and surgery in Europe and America; and Dr. E. Hale, Jr., the same year issued an essay, which had been crowned by the Boylston prize, on the connection between the Stomach and the Urinary Organs.

The following year was an eventful one in the history of our medical literature. A work of much research was produced by Dr. John C. Warren, styled "A Comparative View of the Sensorial and Nervous Systems in Man and Animals;" a treatise, by Dr. John Eberle, on Materia Medica, was published; and two journals appeared, the New York Medical and Physical Journal, conducted by Drs. Dyckman, Francis, and John B. Beck; and the Western Quarterly Reporter, edited by Dr. John D. Godman, at Cincinnati, marking the rise of medical journalism in the Valley of the Mississippi. The Materia Medica of Eberle was a work of sterling merit, at once learned, practical, and judicious, and written in a clear, simple style. Those most partial to Chapman could hardly help admitting that, in all the qualities sought for by students and practitioners in a text-book or a manual, it was superior to his treatise on the subject. Its author, with far less genius, was a scholar of very much greater research, who had taken time to look extensively through the German and French literature on the subject as well as that in his own language, and to make his work as thorough as industry could render it. It was long one of the most popular text-books in our schools, as well as an authority in the profession. Dr. J. Bigelow, the same year, published a valuable treatise on Materia Medica, intended as a sequel to the Pharmacopœia of the United States. An era was opened in the professional literature of the West by the appearance of Godman's journal at Cincinnati, which, though of short duration, was the beginning of a most prolific series. Only six monthly numbers of his work were issued, but as proof of the activity and zeal with which the editor devoted himself to it, it is worthy of mention that, of the matter composing them, he wrote more than three hundred pages.[1]

The year 1823 is also memorable as having produced the classical work on Medical Jurisprudence by Dr. T. Romeyn Beck and Dr. John B.

[1] Dr. Godman, in 1824, published a thin octavo volume made up of anatomical researches, which was followed, the succeeding year, by a smaller volume, entitled Contributions to Physiological and Pathological Anatomy, founded on his own dissections. His American Natural History, in three volumes, appeared in 1826, and gave him standing with the naturalists of his time. A small volume, composed of Addresses delivered on various occasions, followed in 1829. The "Rambles of a Naturalist," written for a weekly magazine in Philadelphia, and published in a small volume after the author's death, was the last of his labors, all of which bore evidence of a passionate devotion to, and of a genius capable of the highest achievements in, science.

Beck. This admirable treatise has rather gained in reputation than declined during the many years that have elapsed since its publication; nor is there any reason why it should not maintain its enviable position among the works of its class, since the changes which, under a rapid succession of systems, are inevitable in other departments of medicine, are not met with in medical jurisprudence. Besides this learned work, which has conferred so much fame upon our authorship, others of note were written in 1823. One of these was on Diseases of the Eye, by Dr. George Frick, of Baltimore; and a larger one on Fevers, from the pen of Dr. Thomas Miner and Dr. Wm. Tully, two eminent physicians of New England, had the recommendation of being founded on the observation and experience of its authors at the bedside.[1]

Dr. William P. Dewees, who had become eminent in Philadelphia as an accoucheur, and was known in all parts of his country as a writer on obstetrics, appeared in 1824[2] as author of a System of Midwifery, which not only gave new dignity to the art in the New World, but may be said to have created an American School of Obstetrics. The popularity of this work was very great, and in thirteen years it had passed through a dozen editions. The author wrote from his own experience, and expressed himself as one having a right to speak. If he was dogmatic, it must be remembered that he was almost without a rival near him to question his authority, and if his pages betray haste and carelessness, the defect must be set down to the incessant press of business which left him little leisure for correcting what he wrote. There is something painful in the thought that a work once so much read, and held in such high esteem, should now be entirely neglected. But the march of science renders such a fate inevitable. Other systems more advanced have long since taken its place.

A treatise on the Diseases of Females, and one on the Physical and Medical Management of Children, followed Dr. Dewees's Midwifery in 1826, and extended his fame as an author. It was many years before either work was superseded, and while they stood their ground they were to be found in nearly every medical library of the country. In 1830 he entered a new field, sending forth a "Practice of Physic," comprising most of the diseases not treated of in his "Diseases of Females" and his work on Children; but this proved a failure. The writer had undertaken too much. Of all subjects pertaining to obstetrics, he was master, but his medical philosophy was tainted by the declining system of Broussais, and his therapeutics proved unpopular. As a specimen of his practice, it may be mentioned that he reports having bled one of his patients six times for a fever, brought back, after she was convalescent, by a single plate of soup.

Few more remarkable men have appeared in the American profession than Dr. Nathan Smith, author of a treatise on typhus fever written in

[1] The following works were also issued in 1823: An Essay on Suspended Animation, by Dr. Samuel Calhoun; a Guide for Practising Physicians in Visiting the Sick, by Dr. J. Lobstein; a Treatise on Cynanche trachealis, by Dr. W. Sweetzer, of Boston, who also wrote on Consumption and on Indigestion in 1836 and 1837; an Account of Yellow Fever in New York, in 1822, by Dr. T. S. Townsend; and a History of Yellow Fever as it appeared in Natchez, in 1822, by Dr. Henry Tooley.

[2] A work on the Philosophy of Epidemics was contributed in 1824 by Dr. Joseph M. Smith, and also three volumes of Essays on various medical subjects by Dr. David Hosack. The Medical Intelligencer, a monthly in quarto form, was started in the winter of 1823–4 by Dr. J. V. C. Smith, at Boston; and the Medical Review and Analytical Journal by Dr. Eberle and Dr. George McClellan, in Philadelphia, in 1824.

1824. He was a great surgeon, an able teacher, and a judicious author. In independence of thought, and in sturdy, common sense, he had few equals in his day, and has never had many superiors in the profession. When his little volume appeared, the distinction between typhus and typhoid fever had not been recognized, and it was evidently the latter that he had under consideration. "I have never been satisfied," he declares, "that I have cut short a single case of typhus fever that I know to be such. Typhus has a natural termination like other diseases which arise from specific causes." His readers, fresh from the work of Armstrong, on typhus fever, recommending a vigorous treatment by the lancet and purgatives, were not a little disappointed when they came to his concluding words as to its cure: "All that is required is simple demulcent drinks, a very small quantity of farinaceous food, and avoidance of all causes of irritation." Another work on surgery was produced in 1824, by a professor in the University of Pennsylvania, Dr. William Gibson. It was a heavy work, compared with that of Dorsey, and in comparison with later systems it was greatly wanting in information; but the author made amends for these early defects in succeeding editions, and his surgery acquired at last a just claim to the position given it by his official connections. It was not long in supplanting the treatise of Dorsey as a text-book in the American schools.[1]

In 1826, the North American Journal of Medicine and Surgery was established at Philadelphia, under the auspices of the "Kappa Lambda Society of Hippocrates." Its corps of editors embraced some of the best medical talent of the country. It included the names of H. L. Hodge, C. D. Meigs, B. H. Coates, F. Bache, and R. LaRoche, to which, after two years, those of John Bell, George B. Wood, and D. F. Condie, were added. The aim of the Society was to promote harmony in the ranks of the profession, and with its patronage the journal was expected to have wide circulation. It assumed at once a high rank among the periodicals of the day, but, notwithstanding its acknowledged ability, and the favorable circumstances attending its origin, it was discontinued at the end of the twelfth volume for want of adequate pecuniary support. A systematic treatise on anatomy was added to the list of American books in 1826, by Professor William E. Horner, of the University of Pennsylvania, one of the most pains-taking and assiduous of modern authors; but though fuller and more accurate than the volumes of Wistar, it failed to secure the footing in the schools so long maintained by our earliest work on anatomy.

Dr. James Rush produced, in 1827, a work on the Philosophy of the Human Voice, which for originality and depth of research has been pronounced by competent judges one of the most creditable of the productions of our country relating to medicine. The same year, the Monthly Journal of Medicine originated in Philadelphia, and Dr. Drake took charge of the Western Journal of the Medical and Physical Sciences, at Cincinnati. When he projected his periodical, he assumed as its motto, *E sylvis nuncius;* to which, no readers being secured east of the mountains, he added subsequently the words, *æque atque ad sylvas nuncius.*

[1] Dr. Thomas Miner gave an interesting account of typhus syncopalis in 1825, and an epitome of chemical philosophy was published the same year by Dr. James D. Dana. In 1825 also appeared a Manual of Chemistry by Dr. John W. Webster, Professor of Chemistry in Harvard University, who was executed at Boston in 1850 for the murder of Dr. George Parkman.

He conducted it with great energy for twelve years, when it was merged in a journal at Lonisville.[1]

In 1828, Dr. John E. Cooke produced a work in two volumes of decided originality. It was entitled a Treatise of Pathology and Therapeutics, and proposed a new theory of disease. The theory was one of the most compendious ever framed. It referred all fevers, with cholera, dysentery, and a host of other varying affections, to congestion of the *vena cava.* And his therapeutics were no less simple. All his remedial measures had reference to unlocking the liver by purgatives, at the head of which, in point of efficacy, he reckoned calomel. The aim being to secure bilious purging, cathartics were to be repeated, in increasing doses, until the effect was produced. In the end, this led to an abuse of calomel which brought it into great disrepute; and with its popularity the writings and the system of one of the most learned and conscientious of American authors have passed away. With this work, Dr. Cooke, in conjunction with his colleague, Dr. C. W. Short, started the Transylvania Journal of Medicine, which was edited by them for four years. At the close of the fourth volume it passed into the hands of Dr. L. P. Yandell. Dr. Robert Peter became its editor in 1837. Among its more valuable papers are the reports by Dr. B. W. Dudley of his operations for stone in the bladder, and his cure of chronic epilepsy by removing portions of the skull bone depressed by mechanical injuries.

The Boston Medical and Surgical Journal was also set on foot in 1828, by Dr. J. V. C. Smith, author of a work on the Fishes of Massachusetts. In that year, too, the Medical Biography of Dr. Thacher, a work not likely to be superseded, was contributed to our literature. A year later Dr. Horner produced his System of Pathological Anatomy, founded on original observations.[2]

In 1830, an elaborate work on Chemistry, by Prof. Silliman, was announced. Its arrangement was regarded as defective, and, added to this, its size unfitted it for the use of students, so that it never attained general currency; but as a repository of chemical knowledge interesting to all readers, nothing superior to it has been produced in the English language. Dr. Samuel D. Gross, who has since contributed so largely to the literature of the profession, appeared this year as author of a modest volume on the Anatomy, Physiology, and Diseases of the Bones and Joints, which, it is interesting to remark, contains the first account of adhesive plaster as a surgical appliance in fractures.[3]

In 1831, the classical work of Dr. John Bell on Baths and Mineral Waters appeared. The same author, in 1842, produced another work,

[1] Dr. Guy W. Wright and Dr. James M. Mason had started a semi-monthly, the year before, under the name of the Ohio Medical Repository. In 1832 the Western Medical Gazette was set on foot by the Faculty of the Medical College of Ohio, but at the end of nine months was suspended. It was revived five months afterwards by Dr. Silas Reed and Dr. Samuel D. Gross, who continued it until two volumes were completed. In 1842 the Western Lancet was projected at Cincinnati by Dr. Leonidas M. Lawson; and in 1847 the Dental Register of the West was started at the same place by Dr. James Taylor, of the Ohio College of Dental Surgery.

[2] In 1829 the Journal of Health was projected by Drs. John Bell and D. Francis Condie, at Philadelphia. A Formulary, which was greatly useful to the profession, was published by Dr. Benjamin Ellis; a System of Dental Surgery was contributed by Dr. Samuel Fitch; a Journal of Pharmacy was started by Dr. B. Ellis; and a treatise on the Distinct and Confluent Smallpox was produced by Dr. John D. Fisher.

[3] Dr. S. W. Avery, in 1830, issued a work, meant especially for the people, entitled "The Dyspeptic's Monitor."

written in the same elevated style, on Regimen and Longevity. The kindred topics of Alcohol in its Relations to Medicine, and the Importance and Economy of Sanitary Measures to Cities, were treated of in works issued in 1859 and 1869, and discussed with marked ability. Dr. John Eberle, in 1831, enriched our literature by two works of the highest merit—his treatise on the Diseases and Physical Education of Children, and his treatise on the Practice of Medicine. Nothing equal to them, in every respect, had been produced by an American writer. Not only were they admirable in style, but they were full, accurate, and practical in matter. Both were adopted as text-books in our schools, and passed through many editions before they were superseded.[1]

Dr. Nathan R. Smith issued a volume of great interest, in 1831, consisting of memoirs, medical and surgical, by his honored father, with additions from his own pen. In 1832 the same author contributed a work on the Anatomy of the Arteries, which was of value to the profession.

Dr. Amariah Brigham also, in 1832, commenced a series of publications which excited much interest among students of psychological medicine. The first treated of the influence of mental culture on health. It was republished in Glasgow, with notes by Dr. Robert McNish, and was followed, in 1836, by a treatise on the Influence of Religion on Health and the Physical Welfare of Mankind, which involved the author in an embittered controversy. With these a small volume was brought out by Dr. Brigham, in 1833, on Epidemic Cholera; and, in 1840, one of much more note and significance on the Diseases and Functions of the Brain, the Spinal Cord, and the Nervous System.

The leading production, however, of 1832, was the treatise of Dr. Samuel Jackson, on the Principles of Medicine. This volume came out with a heraldry before unknown in the literary history of our country. "Knowing the anxiety of the profession in regard to the forthcoming work of Dr. Jackson," said the leading journal of the Union, "we applied to its publishers for an early copy," which was duly noticed in terms of the strongest commendation. "Its publication," declared the reviewer, "will constitute an epoch in the history of American medical literature." For years before it was published it had been understood that the author was at work on a treatise in which justice should be done to the merits of physiological medicine; but, unfortunately for it, physiological medicine was about to vanish away when it appeared, and its interest declined with the system. One edition of it was sold, and the publishers proposed to issue a second; but the author would not consent to the labor of remodelling it in a way to make it conform to the medical science of the period, and no other was ever issued. It has been often repeated, without truth, that the work fell still-born from the press, and much has been credited to unfriendly criticism as the explanation of this defeat, but with only a show of reason. Certainly, if it was reviewed harshly in some quarters, in others it received, as has been shown, a full meed of praise. And the most formidable of its critics, a writer in the Transylvania Journal of Medicine, at Lexington, assailed it with

[1] The following works also appeared in 1831: Drawings of the Anatomy of the Groin, by Dr. William Darrach; a Treatise on Malaria, by Dr. U. Parsons; a small volume, by Dr. John Ware, on Delirium Tremens; a work on Chemistry, by Dr. Lewis C. Beck; a modest little volume of Essays on Materia Medica, by Mr. Geo. W. Carpenter; and a singular treatise on Dyspepsia, by Mr. O. Halsted, proposing to cure the complaint by shaking and kneading the digestive organs after eating.

arguments which rather recommended than injured it with most readers. The chief objections urged against it by this reviewer were that it leaned to humoralism in pathology, and ignored the claims of phrenology. The book failed, so far as it was a failure, because its philosophy was defective. It perished with the narrow and fanciful system which the author himself lived to renounce as unfounded.

The treatise of Dr. Robley Dunglison on Physiology also appeared in 1832, and formed a contribution to our literature which all readers hailed as of true value. It was such a compend of physiological science as was before nowhere extant in the English language; incomparably superior to the works of Bostock, Richerand, and Blumenbach, which had previously been the dependence of our students of medicine. Availing himself freely of the labors of others, the author's aim was to present a view of the existing state of physiology, without any straining after originality; so that his volumes are marked by learning rather than novelty of doctrine, but are none the less valuable to students on that account. The style in which they are written is unambitious, simple, clear, and pleasing. That many of his views were not accepted at the time, and that not a few have since been set aside, is what was to have been expected in a science advancing like physiology; but his work, the most advanced of its day, forms a rich treasury of facts and learning which the student may still consult with advantage.[1]

A work in some respects the most fortunate that has been produced in America, appeared the next year in Philadelphia. In 1833, the United States Dispensatory, the joint production of Dr. George B. Wood and Dr. Franklin Bache, two of the ablest medical scholars of our country, was published, and at once took the foremost place among American works of its class. Nearly half a century has been completed since it was brought before the profession, but it may be doubted whether its standing was ever higher than it is at the present day. Its authors commenced life in Philadelphia about the same time, and were early friends. When they rose to distinction they were called to teach in rival institutions, but the jealousies of the schools, if any ever arose, never entered their studies, nor for an hour disturbed their fraternal relations. Laboring on together as edition after edition of their great work was called for, one on the chemical, the other on its botanical department, their cordial intercourse ceased only when one of the laborers was called away by death. Dr. Wood, who survives to enjoy the fruits of his honorable industry, may well recount this enduring friendship as among the greatest blessings of a fortunate life.

In 1833 the profession was enriched by a work which may be said to have enlarged our knowledge on some points in physiology more than all that had preceded it—a work that resulted from an accident. Dr. William Beaumont, a surgeon in the United States Army, had the rare opportunity of observing how digestion is accomplished in the human stomach. A Canadian soldier received a wound from the discharge of a gun, that exposed the interior of his stomach to view. An orifice remained after his recovery, through which food could be introduced, and

[1] In 1832, Dr. Thomas D. Mitchell brought out a treatise on Chemistry, on the basis of that of Reid, his chief additions to which consisted in an argument to show that alcohol might be dispensed with not only in medicine, but in pharmacy. Dr. I. Hays originated, in this year, the Cyclopædia of Practical Medicine, to which some of the best writers in our country contributed articles. Dr. James Jackson, Jr., published also, in 1832, a small volume on Cholera, made up of cases which he had witnessed in the Paris hospitals.

the action of the gastric juice upon it noted. The results of his numerous experiments and observations, in many of which he had the aid of Dr. Dunglison, are given by Beaumont in a volume which must remain in all coming time an authority on the subject of digestion. Some of the facts first established in the case of his patient will find a place in every treatise on physiology.

With this valuable monograph, another work appeared in 1833, most creditable to the literature of medicine in our country. This was the medical dictionary of Dr. Dunglison, which has gone on increasing in popularity with the profession from year to year. It was at first issued in two volumes, and contained biographical notices of many of the most eminent medical men of the world, but this feature disappeared in the second edition, the author finding that the biography would swell his work to an inconvenient size. Each successive edition has increased the value as much as the bulk of the dictionary, which it is safe to say stands now unrivalled in the English language. Dr. Dunglison's son, fortunately for the profession, has inherited the gifts which rendered his father the first of medical lexicographers, and promises to maintain the work in its present high position during at least another generation.

Dr. E. Geddings, of the University of Maryland, set on foot another periodical, the same year, the Baltimore Medical and Surgical Journal and Review, but though among its correspondents it numbered such writers as T. R. Beck, N. Potter, and N. R. Smith, it fell through after a few months. Dr. Samuel Metcalf, this year also, added something to our literature in the shape of a small volume presenting a new theory of terrestrial magnetism. Magnetism, he held, was but another form of caloric, which, passing from south to north, gave the directive force to the needle. Ten years later, and after much investigation of the subject abroad, he published, in London, a second and much larger edition of his book. It cost the author, indeed, a vast amount of labor, which was but poorly requited either in fame or money, while the advancing years of his life were embittered by disputes with contemporary scientists about his claims to priority of discovery.

In 1834 numerous works were contributed. Dr. J. R. Coxe started an "Inquiry into the Claims of Harvey to the discovery of the Circulation of the Blood," in which he succeeded in showing that in this memorable instance, as in all cases before and since, the truth was not reached at once by the intuition of a single mind, but by the efforts of a succession of inquirers. Dr. Frost, of the University of South Carolina, published a volume of lectures on Materia Medica. Dr. Wm. W. Gerhard made a valuable publication on the cerebral affections of children, which he followed, in 1836, by a volume of lectures on the diseases of the chest, and, in 1837, by a clinical guide for students.[1] A work of much merit on Pulmonary Consumption was produced, in 1834, by Dr. Samuel George Morton, whose labors in other fields subsequently made his name illustrious in science. In 1849, he brought out a work on human anatomy, but, in 1839, he had published his great work, entitled "Crania Ameri-

[1] Dr. Gerhard's treatises were founded on clinical observations, from which they derived unusual interest. He did much for the education of his young countrymen in physical diagnosis of diseases of the chest; and was the first, or among the first, to point out the distinction between typhus and typhoid fever. He made the diagnosis unequivocal. He also called attention to the relation of hydrocephalus, in infants, to tubercular meningitis. Few more successful investigators than Dr. Gerhard have labored in our country to advance medicine.

cana." His "Crania Ægyptica" followed in 1844. His collection of crania enabled him to compare the cerebral capacity of most of the nations of the earth, thus furnishing data from which physiologists are able to draw interesting conclusions.[1]

Dr. James Jackson, in 1835, published a memoir of his lamented son, Dr. James Jackson, Jr., who gave promise of great distinction in his profession; and, in 1838, he reported a number of cases of typhoid fever, which occurred in the Massachusetts hospital; a paper which Dr. Bartlett found of the greatest service in the preparation of his work on fevers. In 1835, Dr. Dunglison published his scholarly work on Hygiene; Dr. John P. Harrison collected and published a volume of Lectures and Essays on Medical Subjects, which had previously appeared in another form; and Dr. John B. Beck issued a volume of Researches in Medicine and Medical Jurisprudence. A volume consisting of papers read before various learned bodies was also issued, by Dr. Richard Harlan, entitled Medical and Physical Researches, or Original Memoirs; it embraced valuable contributions to Zoology and Comparative Anatomy. But the most original work of this year, and, in its effects, far the most important, was the paper read before the Massachusetts Medical Society, by Dr. Jacob Bigelow, on Self-limited Diseases. The conception of such a class of affections did not originate with the author, but its existence was never so fully recognized before he wrote. The recognition has effected a revolution in the treatment of many complaints, while the class in which the principle obtains has been steadily extending since he called professional attention to it. Another work of this year, which, though a compilation, claims a favorable notice, is the First Lines of Physiology, by Professor Daniel Oliver, of Dartmouth College. It is an admirable digest of the facts of the Science. The Author was in advance of the Physiologists of his day in ignoring the pretensions of Phrenology, and in admitting the truth of certain phenomena in animal magnetism which rest, as Sir W. Hamilton expresses it, "upon a tenfold superfluous evidence."

From Dr. Dunglison's prolific pen another work appeared the following year; his General Therapeutics, or Principles of Medical Practice, with tables of the chief remedial agents and their preparations, and of the different Poisons and their Antidotes. But as it was a production of the closet, and not the result of clinical observation, though it was written by a profound and judicious scholar, it never took rank with his Physiology, or his Dictionary. Dr. John C. Warren this year published an elaborate treatise on tumors. During the year, an interesting prize essay, on the Physical Signs in Diseases of the Abdomen and Thorax, was published by Dr. R. W. Haxall, of Richmond.[2]

[1] Dr. Togno, in 1834, published a work on the Anatomy and Physiology of the Ear. Dr. Casanova published some observations on cholera morbus. Drs. Ogier and Logan issued a compend of operative surgery, and Dr. Edward Barton, who had appeared as an author in 1828, wrote a treatise on yellow fever.

[2] Dr. Caleb Ticknor, of New York, published, this year, a popular volume in Harper's Family Library, on the Philosophy of Living, taking for his motto the wise saying of Bacon, that "A man's own observation, what he finds good of, and what he finds hurt of, is the best physic to preserve health." And a volume was issued by Dr. Parrish, of Philadelphia, entitled Practical Observations on Strangulated Hernia, based on a large experience. Two new journals also appeared this year—the Electic Journal of Medicine, afterwards the Bulletin of Medical Science, projected by that indefatigable writer, Dr. John Bell, and the Southern Medical and Surgical Journal, at Augusta, Ga., edited by Dr. M. Anthony and Dr. Joseph A. Eve. Dr. Luther V. Bell, of New Hampshire, published this year some Observations on certain obscure and undecided Doctrines in relation to Smallpox, Varialoid, and Vaccination. Dr. George Bushe, of New York, in 1837, wrote a treatise on the

In 1838, also, a large number of works was issued, first among which, in ability and in the impression it produced, was the Philadelphia Practice of Midwifery, by Dr. Charles D. Meigs. Another was the Medical Jurisprudence of Insanity, by Dr. Isaac Ray, which treats ably of a subject too little understood by physicians. The lectures of Dr. Hosack on the Theory and Practice of Physic, edited by Dr. Henry W. Ducachet, were published this year, as were also Lectures on Lithotomy, by Dr. A. H. Stevens. Dr. Charles Hooker, too, produced a well written Essay on the Relations between the Respiratory and Circulatory Functions, while Dr. Tripler wrote on Recruiting, and Dr. J. W. Monette on Yellow Fever in New Orleans and Natchez; and the Philadelphia Medical Examiner, edited by Drs. Biddle and Clymer, was originated.

Dr. Henry I. Bowditch, the succeeding year, made an important communication in reference to thoracentesis in chronic pleurisy; an operation much opposed at first, but now generally conceded to be legitimate. It has been performed by Dr. Bowditch 325 times, and on 204 patients. To this valuable contribution to our literature, that author has added some interesting statistics relating to the influence of a wet soil on pulmonary consumption. Dr. Dunglison came out this year with still another book, entitled New Remedies, which, like all the productions of his pen, was marked by industry, sound judgment, and a clear English style. Dr. Samuel D. Gross, then a professor in the Cincinnati Medical College, produced a work of much labor on Pathological Anatomy, the first in our language to present a systematic view of the science, and useful in directing the attention of students more generally to that important study.

In 1840 the following works appeared: Dr. B. B. Shober, of Charleston, issued an Essay on Yellow Fever, designed to prove the transmissibility of the disease; and Dr. Upham wrote on Disorders of the Mind; but the great work of the year was from a professor in the University of New York, Dr. Martyn Paine, entitled Medical and Physiological Commentaries. Hardly a work issued from the medical press of America will compare with these ponderous volumes in the learning or in the tedious prolixity with which they are composed. Holding resolutely to an exploded medical philosophy, the author was born too late to command the reverence which his great erudition would have excited in an earlier age. His elaborate and exhaustive commentaries were followed, some years later, by a volume on Materia Medica, characterized by all their merits and all their defects. His volumes are works for teachers and writers, rather than for practitioners or students of medicine. Dr. Edward Warren published, this year, two Boylston Prize Essays, on Scrofula, Rheumatism, and Erysipelatous Inflammation. The Western Journal of Medicine and Surgery was started at Louisville. It succeeded to the Louisville Journal of Medicine, edited by Drs. Miller, Yandell, and Bell, two numbers of which were issued in 1838, and to the Western Journal of the Medical and Physical Sciences, thirteen volumes of which had been completed by Dr. Drake at Cincinnati. It was edited by Dr. Daniel Drake and Dr. L. P. Yandell, with whom, after the second year, Dr. T. W. Colescott was associated, and on the retirement

Malformations, Injuries, and Diseases of the Rectum. The American Intelligencer and Medical Library was set on foot the same year, by Dr. Dunglison. In it were republished many valuable English works on medicine. Dr. Doane also issued this year an illustrated Surgery, and Dr. Dunglison added his Medical Student or aids to the Study of Medicine.

of Drs. Drake and Colescott, some years later, Dr. T. S. Bell became one of its editors.

The only publications in 1841, of which I have been able to find any account, were a volume of Essays on Fevers, by Drs. Davidson and Hudson, and a Practical Dictionary of Materia Medica, by Dr. John Bell.

The following year, Dr. Elisha Bartlett, who had made himself known as one of the most elegant of our writers by his papers in the medical journals of the day, published his well-known work on Fevers. It was succeeded in 1844 by his Philosophy of Medical Science, and in 1848 he added his Inquiry into the degree of certainty in Medicine; three volumes which, for grace of manner and philosophical breadth of view, will not suffer by comparison with any medical works in our language. Dr. Samuel Forry also contributed to our literature, in 1842, a volume which attracted the attention and elicited the praise of the great Humboldt—his work on the Climate of the United States and its endemic influences. It will remain a lasting monument to the genius and industry of the lamented author. While preparing this treatise for the press, he also set on foot the New York Journal of Medicine and the Collateral Sciences, to which he continued to contribute largely, though with failing powers, until removed by death. The name of this young physician ought not to be passed over without at least a word of respectful commemoration. He was "learned, without any fondness for display; a lover of exact detail, but always searching for principles in the facts he accumulated so largely; a statistician, and yet a fluent writer. How entirely his devotion to his pursuits prevailed over the common weaknesses and interests by which most men are influenced, was shown by the fearless readiness with which he threw himself into the focus of a pestilential disease for the sake of studying its nature and causes, as a mere episode in a pleasure trip which he had undertaken to recruit his exhausted forces."[1]

In 1843, a work was produced at Louisville by Dr. W. A. M'Dowell, styled a "Demonstration of the Curability of Consumption in all its stages;" and the same year the St. Louis Medical and Surgical Journal was established by Dr. M. L. Linton. A work of great merit was also published by Dr. Gross, of the University of Louisville, entitled "An Experimental and Critical Inquiry into the Nature and Treatment of Wounds of the Intestines," the result of a series of experiments which he had performed on dogs.

In 1844, numerous works of real value appeared. The following is a list: A Treatise on the Diseases of Children, by Dr. D. Francis Condie; a quarto volume on Operative Surgery, by Dr. Joseph Pancoast; an Atlas illustrative of the structure of the human body, prepared with great care by Dr. Henry H. Smith; a treatise on the Teeth, by Dr. Paul B. Goddard and Joseph E. Parker, a practical Dentist of Philadelphia; an ingenious essay by Dr. John Harrison, of the University of Louisiana, towards a correct Theory of the Nervous System; two volumes of lectures, by the veteran teacher Dr. Nathaniel Chapman; and the American Journal of Insanity. Few physicians in our country have used their pens more industriously or more profitably than Condie, whose work on Diseases of Children was accounted, when written, the most learned and judicious that had appeared on the subject in the English

[1] O. W. Holmes, Report Med. Literature, Trans. Am. Med. Assoc., 1848.

language. The great fame of Chapman as a writer and teacher gave currency to his volumes. Dentistry, which was already making rapid progress, both as a science and an art, in our country, received a favorable impetus from the work of Goddard and Parker. The splendid work of Pancoast secured him a reputation wherever surgery was cultivated. As a guide to the practical surgeon, it was of the highest value, each operation being illustrated by drawings representing the anatomy involved.

The first work by an American, on Diseases of the Skin, appeared in 1845, and was written by Dr. N. Worcester, of the Cleveland Medical College. Other valuable works were issued at the same time, one of which was a volume of essays on Pathology and Therapeutics, by Dr. Samuel H. Dickson of the Medical College of South Carolina, one of the most accomplished of American authors.[1] Another addition was also made this year by a western teacher of medicine to our works on Materia Medica; Dr. John P. Harrison, of the Medical College of Ohio, wrote a treatise on that subject in two volumes, earnestly inculcating "Solidism." "In the discussion of the action of medicines, we have endeavored," he says, "to place the whole question on a footing conclusively vital." He discusses blood-letting at great length. His classification is recommended by its simplicity, all his remedies being embraced in six classes. The work is written in a diffuse style, and was recommended more by its judicious therapeutics than by its scientific teachings.

A volume of Lectures, by Dr. Chapman; a work on Fevers, by Dr. Meredith Clymer; a treatise on Scrofula, by Dr. Phillips; the Young Stethoscopist, by Dr. H. I. Bowditch; an Epitome of the writings of Hippocrates and Galen, prepared by Dr. John Redman Coxe; a valuable memoir by Prof. Robert Peter, containing an analysis of the calculi in the museum of Transylvania University, with remarks on the relative frequency of calculous affections at Lexington, and the probable causes; and First Principles of Chemistry, by Prof. Benjamin Silliman, Jr., who was fast succeeding to the fame of his father, were some of the contributions to our literature in 1846.[2] Dr. Lewis C. Beck, in 1846, wrote on the Adulteration of various substances used in Medicine and the Arts, with the means of detection; and a practical Treatise on Ventilation was also published by Dr. Morrill Wyman.

The discovery of anæsthesia renders this year memorable in the history of our profession. Towards its close, Dr. Henry J. Bigelow read before the Boston Medical Improvement Society, a paper in which he detailed the facts concerning the great event that had fallen under his observation, and vouching for the efficacy of sulphuric ether in annulling pain in surgical operations. This first communication on the subject appeared in the Boston Medical and Surgical Journal, Nov. 18, 1846, and is noteworthy as announcing to the world one of the noblest discoveries in Medicine.

[1] Dr. G. R. B. Horner added an interesting volume on the Medical Topography of Brazil and Uruguay, and Dr. Stephen W. Williams contributed one on the Medical Biography of the United States. The Buffalo Medical and Surgical Journal, under the able editorial direction of Dr. Austin Flint, was established. Dr. Horner had in 1839 published a work on the Mediterranean, and in 1854 he contributed one on the diseases and injuries of seamen. Dr. J. H. Griscom published in 1845 an instructive little work on the Sanitary condition of the laboring Population of New York.

[2] Dr. Peter also contributed to Medical Science by his Analysis of the Mineral Springs of Kentucky, contained in the geological reports of that State.

The meeting of physicians at New York, which resulted in the organization of the American Medical Association, is another event of this year, which marks an era in the history of Medicine in the western hemisphere. The effect of the Association upon the profession has been in every way beneficent, elevating its tone, promoting harmony among its members, and stimulating and improving its literature. Few movements of the century to which these papers refer have contributed so much to the advancement of American Medicine.

Dr. Horace Green, in 1846, startled the profession by a work on the Diseases of the Throat and Air-passages, proposing to introduce remedies into the larynx; but his bold practice has been justified by subsequent experience, and he is now admitted to have been a pioneer in this department of medicine, and to have led the way to what others with better instruments have accomplished.

Numerous works apeared in 1847, conspicuous among which was the learned treatise of Dr. George B. Wood on the Practice of Medicine. It had the advantage over all its predecessors in that department, of being founded on clinical observation, and as a consequence was soon adopted as the text-book in our schools, and became the popular work of reference with practitioners. Dr. Charles D. Meigs issued another of his works, written in his peculiar, somewhat fantastic style—his "Woman; her Diseases and Remedies," with the effect of increasing the growing interest of the profession in Gynæcology. Dr. Joseph Carson commenced the issue of a series of colored "Illustrations of Medical Botany," and Dr. R. Eglesfeld Griffith produced a practical volume on the same subject. A most valuable contribution was made to pathological anatomy by Dr. J. B. S. Jackson, in his Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement. In 1847 Dr. David H. Tucker produced a small volume entitled Elements of the Principles and Practice of Midwifery.[1]

In 1848, a volume of Transactions was issued at Baltimore by the American Medical Association, which had been organized at Philadelphia the year before. A volume has been sent out by the Association annually since, and while it would be easy to point out defects in them all, it will not be denied by any unprejudiced reader that they form a valuable body of medical literature. All that comes before each meeting, or nearly all, appears in the Transactions, and it follows that much is published which if submitted to a rigid criticism would never see the light. But the series embraces many strong, original, and truly valuable papers from the best minds in the profession, and, apart from the diffusion of knowledge thence directly resulting, the effect of inviting physicians everywhere to write for the Association has been to develop and improve professional talent in a way that nothing but such a society could have done.

The following works also came out in 1848: A volume by Dr. Alfred Stillé, on General Pathology, which possessed great merit, both as to its

[1] Dr. H. H. Smith also published this year a Minor Surgery; and Dr. Edward Jarvis contributed a treatise on Physiology for the Use of Schools and Families. Dr. J. W. Francis delivered an admirable anniversary address before the New York Academy of Medicine, which was published; Dr. John Ware published an able address on Medical Education. "Water versus Hydropathy" appeared, a practical essay on Water in its relations to Medicine, by Dr. Henry Hartshorne. Dr. John Neill published Outlines of the Veins and Lymphatics; and Dr. J. J. Reese a small work on Physiology. The Dental Register of the West also came out this year.

matter and the style of its execution; a practical and excellent Treatise on the Diseases of Children, by Dr. J. Forsyth Meigs; the learned lectures on the Theory and Practice of Physic, of Drs. Bell and Stokes; a treatise on Etherization, by Dr. Walter Channing, embodying his experience of anæsthesia in midwifery; a work of much originality on Croup, by Dr. Horace Green; the ingenious essay on the Cryptogamous Origin of Diseases, by Dr. J. K. Mitchell, a man of genius as well as of learning and large experience; a posthumous volume on Surgery, by the gifted Dr. George McClellan, of the Jefferson Medical College, edited by his son, Dr. J. H. B. McClellan; Observations on Diseases of the Chest, and on Auscultation, by Dr. Blakiston; Dr. Pliny Earle's interesting history of the Bloomingdale Asylum for the insane; and two new journals of Medicine, the Charleston Medical Journal and the Northwestern Medical and Surgical Journal.[1]

The additions to our literature in 1849 were hardly less numerous than those of the preceding year. Dr. Meigs produced a treatise on Obstetrics; and one on the same subject appeared from the pen of Dr. Henry Miller, of the University of Louisville. Both works possessed great merit, as the productions of independent, vigorous minds long turned upon that branch of medicine. Dr. E. D. Fenner issued a volume of Southern Medical Reports, full of original observations on the diseases of the Southern States, which was followed by a second volume in 1851. Dr. Worthington Hooker produced his "Physician and Patient," presenting a sensible view of the mutual duties, relations, and interests of the medical profession and the community; and Dr. J. W. Powell contributed a work on the eye and its diseases. Equal in merit to any of these, and superior to most, was a volume of Essays, by Dr. John B. Beck, on Infantile Therapeutics, to which are added observations on ergot, and an interesting account of the origin of the use of mercury in inflammatory complaints.[2]

The contributions of 1850 embrace some of the ablest made to our literature by the American medical profession. They are the following: a Treatise on the Diseases of the Great Interior Valley of North America, by Dr. Daniel Drake; Observations on certain of the Diseases of Young Children, by Dr. C. D. Meigs; a work on Materia Medica and Therapeutics, by Dr. Thomas D. Mitchell; the Principles of Medical Jurisprudence, by Amos Dean, Esq., of the Albany Medical College; a work on the Diagnosis and Pathology of Renal diseases, by Dr. Charles Frick; a History of Medical Education and Institutions in the United States, by

[1] A small volume on Etherization, with surgical remarks, by Dr. John C. Warren, was contributed to the literature of that subject: and an absurd work, by Dr. J. W. Hood, of Kentucky, was issued setting forth a theory that all abdominal diseases are caused by displacement of the abdominal organs, and curable by pads, trusses, and supporters. In addition to these, the following also appeared that year: a small volume on Bandaging and other operations in Minor Surgery, by Dr. F. W. Sargent; a Compendium of the various branches of medical science, by Drs. J. Neill and F. G. Smith; a Handbook of Surgery, by Dr. J. Neill; Lectures on Yellow Fever, by Dr. John Hastings, U. S. A.; a Medical Chemistry for the use of Students, by Dr. D. P. Gardner; an essay on epidemic meningitis, by Dr. S. Ames; and a dictionary of dental surgery and medical terminology, by Dr. Chapin Harris.

[2] The following also appeared in 1849: a small volume on Epidemic Cholera, by Dr. C. B. Coventry; a Treatise on the Diseases of the South, by Dr. G. McGowan; Surgical Essays and Cases, by Dr. D. L. Rogers; and an Essay on Respiration and its effects, by Dr. E. Willard. The Transylvania Journal of Medicine, which had been discontinued, was revived by Dr. Ethelbert L. Dudley for a short time. A paper on Aneurism, by his illustrious uncle, Dr. B. W. Dudley, enriched one of the early numbers of the journal.

Dr. N. S. Davis; a work on the Practice of Surgery, by Dr. J. Hastings; a work on Mental Hygiene, by Dr. Wm. Sweetzer; a Practical Treatise on the Diseases and Injuries of the Urinary Bladder, by Dr. Samuel D. Gross; and a work on Sleep, psychologically considered, with reference to Sensation and Memory, by Dr. B. Fosgate. To this long list must be added a history of Typhoid Fever, as it appeared in Georgetown, Kentucky, by Dr. W. L. Sutton; the American Medical Formulary, by Dr. J. J. Reese; an Encyclopædia of Chemistry, by Dr. Jas. C. Booth; and a treatise by Dr. Thomas E. Bond, on Dental Medicine, which is a compendium of Medical Science as connected with the study of Dental Surgery. In the same year four medical journals were started: the New Hampshire Journal of Medicine, at Concord, edited by Dr. H. Parker; the Western Medico-Chirurgical Journal, at Keokuk, edited by Drs. J. F. Sandford and S. G. Armor; the Stethoscope and Virginia Medical Gazette, at Richmond, edited by Dr. F. C. Gooch; and the Nashville Journal of Medicine and Surgery, edited by Dr. W. K. Bowling.

This list embraces several works of great labor and uncommon merit. The Observations of Meigs, like all his other contributions to Medical Science, abound in original views, at the same time practical and judicious. The work of Sweetzer is suggestive and ingenious; that of Gross, elaborate and exhaustive; but foremost among them all, in research, in breadth of scientific view, and in the wide scope of observations recorded, is the treatise of Drake. The author had planned his work when a young physician, even before taking his degree in Medicine, and had been collecting materials for its composition during the term of an ordinary lifetime. It is not made up, as so many books in all ages have been, of matter borrowed from other books, but of facts collected by himself, in a long experience, during protracted journeys, and from original observers. In the accomplishment of his design he traversed the whole Mississippi Valley, tracing the causes of its diseases from the Northern Lakes to Cape Sable. He sought to indicate in his treatise whatever there was that was distinctive in the complaints of the region of which he was writing, as induced by climate, race, mode of living, and the other agencies that modify disease. The result of his extended researches was given in two ample volumes, the second of which was published in 1854, two years after his death. That in every sense the production is a great one, has been conceded by all writers who have noticed the work; and yet it never acquired general popularity, and is now comparatively disregarded. A quarter of a century has passed away since it appeared, and no second edition has been called for by the profession. It is not of a form to be used as a text-book by students, and it must be owned that it is not quite suited to the practitioner as a book of reference. But it is a vast repository of facts, upon which all future writers must hereafter draw who would give an account of the climate, topography, and diseases of the great valley to which it relates. It can hardly be doubted that it belongs to a class of works, once described by Dr. Drake himself, that "lie embalmed in the truths with which they are penetrated, and cannot decay: but like the bodies of kings and philosophers steeped in spices, and deposited in the catacombs, are found only in unfrequented closets and alcoves of libraries."

The most important works published in 1851 were a volume of lectures on Materia Medica, by Dr. John B. Beck, prepared for the press after his death, by his friend, Dr. R. C. Gilman; and a system of Operative Surgery, by Dr. H. H. Smith, based on the practice of the surgeons of

the United States, and comprising a bibliographical index and historical record of many of their operations during a period of two hundred years. Besides these, a history of the Massachusetts General Hospital was contributed by Dr. N. O. Bowditch.[1] Dr. M. L. Linton, of the University of St. Louis, also made this year a contribution which is worthy of notice, his Outlines of General Pathology; an unpretending little volume, but abounding in sound medical philosophy, and written in a lucid, vigorous style.

Dr. Austin Flint, in 1852, commenced a series of publications, the result of his observations of disease in hospitals and private practice, which have placed him at the head of American writers on practical medicine. That year he issued a volume of Clinical Reports on Continued fever, based on an analysis of 164 cases. The same year, Dr. R. U. Piper published an elaborate work on Operative Surgery, containing more than 1900 engravings, with explanatory text; to which is appended an interesting chapter on the use of Ether in Surgery, written by Dr. Henry J. Bigelow. This year, too, was produced a volume of essays written in a charming style, by Dr. S. H. Dickson, on Life, Sleep, and Pain.[2]

Dr. William H. Boling, of Alabama, whose various articles on malarious fevers in the periodicals of the day had given him a wide reputation, published, in 1853, an instructive essay on the mechanism and management in Parturition of shoulder presentment. In the same year Dr. Joseph Warrington contributed an Obstetric Catechism, containing 2347 questions and answers on obstetrics proper. Dr. C. E. Brown-Séquard commenced in 1853 a series of publications of great interest to physiologists and practitioners. The first was a small volume of experimental researches applied to physiology and pathology. A smaller volume followed in 1855, on the physiology and pathology of the spinal cord; and in 1857 another thin volume appeared, entitled Researches on Epilepsy. In 1860 he published a volume of Lectures on the Physiology and Pathology of the Central Nervous System, delivered at the Royal College of Surgeons of England; and in 1868 this was followed by another volume of Lectures on the Diagnosis and Treatment of functional nervous affections.

In 1854, a volume of substantial merit was produced by Dr. René La Roche, the title of which describes its character: Pneumonia, its supposed connection, pathological and etiological, with autumnal fevers, including an inquiry into the existence and morbid agency of malaria. It is a monograph of enduring interest. Other works that enriched our literature also appeared in that year, among which the following are

[1] Dr. Charles Delevy wrote De la Fièvre pernicieuse de la Nouvelle Orléans, following this history, in 1859, with a Précis historique de la Fièvre. Dr. F. B. Flagg published a work on Ether and Chloroform.

[2] The following works were also published: A Treatise on Diseases of the Chest, by Dr. John A. Swett; Records of Maculated Typhus, or Ship Fever, by Dr. J. B. Upham; a Treatise on Uterine Displacements, by Dr. W. E. Cole; Analysis of Physiology, by Dr. J. J. Reese; Outlines of the Arteries, by Dr. John Neill; and a description of a skeleton of the mastodon giganteus, by the veteran anatomist and surgeon, Dr. John C. Warren. Dr. A. Clapp, of New Albany, a man of great attainments in general science as well as in medicine, contributed a valuable synopsis or systematic catalogue of the medicinal plants of the United States, to the American Medical Association, which appeared in its Transactions for that year, and was afterwards issued in a separate volume of more than 200 pages. In 1852 two periodicals were published in New Orleans—the New Orleans Medical Journal, and the Monthly Medical Register. Since the war, the former has been revived, and is ably conducted by Dr. Samuel M. Bemiss.

conspicuous: a treatise, by Dr. Gross, full and exhaustive, on Foreign Bodies in the Air Passages; Dr. T. G. Richardson's Elements of Human Anatomy, in which the author, as far as he could, has substituted English for the Latin terminology—a manual admirably adapted to the dissecting room; Dr. Bennett Dowler's "Tableau of Yellow Fever" as it prevailed in New Orleans in 1853, with topographical, chronological, and historical sketches of the epidemics of that city since their origin in 1796; and a history of the yellow fever of 1853, in New Orleans, by Dr. E. D. Fenner.[1] Dr. Meigs also added to his other valuable works, this year, a volume written with great force, on the nature, signs, and treatment of childbed fevers, contending against their transmission by contact. And Dr. Jacob Bigelow laid the profession under additional obligation by a volume entitled "Nature in Disease, illustrated in various discourses and Essays," recommending by conclusive arguments the conservative medicine, toward which the professional mind of the age is so strongly tending. A volume was published in 1854, by Dr. A. S. Piggott, entitled Dental Chemistry and Metallurgy, which was of much use to practitioners of Dentistry.

The publications of 1855 were, many of them, of very great excellence and interest. The Letters of Dr. James Jackson to a Young Physician, in a small volume, to which he added a second series in 1860, are adapted to the wants of the inexperienced of every age, and may be read with profit by all young practitioners. A small work, by Dr. Hugh L. Hodge, on criminal abortion, discusses a subject, the interest of which is likely to increase with time. The treatise on Medical Jurisprudence, by Francis Wharton, Esq., and Dr. Moreton Stillé, stands on a level with the great work of Beck. The joint production of a learned jurist and an able physician, it possesses merits not likely to be found in a work by a single hand. It is a noble monument to the memory of its medical contributor, who died almost at the moment when the last sheets of his work were issuing from the press. Gifted, cultivated, and laborious, Stillé, in his short life, accomplished a work of which his profession is justly proud, and which only increases the regret at his early death. The monograph on the Diseases affecting the Respiratory Organs, by Dr. Flint, was in the line of his work on Continued Fever, and worthy of it. The tract of Dr. O. W. Holmes, entitled Puerperal Fever as a Private Pestilence, presents strong reasons for believing in the contagiousness of the disease. It was pronounced by Ramsbotham, when it came out, "a masterly performance." Dr. Zina Pitcher, in a paper on the Induction of Puerperal Fever by Inoculation, so called, takes the opposite side of the question, which he supports by a great array of facts. An Army Meteorological Register, for twelve years, originating from an office suggested by Hon. J. C. Calhoun, while Secretary of War, and prepared under the direction of Surgeon-General Thomas Lawson, is of value. Dr. E. R. Peaslee wrote this year a valuable work on Croup, and a work for students on Chemistry was issued by Dr. B. Howard Rand.

But the great work of this year was that on Yellow Fever, by Dr. La Roche. The American press has given birth to no original work superior to it in research or learning. As a monograph on one of the most

[1] To this list must be added a text-book of Anatomy, for the use especially of students of Dental Surgery, by Dr. R. Handy, of the Dental College of Baltimore; a volume of Lectures on pulmonary consumption, by Dr. T. Thompson; Letters on Yellow Fever, Cholera, and Quarantine, by A. F. Vaché; and a work on Human Physiology, by Dr. Worthington Hooker, designed for Colleges and general reading.

formidable of the epidemics that afflict our race, it may be doubted whether anything more exhaustive is contained in any language. It was the production of a laborious student, a thorough medical scholar, and an amiable and accomplished gentleman.

In 1855, the Atlanta Medical and Surgical Journal, under the care of Drs. Westmoreland and Batty, was set on foot, and in the same year a History of the American Medical Association was written by Dr. N. S. Davis.[1]

The following year was also productive of valuable additions to the stock of our literature. Professor Edward Parrish, of the Philadelphia College of Pharmacy, wrote an introduction to practical pharmacy, which was useful in that department of medicine; an original and most able work on Human Physiology, statistical and dynamical, was contributed by Dr. John W. Draper, who before had written a remarkable treatise on the forces which produce the organization of plants, in which he proposed a simple and ingenious theory of the capillary circulation. Dr. George B. Wood communicated the results of his matured researches concerning medicines, in his able treatise on Therapeutics and Pharmacology, or Materia Medica, which has long been an authority in the profession. A prize essay of great interest was published by Dr. George H. Lyman, giving the history and statistics of Ovariotomy, and the circumstances under which the operation may be regarded as safe and expedient. A Treatise on Surgery, the result of much labor, by Dr. H. H. Smith, was added to our literature on that subject. A history of the medical profession in ancient times was contributed by Dr. John Watson. Dr. James Stewart produced a practical treatise on the Diseases of Children. Dr. Isaac Taylor, in a short paper, called the attention of the profession to the sun-burnt appearance of the skin as an early diagnostic symptom of supra-renal capsule disease. Dr. J. M. Allen added another to the list of our anatomical works, in his Practical Anatomist. The North American Medico-Chirurgical Review, which originated in Louisville, but was shortly afterward transferred to Philadelphia, was added by Drs. Gross and Richardson to our long list of medical journals.[2]

The question of the original unity of the human race has been a good deal discussed in our country since the appearance of Dr. S. S. Smith's work on the subject, and in 1857 a treatise of much bulk and labor was contributed by Drs. Nott and Gliddon, On the Indigenous Races of the Earth, of the merits of which various opinions have been expressed, according to the preconceived notions of the writers. Dr. E. R. Peaslee in this year published a work on Human Histology, to which, in 1872, he added one of great interest, on Ovarian Tumors. Dr. Paul F. Eve did the profession a service by collecting into a volume many Remarkable Cases in Surgery, showing into what errors surgeons of experience and

[1] An Essay on Malignant Cholera, by Dr. B. M. Byrne; the Practitioner's Pharmacopœia, by Dr. J. Foote; a Discourse, by Dr. J. H. Griscom, on the relation between the people and the Science of Medicine; and a volume, by Drs. Comstock and Comings, on Physiology, chiefly taken without acknowledgment from the writings of Carpenter, were also among the publications of 1855.

[2] Dr. Joseph Jones published in this year some investigations relative to American Vertebrata; and Dr. M. L. Knapp wrote an interesting inquiry into the cause of Nurse's Sore Mouth, and in the following year produced a rambling treatise, styled Researches as to the Pathology and Origin of Epidemics, tracing all to the existence of a scorbutic taint. Dr. Benjamin Haskell also published this year some ingenious Essays on the physiology of the nervous system, with an appendix on hydrophobia, and the Medical and Surgical Reporter, under the editorial care of Dr. S. W. Butler, took its place among our periodicals.

skill may fall, and under what extremities patients may sometimes recover. This year, also, was published an interesting Catalogue of Crania, by Dr. J. Aitken Meigs. But a more important contribution to our literature was made by Dr. H. F. Campbell, in an essay to which a prize was awarded by the American Medical Association, On the Excito-Secretory System of Nerves. In the views advanced on this subject, Dr. Marshall Hall admitted that Dr. C. had anticipated him, as well as M. Claude Bernard.

The succeeding year two large octavo volumes on Materia Medica, full of useful matter, were issued by Dr. William Tully; a text-book on vegetable and animal physiology was produced by Dr. H. Goadby, and an interesting monograph on Scarlatina, by Dr. Caspar Morris, appeared in a new dress, having been first issued some years before; a work, too, on the diseases of the urinary organs, which was followed by one, in 1861, on the effects of the retention of the elements of urine in the blood, was published by Dr. W. W. Morland.[1] A volume of lectures on Surgery, delivered by Prof. E. Geddings, of the Medical College of Charleston, was published in 1858. It is to be regretted that the learned and able author has not followed up his work.

In 1859, the treatise of Dr. John C. Dalton, on Physiology, which still maintains its place as a text-book in our schools, was published, and is not likely to be soon superseded. It is a work which has advanced physiological science, and in which the student finds a condensed but lucid summary of its truths, communicated in a pleasing style. With it appeared another work which has conferred lustre upon the medical literature of our country, the learned and able System of Surgery, by Dr. Samuel D. Gross. With an industry truly indefatigable, the author has continued to add to its successive editions, until he has made it one of the most thorough systems of surgery to be found in any language. Dr. Austin Flint, the same year, produced a practical treatise on the Diseases of the Heart, which ranks among the high authorities on that subject.

A volume of essays, written by his gifted father, was published this year by Dr. S. Weir Mitchell. In these essays, the author appears as a chemist and philosopher, and shows that, if he had continued his labors, so successfully begun in that direction, he might have attained a high rank among the scientific men of his time. His researches concerning the diffusion of gases gave him a glimpse of some of the most interesting discoveries of which modern chemistry can boast.[2]

In 1860 a number of excellent books were produced, foremost of which in research and learning, as well as in size, is the treatise of Dr. Alfred Stillé, on Therapeutics and Materia Medica. Of the progress of our medi-

[1] Valuable contributions to operative surgery and surgical pathology were made this year by Dr. J. M. Carnochan, and Dr. Horace Green published selections from favorite prescriptions of living American practitioners. The following journals were set on foot in 1858: The Chicago Medical Journal, by Dr. J. Adams Allen; the Cincinnati Lancet and Observer, by Dr E. B. Stevens; and the Cincinnati Medical News, by Dr. J. A. Thacker. A little work was also published by Dr. Jacob Bigelow, entitled Brief Expositions of Practical Midwifery, to which is prefixed the Paradise of Doctors.

[2] Dr. E. J. Coxe wrote, this year, on the Yellow Fever of New Orleans; Dr. V. M. Francis published a Thesis on Hospital Hygiene, and a work styled Contributions to Midwifery; the Diseases of Children was issued by Drs. Noeggerath and Jacobi; Dr. J. J. Moorman wrote an account of the Virginia Springs and the Springs of the South-West, which was of great value to invalids in search of watering-places. A small treatise was published by Dr. John M. Watson, of the University of Nashville, on trismus nascentium, which he traces to irritation of the umbilicus in the young child. Dr. James E. Reeves, the same year, published a Practical Treatise on Enteric Fever.

cal literature during the century, we could hardly point to any evidence more conclusive than that afforded by a comparison of this scientific work with the elementary treatises on the same subject written forty years before. We are quite willing that by it the world shall judge of American Medical authorship. The "Practical Treatise on Fractures and Dislocations," by Dr. Frank Hastings Hamilton, has become a standard authority on the subject.[1] An Elementary Treatise on Anatomy was prepared by the learned Professor of Anatomy in the University of Pennsylvania, Dr. Joseph Leidy. Dr. Henry Hartshorne published "Memoranda Medica," or note-book of medical principles. A very instructive work was issued by Dr. Hugh L. Hodge, on the diseases peculiar to females, including displacements of the uterus. And among the "Contributions" of the Smithsonian Institution appeared an original paper of much interest, by Dr. S. Weir Mitchell, on the venom of the rattlesnake.[2]

Of the publications of 1861, the most important was a work on the principles and practice of Obstetrics, by Dr. Gunning S. Bedford, of the University of New York. But the American Medical Biography, edited by Dr. Samuel D. Gross, has also high claims upon the profession, and ought to be extended. It is a work which must be of interest to every American physician, as a record of the lives of men who have done honor to the profession. Dr. Gross, the same year, produced an excellent manual of military surgery. A "Manual of Military Surgery," was also produced by Dr. J. Julian Chisholm, at Columbia, South Carolina, and was the text-book of the surgeons of the Confederate army during the civil war. And one on the same subject was contributed by Drs. Blackman and Tripler.[3]

The effect of the civil war in repressing our literature, was manifest in 1862, in the course of which very few original books were published. One of these was the Medical History of the Philadelphia Almshouse, by Dr. D. Hayes Agnew; a second was a treatise, by Dr. A. Jacobi, on dentition and its derangements; and a third was a practical guide to diseases of the eye, by Dr. H. W. Williams. Dr. Stephen Smith added an opportune hand-book on surgical operations.

In 1863, the number was also small. They were a Manual of the institutes of military surgery, by Dr. J. Ordronaux; a manual of minor surgery, by Dr. J. H. Packard, and the practice of surgery for field and hospital, by Dr. E. Warren. Dr. J. J. Woodward made a practical and substantial contribution to military medicine, in a volume on the chief

[1] A useful medico-legal treatise on Malpractice was written by Dr. John J. Elwell. Dr. Samuel W. Francis presented to the profession a report of the Clinical Lectures of Dr. Valentine Mott in the University of New York.

[2] A Catalogue of the Pathological Cabinet of New York Hospital, classified and arranged, was published this year by Dr. Robert Ray, Jr., and also an epitome of medicine and surgery, by Dr. W. S. Wells. Dr. Edward Warren, in 1860, published an interesting biography of Dr. John C. Warren. Two new medical journals were also projected: the Pacific Medical and Surgical Journal, edited by Dr. Henry Gibbons, and the Chicago Medical Examiner, edited by Drs. N. S. and F. H. Davis.

[3] Besides these, the following works also appeared in 1861: A Practical Treatise on Phthisis Pulmonalis, by Dr. L. M. Lawson, of Cincinnati; a work by Dr. R. M. Hodges, on Excision of the Joints; one by C. H. Jackson, on Etherization; a sprightly paper on Currents and Counter-currents in Medical Science, by Dr. O. W. Holmes; a work on Placenta Prævia, by Dr. William Reid; and two valuable Essays, by Dr. Fordyce Barker, one on an Effort to shorten the first stage of Labor; the other on the use of anæsthetics in Midwifery. Dr. Edwin R. Maxson also published this year a volume on the Practice of Medicine, and, in 1868, he published an account of Hospitals, British, French, and American.

camp diseases of the U. S. armies as observed during the war of the rebellion.

In 1864, an important work on Gunshot Wounds and Injuries of Nerves was contributed by Dr. S. Weir Mitchell and Drs. Morehouse and Keen; and in this year the following works also appeared: Lectures on Venereal Diseases, by Dr. William A. Hammond; a Medical Dictionary, by Dr. J. Thomas; a paper on the puerperal disease, by Dr. B. F. Barker; a work by Dr. Bartholow on disinfection, which was followed in 1867 by a manual on enlisting and discharging Soldiers, and in 1868 by a work on spermatorrhœa; a prize-essay by Dr. H. F. Damon, on leucocythæmia; a volume of Medical and Surgical Essays, by Dr. W. A. Hammond; a "Mémoire sur la fièvre paludienne," by Dr. J. C. Faget; and an account, by Dr. J. O'Reilly, of the Nervous and venous connection between the Mother and the Fœtus.

The following is a list of the publications in 1865: a Monograph on glycerin, by Dr. H. Hartshorne; the Book of Prescriptions, by Dr. Henry Beasley; a Treatise on Military Surgery and Hygiene, by Dr. F. H. Hamilton; a Vest-pocket Medical Lexicon, by Dr. D. B. St. John Roosa; a Treatise on the Diseases and Accidents incident to Women, by Dr. W. H. Byford; and Elements of Materia Medica for the use of students, by Dr. John B. Biddle. The last two are substantial additions to our practical medical literature. No more eligible text-book than Dr. Biddle's has yet been written; and the volume by Dr. Byford illustrates a class of affections which he has studied with success. To this list we have to add the able work of Dr. W. H. Van Buren on the Surgical Diseases of the Genito-Urinary Organs, and his contributions to Practical Surgery; and the Lectures of Dr. J. H. Packard on inflammation.[1]

Cholera prevailed in the United States in 1866, and a number of works were written concerning the epidemic; but besides these, that year was productive of some of the best contributions made by American writers to medical science. The work of Dr. Austin Flint, on the Principles and Practice of Medicine, is entitled to the first place among our treatises on that subject. It is the production of a practical, able, and patient observer, who speaks from long clinical experience, and is written in a clear, direct, concise style, and with a fulness that leaves nothing to be desired. And by the side of this admirable work deserves to stand the treatise of Dr. Austin Flint, Jr., on Physiology, the first volume of which appeared about the same time, and which has since been completed in five volumes. Few works of equal merit to these, the productions of father and son, have proceeded from American authors. Another publication of that year possessing great value was the treatise of Dr. J. M. Da Costa, on Medical Diagnosis, a work with which our students of medicine can hardly dispense; and one of equal value to the practitioner was that of Dr. J. Marion Sims, entitled Clinical Notes on Uterine Surgery, with special reference to the sterile condition; the first treatise written in our language on the surgery of the uterus.[2]

[1] In this year the following works too appeared: A Sanitary Code for Cities, by Dr. H. G. Clarke; a volume on Hypodermic Injections, by Dr. Antoine Ruppaner; and the New York Medical Journal, edited at first by Dr. W. A. Hammond, and subsequently by Drs. J. B. Hunter and W. T. Lusk.

[2] A treatise appeared, this year, by Dr. J. W. Wright, on Psychology, and one by Dr. S. G. Webber, on cerebro-spinal meningitis. Dr. A. C. Garratt also contributed one on electro-therapeutics, and three new journals of medicine were projected—the Detroit Review of Medicine, by Drs. Conner and Lyons; the New York Medical Record, by Dr. Geo. F. Shrady,

Dr. Henry Hartshorne in 1867 produced a volume which has met with much professional approbation, entitled Essentials of the Principles and Practice of Medicine. A very able work of an original character, on the micro-chemistry of poisons, was produced by Dr. Theo. G. Wormley. Dr. J. Solis Cohen contributed a valuable volume on Inhalation, its therapeutics and practice. One was produced on injuries of the Spine, by Dr. John Ashhurst, Jr., containing an analysis of nearly four hundred cases. Drs. Francis Minot and Charles Homans, Secretaries of the Society, published a volume of Extracts from the Transactions of the Boston Society for Medical Improvement, with Papers read before the Society. The lamented Dr. J. Mason Warren contributed a valuable volume of Surgical Observations, with Cases and Operations. Dr. N. R. Smith wrote on the treatment of fractures of the lower extremities, by the use of the anterior suspensory apparatus; and Dr. John H. Packard published Notes on Fractures of the Upper Extremity. A volume of Researches upon Spurious Vaccination was contributed by Dr. Joseph E. Jones. Dr. Henry J. Bigelow published a practical paper on Ununited Fractures successfully treated; and Dr. B. Howard Rand contributed an elementary work on Chemistry. A work of much labor was issued under the editorial supervision of Dr. A. Flint, entitled Contributions relating to the Causation and Prevention of Disease, and to Camp Diseases, together with a report of the diseases, etc., among the prisoners at Andersonville. A large volume, entitled Mechanical Therapeutics, a treatise on surgical appliances, was produced by Dr. Philip S. Wales, at Philadelphia. A Monograph on Cerebro-Spinal Meningitis, treating ably of the subject, was issued by Dr. Alfred Stillé. Dr. S. H. Dickson made a valuable contribution to Studies in Pathology and Therapeutics; and an interesting work on the Medical Use of Electricity was produced by Drs. George M. Beard and A. D. Rockwell. To which long catalogue of publications for that year we have to add the Leavenworth Medical Herald, by Dr. Tiffen Sinks, and the Medical Archives, by Dr. J. C. Whitehill, published at St. Louis.[1]

In 1868 three important works were produced on the diseases and accidents incident to females; an Obstetric Clinic, by Dr. George T. Elliot; a Practical Treatise on the Diseases of Women, by Dr. T. Gaillard Thomas; and a work on Vesico-vaginal Fistula, by Dr. Thomas Addis Emmet. Dr. Dalton wrote a Treatise on Physiology and Hygiene, for schools, families, and colleges; a volume of Reports of the Pennsylvania Hospital, was edited and issued by Dr. Da Costa and Dr. W. Hunt; and a work on Physical Diagnosis was published by Dr. Alfred Loomis.[2]

and the Richmond (now Richmond and Louisville) Medical Journal, by Dr. E. S. Gaillard, editor and proprietor. This year the following works also were published: Contributions to bone and nerve Surgery, by Dr. J. C. Nott; a small treatise on food and digestion, by Dr. Howard Townsend; Lectures on Orthopedic Surgery, by Dr. L. Bauer; a small treatise on Orthopedics, by Dr. D. Prince, followed, in 1871, by one on Plastics; and works of greater or less length on Cholera, by Drs. J. C. Peters, R. Nelson, E. Whitney, F. A. Burrall, Palmer, Fletcher, and Henry Hartshorne.

[1] Two works on conservative surgery, one by Dr. H. G. Davis, and another by Dr. A. G. Walter, appeared in 1867, and with them a Manual of Examinations, by Dr. J. L. Ludlow; a paper on Epidemic Cholera, by Dr. G. Hurt; a Treatise on Polypus of the Ear, by Dr. Edward H. Clark; a work by Dr. D. Wooster, on Diseases of the Heart, and a Thesis on Aneurism of the Aorta, by Dr. E. Souchong.

[2] Beside these the following also appeared: The Law of Human Increase, or Population based on Physiology and Psychology, by Dr. Nathan Allen; a Practical Anatomy, by Dr. D. H. Agnew, who followed it in 1873 by a work on laceration of the perineum; a Treatise, by Dr. W. Bodenhamer, on Diseases of the Rectum; a Dissertation, by Drs. Edes,

The following list comprises the most noted publications of 1869: A Treatise on the Diseases of Infancy and Childhood, by Dr. J. Lewis Smith; a Conspectus of the Medical Sciences, by Dr. H. Hartshorne; Outlines of Comparative Anatomy and Medical Zoology, by Dr. Harrison Allen; a History of the Medical Department of the University of Pennsylvania, by Dr. Joseph Carson; the Intermarriage of Relations, by Dr. Nathan Allen; a treatise, by Dr. J. S. Lambert, on Longevity; Contribution to the Medical History of the United States, by Dr. J. M. Toner; the Mechanism of Dislocation and Fracture of the Hip, by Dr. H. J. Bigelow; an essay on External Perineal Urethrotomy, by Dr. J. W. S. Gouley; a volume on the Jurisprudence of Medicine, in its relations to the law of contracts, by Dr. Ordronaux; a work, in quarto, on Amputation of the Cervix Uteri in certain cases, by Dr. Isaac E. Taylor; a compend of Materia Medica, by Dr. John C. Riley, and a volume on the physiology and pathology of the sympathetic nervous system, by Dr. Robert T. Edes. In addition to these, a small treatise was written on Veratrum Viride, by Dr. R. Amory; one on physical culture in Amherst College, was contributed by Dr. N. Allen; and one, of much greater size, on the Principles of Naval Staff Rank and its History for over half a century, by a Surgeon in the U. S. Navy. The Archives of Ophthalmology, edited by Profs. Knapp and Moos, and the Oregon Medical and Surgical Journal were projected this year.

The most important contributions in 1870, were the Surgical Memoirs of the War of the Rebellion, which when completed will form the most magnificent work pertaining to medicine that our country has produced; and the Treatise on the Theory and Practice of Obstetrics, by Dr. W. H. Byford, which stands on a level with the best of a class of works in which the profession of America is conceded to have excelled. The Lectures on the Diseases of the Rectum, by Dr. W. H. Van Buren, and the Hand-book of Operative Surgery, by Dr. J. H. Packard are valuable contributions to practical surgery; as are also the Bellevue and Charity Hospital Reports; the Medical and Surgical Reports of the Boston City Hospital, and the work of Dr. J. A. Lidell on wounds of the bloodvessels.[1]

The publications of the Surgeon General's Office were continued in 1871, and some very able works appeared with them, at the head of which professional opinion ranks the Principles and Practice of Surgery,

Hibbard, and Spare, on Nature and Time in Disease; a posthumous work on Electro-Physiology, by Dr. Morgan, edited by Dr. Hammond; and the Principles and Practice of Laryngoscopy, by Dr. A. Ruppaner. The American Journal of Obstetrics, edited by Dr. B. F. Dawson, was also added to the list of our serials.

[1] An excellent little Manual of the Urine was published by Dr. A. Flint, Jr.; and an interesting tract styled "New Facts and Remarks concerning Idiocy" was issued by Dr. Edward Seguin. The Microscopical Anatomy of the Liver was illustrated by Dr. H. D. Schmidt, in an article in the New Orleans Medical Journal, which was afterwards issued in a separate form; and a useful treatise was produced, on naval hygiene, by Dr. Joseph G. Wilson and Dr. Albert C. Gorgas; in addition to which Dr. M. Clymer published valuable notes on the physiology and pathology of the nervous system; Dr. J. C. Hutchison contributed a small treatise on physiology and hygiene; and Dr. L. H. Morgan published a system of consanguinity of the human family. The new journals set on foot in 1870 were the Clinic, at Cincinnati, by Dr. J. T. Whittaker; the Philadelphia Medical Times, edited by Dr. H. C. Wood; the Michigan University Medical Review, by Drs. Cheever, Rose, Prescott, and Frothingham; the American Journal of Syphilography, by Dr. M. H. Henry; and the American Practitioner, by Dr. David W. Yandell and Dr. Theophilus Parvin—a continuation of the Western Journal of Medicine, which was for some years conducted by Prof. Parvin at Indianapolis. The Photographic Review of Medicine and Surgery was also set on foot.

by Dr. John Ashhurst, Jr. The treatise of Dr. W. A. Hammond, on Nervous Diseases, is also a graceful addition to our professional literature, albeit his readers may not always obtain the gratifying results from remedies reported as occurring in his practice. The Report to the Surgeon General's Office of the surgical cases in the army of the United States from 1865 to 1871, is an invaluable storehouse of facts; and eminently useful also, for its statistics, is the first annual Report of the Board of Health Department of New York.[1]

In 1872, a volume was produced by Dr. S. Weir Mitchell on Injuries to the Nerves, and their consequences, which is esteemed one of the most valuable contributions to surgical science made by an American writer. An elaborate work on ovarian tumors was issued by Dr. E. R. Peaslee. Dr. Gurdon Buck illustrated lithotomy and lithotrity by a number of cases in a brief memoir on the subject. Dr. Thomas M. Markoe wrote an able treatise on diseases of the bones, and Dr. J. W. S. Gouley contributed one on the diseases of the genito-urinary organs, which has been much commended. The little volume of Dr. H. C. Wood, Jr., on Thermic Fever or Sunstroke, is scientific and practical, the production of a mind trained and adapted to philosophical investigation. With it appeared his Year Book of Therapeutics, Pharmacy, and the allied sciences. A volume of clinical lectures on various important diseases, possessing uncommon excellence, was contributed by Dr. N. S. Davis.[2] In this year also appeared the last work of the learned and indefatigable Dunglison, who had been called away from his earthly labors, leaving his History of Medicine to be completed by his son Dr. Richard J. Dunglison. This posthumous work of the great medical scholar is a fit companion of his dictionary, by which his name is destined to be transmitted to the latest

[1] A Handbook of Medical Microscopy was a seasonable contribution by Dr. Joseph G. Richardson. Three new journals appeared during the year, viz.: New Remedies, by Dr. H. C. Wood, Jr., at New York; the Kansas City Medical Journal, by E. W. Shauffler; and the Southern Medical Record, at Atlanta, by Drs. Powell and Goldsmith. Dr. B. Joy Jeffries wrote a treatise on the eye in health and disease; Dr. M. L. Holbrook produced one on parturition without pain; Dr. A. L. Gihon wrote on surgery and naval hygiene; and Dr. Joseph Jones appeared in one of his laborious memoirs, giving an account of the clinics at the Charity Hospital in New Orleans. Emergencies, and how to treat them, is the title of a manual by Dr. Joseph W. Howe, of the University of New York, which affords knowledge useful to all; and the work on Bromides, by Dr. Bartholow, was read with interest at a time when the action of these medicines was so much discussed. Internal urethrotomy was discussed in a short paper by Dr. C. H. Mastin; Dr. Allen produced another treatise bearing on vital statistics, entitled Lessons on Population, suggested by Grecian and Roman History. Dr. B. W. Neftel added one on Galvano-Therapeutics, and Drs. Beard and Rockwell brought out their work on the Medical and Surgical uses of Electricity in an enlarged form, rendering it complete as to the present state of that branch of therapeutics. The tenth edition of the Medical Students' Vade mecum, by Dr. George Mendenhall, was issued that year.

[2] The little works of Drs. Clarke and Amory on the Bromides of potassium and sodium, and Dr. Morrill Wyman on Catarrh or hay fever, are both admirable in their way. A hand-book of post-mortem examinations and of morbid anatomy, by Dr. Francis Delafield, also appeared. The attention of the profession was directed to Earth as a topical application in Surgery, by Dr. Addinell Hewson, in a work on that subject. A Clinical Manual of the Diseases of the Ear was prepared by Dr. Lawrence Turnbull. A brief account of the treatment of the Venereal Disease in the Vienna Hospitals was published by Dr. M. H. Henry. Two excellent little monographs on Diseases of the Skin, and Animal and Vegetable Parasites of the Human Skin, were issued by Dr. B. J. Jeffries; the Ten Laws of Health were illustrated by Dr. J. R. Black; and Dr. J. S. Cohen contributed a practical work on Diseases of the Throat. Dr. J. W. Richardson read a judicious paper on puerperal convulsions to the Medical Society of Tennessee; and Dr. Stephen Smith produced a volume styled Doctor in Medicine, with other papers. The Charleston Medical Journal, which for some time had been suspended, was revived this year by Drs. Porcher and Kinloch.

ages. The medical journals originated in 1872 were the Western Lancet, at San Francisco, by Drs. Trenor and Babcock; and the Sanitarian, edited by Dr. A. N. Bell, at New York.[1]

In 1873 a number of valuable works were published, prominent among which was Contributions to Surgery, by that faithful worker and accomplished medical scholar, Dr. George W. Norris. A handsome volume of superior merit on ovarian tumors was brought out by Dr. Washington L. Atlee, and Dr. John A. Lidell produced one on apolexy. A volume of clinical lectures on diseases of the nervous system swelled the contributions of Dr. W. A. Hammond to medical science. A volume of huge proportions was written by Dr. James E. Garretson on the somewhat limited subject of oral surgery, and Dr. Isaac Ray published one entitled Contributions to Mental Pathology.

In 1874, a work possessing some novel and attractive features was issued by Dr. Horatio C. Wood, Jr., on Therapeutics, Materia Medica, and Toxicology. The author devotes much more space than any previous writer has done to the physiological action of medicines, facts bearing upon which he has collected with great industry from every accessible source. This departure of Dr. Wood from the old beaten track of writers on materia medica imparts unusual interest to his pages. The other works of this year were a treatise by Dr. R. A. Gunn, on the Venereal Disease, and a small volume on Croup, by Dr. J. Solis Cohen.[2]

In 1875, an exhaustive volume was issued on the Epidemic Cholera of 1873. It was the joint production of Drs. Ely M'Clellan, J. C. Peters, J. M. Woodworth, and J. S. Billings, prepared by order of the General Government. No more thorough discussion of the etiology of cholera is to be found in any American work, and, as a whole, it will take its place with the foremost of the many volumes called forth in our country by that pestilence. The work of Dr. Loomis on the Diseases of the Respiratory Organs, notwithstanding the fulness with which the subject had been treated in earlier works, was an acceptable contribution. A volume was published by Dr. R. W. Taylor on Syphilitic Lesions of the Osseous System. Dr. C. S. Fenner contributed a work on Vision, its optical defects, and the adaptation of Spectacles. A work on pulmonary tuberculosis was brought out by Dr. Addison P. Dutcher. Dr. W. B. Atkinson issued a small but practical volume, entitled Hints in the Obstetric Procedure. Dr. James E. Reeves established, at Wheeling, a journal styled the West Virginia Medical Student. Two volumes of immense labor, made up of Statistics, medical and anthropological, of the Provost-Marshal-General's Bureau, were prepared by Dr. J. K. Baxter; and Dr. B. J. Cotting produced a volume of medical addresses.

The works issued in the first months of 1876 belong to the century we are commemorating. Of these, one was a characteristic volume of medical and surgical memoirs, by Dr. Joseph Jones, the first of an elaborate series which the author contemplates. This treats of many subjects, and

[1] Dr. H. V. W. Sweringen also added a pharmaceutical lexicon; Dr. M. A. Milland wrote a work on civil mal-practice; Dr. D. B. St. J. Roosa produced a treatise on diseases of the ear; Dr. Prince published a report on galvano-therapeutics, and Dr. Henry G. Piffard wrote a little work on urinary analysis.

[2] Besides these the following were contributed: A treatise by Dr. A. D. Williamson on diseases of the ear; one on the "breath and fetid odors," by Dr. J. W. Howe; one by Dr. E. Holden on the Sphygmograph; a manual of toxicology, by Dr. J. J. Reese, and a small volume by Dr. Thomas C. Minor on erysipelas and childbed fever. A new journal, the Virginia Monthly, was also started by Dr. L. B. Edwards, at Richmond, and one, the Medical Times, at Louisville, by Dr. E. S. Gaillard.

with a patient industry seldom shown by native writers. Dr. Austin Flint, Jr., issued a splendid volume, comprising the substance of his five volumes on physiology, intended as a text-book for students. A work on extra-uterine pregnancy was produced by Dr. John S. Parry, whose early death the profession was called on to deplore soon after the appearance of his work, giving so much proof of his ability to enrich his profession. Two works were contributed by Dr. Austin Flint, a monograph on Phthisis, and a small volume of essays on Conservative Medicine, works worthy of the fame of their admired author. Dr. Lewis A. Sayre added to the productions of the century a volume, abundantly illustrated, on orthopædic surgery and diseases of the joints, which has already been translated into more than one foreign language. A new journal, too, was added to our long list of periodicals, the Louisville Medical News, a weekly publication, edited by Dr. Richard O. Cowling and Dr. William H. Galt, writers of wit as well as learning, whose keen satire of abuses in medical schools will cause their work to be remembered as one of the most useful of its period.

This retrospect of our literature ought not to be concluded without a reference to the publications of the various medical societies of our country. The Transactions of the great National Association have been mentioned; but there are societies in nearly all the States of the Union which annually send forth volumes of greater or less bulk, containing papers of lasting interest. They began as far back as 1788, when the Medical Society of New Haven County, Conn., issued a volume of "Cases and Observations." This was followed, in 1793, by a volume from the Philadelphia College of Physicians. Massachusetts has been for seventy years sending out valuable communications through her societies. The Medical Society of the State of New York, too, has for more than half a century been making contributions to medical science. The effect of these societies, not only upon the literature, but upon the tone of the profession, is everywhere felt to have been most salutary.

The reports of the lunatic asylums, now founded in all but a few of the States, are also to be mentioned as works which have enriched our professional literature. It was a sagacious remark of the Governor of Kentucky, General Adair, in his message urging upon the legislature the establishment of an asylum at Lexington, that "the hospital would help the medical school, which would return the obligation by improving the hospital system, making discoveries in treatment." Since the day that medical men were first placed over these institutions, it is a fact conceded by all that a steady improvement has been going on, as well in the therapeutic treatment of their inmates as in their whole administration. The annual communications of Earle, Kirkbride, and others who have labored so successfully in psychological medicine, have received their full measure of applause.

As forming a voluminous branch of our literature, the introductory lectures published at the opening of the numerous schools of the country each autumn, deserve a passing notice. Many of these efforts are worthy of preservation, as the offspring of the best minds in the American profession; but others are "open to severe comment," as was remarked by Dr. Holmes thirty years ago, who then added that "turgid and extravagant attempts at eloquence, a fondness for effete Latin quotations, a parade of scholastic terms where simple ones only are called for, are the common faults of these productions."

In reviewing the literature of medicine in the United States, the first fact that will strike the mind of the reader, perhaps, is the very large proportion of the works that have emanated from the medical schools. Another that must occur to every reader is the disproportion between monographs and the manuals written for the use of students. Teachers have both greater leisure and greater facilities than others of the profession for making books, as they are also more likely to find sale for them; and they write text-books rather than monographs, because students demand them. But, in the nature of things, such productions are short lived. The manual that gives a fair view of its subject to-day, must fall short of representing it to-morrow, and yields of necessity to a later system. Mundinus, when medical science stood still, held his place as a text-book on anatomy in the medical schools of Italy two hundred years. Wistar, who stood his ground longer than any of our text-books, hardly kept his place thirty years; and as to those that have succeeded him, "clouds that rake the mountain summit" scarcely follow each other faster than they have followed one another as popular favorites.

The growth of our periodical literature has been unprecedented. In 1848, the committee on medical literature reported to the Medical Association, that the number of journals in the United States was about twenty. They had been fifty years reaching that number; but in a little more than half that time their number has been doubled. Since the origin of the first, in 1797, one hundred and ninety-five, including reprints of foreign journals, have been set on foot, of which more than fifty, counting those that relate to pharmacy and dentistry, are still in progress. In all, they have made one thousand six hundred and thirty-seven volumes, a bulk which exceeds that of our monographs and manuals. Hardly any other country in the world has projected so many in the same period, or is now sustaining so large a number. Germany and Austria together have only fifty-seven; France has but fifty-two; Great Britain has but twenty-nine; Italy thirty-one; Holland six; and Spain the same number. Most of our journals have subscribers enough to justify their publication, and to support them in a state of comparative ease, though one is dropped, from time to time, for want of patronage. They have, collectively, a large circle of readers, and tend to a wide diffusion of medical knowledge. The discoveries in any branch of medicine are at once announced by them, and carried in a little while to the door of every reading physician, by which the profession in country and city is placed on comparative equality. And besides these benefits, they increase a taste for reading, and stimulate practitioners to become writers; so that the admitted superfluity of our journalism has been beneficial to the profession, promoting activity among its members, bringing to light much useful matter, exciting greater and more general exertion.

But it cannot be denied that these benefits are secured at the expense of many countervailing evils. To the excess of this literature we are to attribute its deficiencies, which all admit to be greater than those that attach to our books. The aliment that would keep a dozen journals vigorous, divided between four times that number, is barely sufficient to keep many of them alive. The support of too many of them, mental, pecuniary, and literary, is flagrantly inadequate. Not only is their list of subscribers insufficient for anything beyond a feeble existence, but the corps of contributors is too limited to give the necessary variety to their pages. The editor in the hour of pressing need is often obliged to admit

articles which he would not hesitate to reject if he had anything better on hand. But no choice is left him. The inevitable day is coming round when the number must appear; the printer is waiting, and copy must be forthcoming.

Nor is this the only fault of our journalism attributable to its excess. The editors in too many cases devote only such odds and ends of their time to their publications as they can spare from more profitable engagements. Deriving hardly any pecuniary emolument from their journals, they are compelled to look to other employment for subsistence. Nearly all their time and thoughts are engrossed by other duties, and their editorial functions are necessarily performed in a hurried and slovenly manner. They cannot take the time to "*edit*" their publications in any true sense of that term. They have no leisure for correcting the papers sent them, and for putting them into a shape suitable for the public eye. These contributions, it is safe to say, are for the most part hurriedly written, very often by young, unpractised writers, and while presenting many grains of valuable truth, contain mixed up with them no small amount of chaff which calls for the winnowing hand of the editor. Wanting this friendly office, they appear before a critical public "with all their imperfections on their heads."

Editors are not responsible for either the facts or the opinions of their contributors, but the public has undoubtedly the right to expect that the matter composing their pages shall be free from glaring literary blunders. It has a right to insist, at least, that their own papers shall be written carefully, and with a moderate show of accuracy.

But the department in which our periodical literature stands most in need of improvement is that of critical reviews. In only a few of our journals has this office—one of the most important pertaining to journalism—been properly performed. It is not too much to say that it has been generally neglected. The articles that appear in the critical department rarely aspire to the dignity of reviews, but are, as a rule, mere bibliographical notices, written after the most cursory examination of the books in hand, and often with slight knowledge of their contents. The notices, therefore, are too generally made up of commonplace strictures or indiscriminate praise—mere vague generalities which leave the reader as much as ever in the dark as to the true character and value of the book reviewed.

Still, with all these deficiencies, and others that might be pointed out, our journalism is of a character to commend it to the respect of the profession. No one who has carefully observed its progress in the last fifty years can have a doubt that it is fast outgrowing its defects. Not only are the journals appearing in a mechanical dress more neat and pleasing to the eye, and with fewer typographical errors, but in point of style they are more scholarly than those of half a century ago. The articles are more generally practical, and the selections are almost always made from original sources. If many of them were started in the interest of medical schools (which will hardly be denied), it must be owned that their conductors have generally been observant enough of public opinion to keep that object in the background. Few of them have betrayed a narrow, partisan temper. Hardly an editor has availed himself of his station to defame his professional brethren. While many articles find their way into the journals that are not instructive—many that are carelessly written and marred by many literary blemishes—it is seldom that we meet with one that offends against decency or the proprieties of social life. Personali-

ties, as they are discountenanced by the profession, are almost universally eschewed.

It is easy to see that the rivulet which represented our literature at the beginning of the century just closed, "has swollen into a torrent, augmented into a river, expanded into a sea." When we assumed our position among the sovereignties of the earth, we could hardly point to a single original volume on medicine. We were dependent as a profession almost wholly upon our brethren of the old world for instruction. All our books were the production of foreigners; but we may now claim to be independent of the world. If by any chance we could be cut off from all intercourse with other nations, the authors of America would supply the text-books required by its students, and guides to its practitioners, in every department of the profession. Nor am I assuming too much when I assert that, as lucid exponents of the existing art and science of medicine, as trustworthy companions in the sick-room, as lights in everything relating to the practice of physic, using that term in its widest scope, the works of American physicians are up with the times, and equal to the books which come to us from abroad. And this is said in full view of the fact that in some branches of medical science we have not done our part towards its advancement, while in others we have hardly done anything at all. But whether we compare the later issues of our medical press with its earlier productions, or with analogous works by physicians of the old countries, we find cause for congratulation as to the past, and for high expectations in regard to the future.

ADDRESS ON THE PROGRESS OF MEDICAL EDUCATION IN THE UNITED STATES OF AMERICA,

DURING THE CENTURY COMMENCING IN 1776.

BY

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From the date of the first English settlement in America, which was in 1607, to the Declaration of Independence in 1776, thirteen separate colonies had been established, embracing the Atlantic Coast from Massachusetts to Georgia inclusive, and all acknowledging allegiance to the government of Great Britain. Distributed over this wide extent of territory, there had accumulated about three millions of people, among whom there were between three and four thousand engaged in the practice of medicine. Of these, it has been estimated that not more than four hundred had received the degree of Doctor in Medicine from any medical college; and these, with but few exceptions, had received their collegiate education and honors in the medical schools of Europe. Only two medical schools had been organized in the colonies, namely, the Medical Department of the College of Philadelphia (now the Medical Department of the University of Pennsylvania), in 1765, and the Medical Department of King's (now Columbia) College, in New York, in 1768. Only fifty-one medical degrees had been conferred by both of these institutions prior to 1776, when active operations in them were suspended by the progress of war. In all the colonies there were not more than twelve or fifteen cities and towns having over five thousand inhabitants, and these were so widely separated from each other, and possessed of such limited means of communication, that attendance on a medical college, or on the meetings of medical societies, was hindered by delays and difficulties but little thought of at the present day.

The same condition of the country compelled many of those practising in the smaller and more remote settlements to unite other occupations with their practice. In the New England Colonies, the offices of physician and clergyman were often united in the same person, and in all the colonies it was common for the physician to occupy a part of his time and increase his income by agricultural labor.

Notwithstanding all the obstacles presented by the sparseness of the population, and the want of roads and means of communication, a large proportion of those engaged in practice had attended one course of medical college instruction, and had served a regular apprenticeship with some practitioner of note, before entering upon the active duties of the profession. Only three medical societies are known to have been organized during the colonial period of our history. The most important of these was the Medical Society of the State of New Jersey, which was organized in July, 1766. It appears that a few months previous to this, a society called the Philadelphia Medical Society was formed in that

city, chiefly through the influence of Dr. John Morgan, who had returned from Europe the year previous. It did not probably survive the war for Independence, and is not known to have left any permanent record of its doings. The Delaware State Medical Society was organized in 1776, and hence has a history parallel with that of the independence of the States.

In nearly all of the colonies, laws had been enacted concerning some topics of interest to the profession. Most of these laws were designed either to protect the people of the colonies from the introduction and spread of contagious diseases, from injury by ignorant and reckless midwives, or from exorbitant charges by physicians; or for the establishment of hospitals for the sick and insane. In only two of the colonies had laws been enacted to define the qualifications of physicians and surgeons, with provisions for enforcing an observance of the same. The General Assembly of New York, in 1760, ordained that "no person whatsoever shall practise as Physician or Surgeon in the City of New York before he shall have been examined in Physic and Surgery, and approved of and admitted by one of His Majesty's Council, the Judges of the Superior Court, the King's Attorney-General, and the Mayor of the City of New York for the time being, or by any three or more of them, taking to their assistance for such examination such proper person or persons as they in their discretion shall see fit." Such candidates as were approved received certificates conferring the right to practise physic or surgery, or both, throughout the whole province; and a penalty of five pounds was prescribed for all violations of this law.[1] A similar act was passed by the General Assembly of New Jersey in 1772.

Although most of the colonies had provided temporary hospitals for seamen, emigrants, and the victims of smallpox and other epidemic diseases, the only permanent general hospital for the sick was established in Philadelphia, in 1752, and was aided by a grant of two thousand pounds from the Colonial Assembly. Dr. Thomas Bond was appointed superintendent, with Drs. Lloyd Zachary, Thomas Cadwalader, Samuel P. Moore, John Redman and Phineas Bond, as associates.

Dr. Thomas Bond, from the opening of the institution, introduced his class of students for bedside instruction, and thereby became the first regular clinical lecturer in America. In 1767, the colonial government of New York was induced to grant a charter for a general hospital, and an organization was effected, and the work commenced. But the building when nearly completed was destroyed by fire, in 1772, and owing to the supervention of the Revolutionary war was not rebuilt until 1791. From this very brief glance at the condition of medical matters in the colonies, more especially in relation to educational facilities, it will be seen that at the commencement of our history as an independent nation, in 1776, there were between three and four thousand practitioners of medicine, supplying about three millions of people, their field of duty occupying thirteen States, embracing the whole Atlantic coast from Maine to Florida, and containing two medical colleges, two organized medical societies, and one permanent general hospital. Small as were these beginnings, they nevertheless constituted the germs from which have since developed all the educational institutions belonging to the profession in our country.

[1] See History of Medical Education and Institutions in the United States, by N. S. Davis, M.D., p. 22.

The Medical Department of the College of Philadelphia, which had been organized in 1765, with Dr. William Shippen, Jr., as Professor of Anatomy and Surgery, and Dr. John Morgan, as Professor of Medicine, had continued regular annual courses of medical instruction until 1776, when the session was broken up by the vicissitudes of war, and the most valuable movable materials of the college were privately removed to places of safety by the provost and members of the faculty. Previous to this time, Drs. Adam Kuhn and Benjamin Rush had been added to the medical faculty of the college—the first, as Professor of Materia Medica and Botany, and the second, as Professor of Chemistry. During the same time, Dr. Thomas Bond had been lecturing on clinical medicine in the Pennsylvania Hospital. On an attempt being made to resume the courses of medical instruction in the College, in 1779, political differences and suspicions caused the State Legislature to pass an act abrogating the charter of the college, removing its officers, and transferring its property to a new institution, called the "University of the State of Pennsylvania."

The new University was granted a very liberal charter and considerable endowments. The Rev. John Ewing, D.D., was appointed Provost, and an effort immediately made to organize a medical department, by offering those who had held professorships in the College, the same positions in connection with the University. Regarding the act of the Legislature abrogating the charter of the College and confiscating its property as unjust, all but Dr. William Shippen, Jr., refused the offer and continued their annual courses of instruction independently, but without legal authority to confer degrees. The Trustees of the University, finding themselves unable to fill the several medical chairs satisfactorily, agreed to confer degrees on such students as should be examined and recommended by the several professors of the College whose charter had been abrogated. By this arrangement, at the first public commencement of the University, held in Philadelphia, June 27, 1780, the degree of Bachelor of Medicine was conferred on William W. Smith and Ebenezer Crossly, and that of Doctor of Medicine on David Ramsey, who was in the service of the American Army, and at the time a prisoner in the hands of the British. These were the first medical degrees conferred by any institution created under the auspices of one of the independent States of America. This state of antagonism between the professors in the College of Philadelphia and the trustees of the University, continued until 1789, when, aided by the influence of Benjamin Franklin, after his return from Europe as foreign minister, the former succeeded in inducing the State Legislature to repeal the act of abrogation, and to restore to the College all its rights and privileges. This was done in March, 1789, but it left the University in existence, with its endowments from confiscated estates the same as before. The officers and faculty of the College of Philadelphia were immediately reinstated, with Dr. Benjamin Franklin himself as president of the Board of Trustees.

In October of the same year, Dr. John Morgan died, and Dr. Adam Kuhn resigned, leaving the chairs of Practice of Medicine, and Materia Medica and Botany, vacant. The vacancies were immediately filled, and the Medical faculty of the College as reorganized for the session of 1789–90, was composed of William Shippen, Jr., M.D., Professor of Anatomy, Surgery, and Midwifery; Benjamin Rush, M.D., Professor of the Theory and Practice of Physic; Caspar Wistar, M.D., Professor of Chemistry and of Institutes of Medicine; Samuel P. Griffiths, M.D., Pro-

fessor of Materia Medica and Pharmacy; Benjamin Smith Barton, M.D., Professor of Natural History and Botany. At the same time Dr. Shippen continued to occupy the same chair in the University of the State of Pennsylvania, and was joined in that faculty by Dr. Adam Kuhn, in the Chair of Theory and Practice of Medicine, and by Dr. James Hutchinson, in that of Chemistry.

The trustees of the College of Philadelphia not only reinstated a full corps of medical professors, but they revised their former rules in regard to medical requirements for graduation. These rules, as adopted in 1767, provided for the conferring of the degrees of Bachelor of Medicine and Doctor of Medicine. The student was permitted to apply for the first after "a sufficient apprenticeship to some reputable practitioner in Physic," attendance on "at least one course of Lectures on Anatomy, Materia Medica, Chemistry, the Theory and Practice of Physic, one course of Clinical Lectures, and attendance on the practice of the Pennsylvania Hospital one year." It was expected by the founders of the College, that those who took the Bachelor's degree would return after three years of study and practice, and take the higher degree of Doctor. Experience, however, had proved this expectation fallacious, as very few of those who entered into practice after receiving the first degree, ever returned for the second. For this reason the Bachelor's Degree was abolished, and the revised regulations adopted for the College by the trustees, November 17, 1789, and published in the Pennsylvania Gazette were as follows:—

"I. No person shall be received as a candidate for the degree of Doctor of Medicine until he has arrived at the age of twenty-one years, and has applied himself to the study of medicine in the College for at least two years. Those students, candidates, who may reside in the city of Philadelphia or within five miles thereof, must have been the pupils of some respectable practitioner for the space of three years, and those who may come from the country, and from any greater distance than five miles, must have studied with some respectable physician there, for at least two years.

II. Every candidate shall have regularly attended the Lectures of the following professors, viz.: of Anatomy and Surgery; of Chemistry and the Institutes of Medicine; of Materia Medica and Pharmacy; of the Theory and Practice of Medicine; the Botanical Lectures of the Professor of Natural History and Botany; and a course of Lectures on Natural and Experimental Philosophy.

III. Each candidate shall signify his intention of graduating to the Dean of the Medical Faculty, at least two months before the time of graduation, after which he shall be examined privately by the professors of the different branches of medicine.

If remitted to his studies, the professors shall hold themselves bound not to divulge the same; but if he is judged to be properly qualified, a medical question and a case shall be proposed to him, the answer and treatment of which he shall submit to the medical professors. If these performances are approved, the candidate shall then be admitted to a public examination before the Trustees, the Provost, Vice Provost, Professors and Students of the College; after which he shall offer to the inspection of each of the Medical Professors a Thesis, written in the Latin or English Language (at his own option) on a medical subject. This Thesis, if approved of, is to be printed at the expense of the candidate, and defended from such objections as may be made to it by the Medical Professors at a commencement to be held for the purpose of conferring degrees, on the first Wednesday in June every year.

Bachelors in Medicine who wish to be admitted to the degree of Doctor in Medicine, shall publish and defend a Thesis agreeably to the rules above mentioned.

The different Medical Lectures shall commence annually on the first Monday in November, the lectures in Natural and Experimental Philosophy about the same time, and the lectures on Botany on the first Monday in April.

BENJAMIN FRANKLIN, President of the Board of Trustees.
WILLIAM SMITH, Provost of the College and Secretary of the Board of Trustees."[1]

The Trustees of the University of the State of Pennsylvania adopted very similar regulations regarding courses of lectures and time of study for the degree of Doctor in Medicine, but they continued also to confer the degree of Bachelor, as before. One hundred and four medical students attended the Lectures in Philadelphia during the College term of 1790–1, and appear to have been nearly equally divided between the two schools. The disadvantages arising from this division of patronage were too apparent to be overlooked, and the friends of both schools soon instituted measures for an amicable union. These measures resulted in the passage of an Act of the Legislature of the State, September 30, 1791, uniting the College and the University on the terms which had been mutually agreed to by both the parties in interest. The name adopted for the united institution was the "University of Pennsylvania." Dr. John Ewing was elected Provost and Professor of Natural and Experimental Philosophy, and all the professors in the medical department of the two previous institutions were elected professors in the new one. The full medical faculty of the University, as thus constituted, was arranged as follows: William Shippen, M.D., Professor of Anatomy, Surgery, and Midwifery; Caspar Wistar, M.D., Adjunct; Adam Kuhn, M.D., Professor of Theory and Practice of Medicine; Benjamin Rush, M.D., Professor of Institutes of Medicine and of Clinical Medicine; James Hutchinson, M.D., Professor of Chemistry; Samuel P. Griffiths, M.D., Professor of Materia Medica and Pharmacy; Benjamin Smith Barton, M.D., Professor of Botany and Natural History.

The University, as thus reorganized, ceased to confer the degree of Bachelor of Medicine, and left it optional with the medical students whether they should attend the lectures on Natural History and Botany, but in all other respects adopted the "Rules respecting a medical education and the conferring of Degrees in Medicine," which have been already given as having been adopted by the Trustees of the College of Philadelphia in 1789.

We have thus sketched briefly the progress of medical instruction from its incipient beginnings in Philadelphia, to the complete establishment of the University of Pennsylvania by the formal election of the faculty above named, in January, 1792, not merely because it was the pioneer school, and one which still continues to exercise an important influence over the educational interests of our profession, but because it has served as the type or pattern for nearly all the medical schools subsequently organized in this country. While the cause of medical education was progressing, as we have detailed, in Philadelphia, the profession in New York was not idle. We have stated that, as early as 1767, a charter was granted by the Colonial government of New York for the establishment of a General Hospital in that city; that, chiefly through the efforts of Drs. Samuel Bard and Peter Middleton, a society was organized, and a hospital building nearly completed, which was destroyed by fire in 1772; and that the enterprise received liberal pecuniary aid from Sir Henry

[1] See History of the University of Pennsylvania, by Joseph Carson, M.D., pp. 95–6.

Moore, the provincial Governor, from the Legislature, from the City Corporation of New York, and from many private individuals. The loss of the building by fire, and the speedy supervention of the war for Independence, prevented all further efforts in that direction until after the close of that war. Soon after this latter event, the corporation known as the Society of the New York Hospital was revived, its work resumed, and adequate hospital buildings completed ready for occupation in 1791. The same persons who had been influential in the primary effort to establish the New York Hospital, also induced the trustees or governors of King's College, located in New York City, to establish a Medical Department of that institution, and to appoint a full medical faculty in 1768. This faculty was composed of Samuel Clossy, M.D., Professor of Anatomy; John Jones, M.D., Professor of Surgery; Peter Middleton, M.D., Professor of Physiology and Pathology; James Smith, M.D., Professor of Chemistry and Materia Medica; John V. B. Tennent, M.D., Professor of Midwifery; and Samuel Bard, M.D., Professor of Theory and Practice of Physic.

Some important differences will be noticed in comparing this faculty organization with that of the faculty just previously organized in Philadelphia. First, it makes a more complete division of labor, and at once gives a full recognition to the departments of Surgery and Midwifery, assigning to each a separate professorship on an equality with all other departments, instead of making them appendages to the more prominent departments of Anatomy and Theory and Practice of Medicine. In this respect the New York faculty was in advance, not only of that of the Philadelphia College, but also of that of the University of Edinburgh. Second, it makes no mention of the departments of Botany and of Natural and Experimental Philosophy. Third, as the number of chairs and the consequent division of labor was greater, the annual college term was made shorter by one month; the term closing in May instead of June in each year. In all other respects the regulations were very similar to those adopted by the school in Philadelphia. The first courses of lectures were given in the autumn and winter of 1768–9, at the close of which, in May, 1769, the degree of Bachelor in Medicine was conferred by the Trustees of the College on Samuel Kissam and Robert Tucker. And at the close of the succeeding college term, in May, 1770, the degree of Doctor in Medicine was conferred on one or both of the same persons. These are stated by Dr. J. B. Beck, and several other writers, to have been the first medical degrees conferred by any college in America. This is true, however, only as it relates to the degree of Doctor in Medicine. For, as a matter of fact, the College of Philadelphia conferred the degree of Bachelor in Medicine on ten students in June, 1768, but did not confer the degree of Doctor in Medicine until June, 1771.

The regular annual courses of medical instruction were continued, and attended by small classes of students, until they were interrupted by the war. During the war, the name of King's College was changed to that of Columbia College, which it still retains. Soon after the close of the war an effort was made to re-establish the medical department connected therewith. By some misfortune connected with the procuring of subjects for dissection, a violent popular outbreak, called the "Doctors' Mob," was induced, during which the dissecting-room of the college was broken up, and for three days law and order were trampled under foot. To counteract, as far as possible, the evil influences thus brought to bear upon the profession, and to improve medical science, several of the more

enterprising and younger members of the profession formed a private society, and in 1787 succeeded in inducing the magistrates of the city to establish a public free dispensary for the sick poor. Among the more prominent of those engaged in this enterprise were Drs. William Moore, Nicholas Romaine, Benjamin Kissam, Wright Post, and Valentine Seaman. They not only gave gratuitous attendance on the poor in the dispensary, but in connection therewith gave lectures on most of the branches of medicine; thereby making it the first institution connected with *practical* instruction in medicine under the corporation of the City of New York.

So great was their success that, in 1790, more than fifty students attended their instruction, and an unsuccessful attempt was made to organize an independent school under the name of the College of Physicians and Surgeons. In the autumn of 1791, the private association introduced not less than sixty students into Columbia College, and thereby induced the Legislature of the State to make a grant of $30,000 to the trustees, for the purpose of enabling them to enlarge their buildings, etc. In the following year the medical faculty was reorganized by the appointment of Drs. Bailey, Post, Rogers, Hammersly, Nicol, and Kissam, as professors, and of Dr. Samuel Bard, as Dean of the Faculty. Some of these appointments were very unsatisfactory to the students, and were followed by such a degree of opposition as to greatly interfere with the prosperity of the College, and to lead many members of the profession in the city to use their influence in favor of the establishment of another and independent medical school.

In accordance with their wishes, the regents of the University of the State of New York granted a charter for a new college in 1807, to be located in the city, and called the College of Physicians and Surgeons of New York. It was placed under the control of a board of trustees, consisting of the whole Medical Society of the city and county of New York, and the degree of Doctor of Medicine was to be conferred by the regents of the University of the State, on the recommendation of the trustees and faculty of the College. The first course of lectures was given in the winter of 1807–8, to a class of fifty-three students. In 1810 the medical department of Columbia College was finally discontinued, leaving the College of Physicians and Surgeons the only one in the State, with a class of students numbering eighty-two, and the following very able faculty, viz.: Samuel Bard, M.D., President; David Hosack, M.D., Vice President and Professor of the Theory and Practice of Medicine and of Clinical Medicine; William James Macnevin, M.D., Professor of Chemistry; Samuel L. Mitchill, M.D., Professor of Materia Medica and Botany; Valentine Mott, M.D., Professor of Surgery; John W. Francis, M.D., Professor of Obstetrics and Diseases of Women and Children; Wright Post, M.D., Professor of Anatomy.

But instead of that rapid prosperity which the friends of the institution and the regents of the University now anticipated, the very numerous board of trustees, being mostly medical practitioners in the immediate vicinity of the College, soon became distracted by opposing councils, and jealousies arose between them and the members of the faculty, which caused much difficulty and greatly retarded the prosperity of the college. These defects were corrected in subsequent years, and the institution has maintained a good reputation and leading influence down to the present time.

In 1783, the medical department of Harvard College, in Cambridge,

near Boston, was organized by the appointment of John Warren, M.D., Professor of Anatomy and Surgery; Aaron Dexter, M.D., Professor of Chemistry and Materia Medica; and Benjamin Waterhouse, M.D., Professor of the Theory and Practice of Medicine. The medical department of Dartmouth College, at Hanover, New Hampshire, was organized in 1797, chiefly through the influence of Dr. Nathan Smith, who was appointed Professor of Medicine, and for ten or twelve years taught all the branches of medicine with signal ability. The medical department of the University of Maryland, at Baltimore, was incorporated in 1807, and was supplied with an able faculty, consisting of John B. Davidge, M.D., Professor of the Principles and Practice of Surgery, and Nathaniel Potter, M.D., Professor of the Theory and Practice of Medicine, assisted by Drs. Shaw, of Maryland, and Cooke, of Virginia.

By the foregoing brief historical statements it will be seen that during the first thirty years after the close of the war for Independence, which included the first decade of the present century, seven medical schools had been organized, namely, two in Philadelphia, two in New York City, one in Boston, one in Hanover, and one in Baltimore. The two in Philadelphia had been speedily united into one, and one of those in New York had been discontinued. We find, therefore, only five medical schools in existence in the United States in 1810, with an aggregate number of medical students in attendance of about 650, of whom about 100 received in that year the degree of either Bachelor or Doctor of Medicine. Two-thirds of this whole number were in the University of Pennsylvania. Only two public general hospitals had been established, namely, the Pennsylvania Hospital in Philadelphia, and the New York Hospital in New York City.[1] The former was a very important aid to the University in attracting students to Philadelphia, by the clinical instruction which it afforded, as inaugurated by Dr. Thomas Bond soon after it was opened for the reception of patients.

All but one of these medical schools were organized as departments of colleges or universities of literature and science, already established, and all but the one began with a small number of professors, making it necessary that one man should teach two, and sometimes three, branches of medicine during each annual college term. For this reason more than any other, the medical college terms were made to commence generally on the first of October, and to continue until the following May or June. All these schools adopted, at first, the policy of conferring the degree of Bachelor of Medicine on students who had studied medicine with some reputable practitioner not less than two years, and had attended the medical instruction in the college one year, or rather one college term; and the degree of Doctor of Medicine after three years of study and two annual college terms. It must be remembered that, during the colonial period of our history, and for thirty or forty years subsequent to the achievement of our National Independence, it was the universal custom for young men who entered upon the study of medicine to become regularly apprenticed to some practitioner for a term of three or four years, during which time the preceptor was entitled to the student's ser-

[1] Since the above was written, we have learned that, in 1784, Don Andras Almonaster commenced the erection of a public or general hospital in the city of New Orleans, on the site of the one blown down in the great storm of 1779. It was called the New Charity Hospital, and cost $114,000. As Louisiana became one of the States of our Union, by purchase, in 1803, this Charity Hospital should have been included with those of Philadelphia and New York mentioned above.

vices in preparing and dispensing medicines, extracting teeth, bleeding, and other minor surgical operations, and, when more advanced in studies, in attending on the sick; and, in return, the preceptor became obligated to give the student detailed and thorough instruction in all the branches of medicine. Many of the more eminent practitioners often had several students in their offices at one time, constituting a small class who were drilled almost as regularly in their studies as they would be in a college. In some instances the term of apprenticeship was extended to six and even seven years, and was made to commence at the early age of fifteen or sixteen years. All these customs were brought by the emigrants from the parent country, and their perpetuation here was rendered more necessary by the sparseness of the population and the difficulty of access to medical schools.[1]

In the midst of such customs, and at a period in the world's history when railroads, steamboats, and other means of speedy transit were unknown, and even post-coaches were rare, it was entirely reasonable that the first idea of a medical college should be to furnish the means for a rapid review of the several branches of medical science, aided by such experiments and appliances for illustration as could be commanded, and the whole concentrated into as small a part of the year as possible. The idea of the founders of medical schools, both in Great Britain and in this country, was to make them *supplement* but *not supersede* the work of the preceptor and the medical apprentice. The study of anatomy by dissections, the illustration of chemistry by experiments, the clinical observation of disease at the bedside, were capable of being carried on in the offices of preceptors, only to a very limited extent. But by combining several preceptors, each eminently qualified in some one department, in a college faculty, with access to anatomical rooms, chemical laboratory, and hospitals for the sick, all the branches of medicine then recognized could be very well reviewed in the form of didactic instruction, in five or six months of the year. It was expressly to supply the wants here indicated, with the greatest economy of time and labor, that the medical department of the University of Edinburgh was founded in the beginning of the eighteenth century, and conferred its first degree of Doctor of Medicine in 1705. It rose rapidly to distinction among the schools of Europe, and furnished the model after which the first medical schools in this country were organized; as it was the *Alma Mater* of nearly all their first professors.

Assuming that the student would serve from two to four years of his apprenticeship to his preceptor, before resorting to a medical school, the several professors very naturally arranged their courses of instruction to begin nearly at the same time, generally in September or October, and to be completed in time for the public commencement and conferring of degrees in the following May or June. And as the Bachelor's degree was generally conferred after attendance on one full course of college instruction in the several branches taught, no gradation or consecutive order of studies could be incorporated into the college course. The addition of one or two years more of study, including a second course of college instruction, entitled the applicant to an examination for the degree of Doctor of Medicine. At the first organization of all the medical schools to which we have thus far alluded, provision was made for conferring the degrees of both Bachelor and Doctor of Medicine. But, as already men-

[1] See Contributions to the Annals of Medical Progress, etc., by J. M. Toner, M.D.

tioned, the degree of Bachelor was abandoned by the College of Philadelphia in 1789, and by the University of Pennsylvania in 1791, and by all the medical colleges in the country after 1813. The whole number of medical degrees conferred by the seven medical schools whose origin we have traced, prior to 1810, did not exceed six hundred. Very many, however, served their regular apprenticeship with the preceptor, attended one course of college instruction, and entered upon practice without a college degree; and there were not a few who entered upon the responsible duties of practice with simple certificates from their preceptors, without ever having seen the inside of a college building.

We shall not fully appreciate the relations of the medical schools to the needs of the profession at that time, unless we consider also the coincident condition of the different branches of medical science. To the active workers of the present generation, a medical college with only three or four professors, or one professor attempting to teach anatomy, surgery, and midwifery, all in one college term of five or six months, would appear hardly less than absurd. If we remember, however, that down to the commencement of the present century, the principal medical works in use were the writings of Sydenham, Boerhaave, and Cullen, the Physiology of Haller, the Anatomy of Cheselden or Monro, the Surgery of Sharp, Pott, or Jones, the Midwifery of Smellie or Hunter, and the Materia Medica of Lewis, we shall readily see that the field of medical study was limited in comparison to that which now opens before the student.

At that time, surgery had only begun to be recognized as a distinct branch from anatomy. We learn from an introductory lecture by James Spence, F.R.C.S.E., Professor in the University of Edinburgh, that "so late as 1777, when the College of Surgeons petitioned the patrons to institute a separate professorship of surgery in the University, they were opposed by Monro, then Professor of Anatomy, as interfering with his branch; and he succeeded in getting his commission altered so as to include surgery, which was thus made a mere adjunct of the anatomical course, and continued to be so taught (if it could be said to be taught) until the institution of the chair of Surgery in 1831." It was not until June 4, 1805, that Surgery was separated from the chair of Anatomy by the appointment of Dr. Philip Syng Physick as Professor of Surgery, on an equality with the other professors in the University of Pennsylvania. The department of Midwifery was still later in gaining recognition as a distinct branch of medicine. At the request of Dr. Caspar Wistar, who had succeeded to the professorship of Anatomy and Midwifery in the University on the death of Dr. Shippen, in 1808, the trustees separated the chairs, and in 1810 appointed Dr. Thomas Chalkly James the first Professor of Midwifery in that College. And yet it was not until three years later, October 11, 1813, that he was formally recognized as a member of the faculty, and attendance on his lectures rendered obligatory upon the students who applied for graduation. In New York, however, midwifery was recognized as a distinct branch at a much earlier period than in any of the other cities in which medical schools had been organized; for in the first medical faculty appointed in connection with King's College, in 1768, Dr. John V. B. Tennent was made Professor of Midwifery, apparently on an equal footing with all the other professors.

The foregoing facts are sufficient to show the limited field of medical science at the beginning of the present century, compared with the same field at the present time. If this is borne in mind, it will be seen that

the five medical schools, embracing the medical departments of the University of Pennsylvania, Harvard College, near Boston, Dartmouth College, at Hanover, the University of Maryland, at Baltimore, and the College of Physicians and Surgeons, at New York, which were either reorganized or founded *de novo* during the first thirty years of our history as an independent nation, were established on as liberal a basis, and were as well adapted to the then existing wants of the profession and the people, as any that have been organized since. As a general rule, they commenced with a small number of professors, but as the different departments of professional knowledge became better developed, and the work of instruction better systematized, there was manifested that tendency to make divisions of labor by the creation of new chairs, which has continued to the present time. In this respect the University of Pennsylvania, as the leading school, may be taken as a representative of all the rest. Commencing with only two professors, Drs. Morgan and Shippen, aided by the clinical instruction of Dr. Bond in the Hospital, in 1765, the number was increased by the addition of the chairs of Chemistry and Materia Medica, in 1768–9; and on the merging of the College of Philadelphia with the University of the State of Pennsylvania, in 1791–2, the number of chairs was increased to six, by the addition of professorships of Institutes of Medicine, and of Botany and Natural History. A further addition took place by the creation of a separate chair of Surgery, in 1805, and one of Midwifery, in 1810. From the year just named the number of medical schools began to increase with great rapidity.

A medical department was established in connection with Yale College, in New Haven, in 1810, but the first course of instruction was not commenced until 1813. In 1812, the Regents of the University of the State of New York incorporated the College of Physicians and Surgeons of the Western District, located in Fairfield, Herkimer County. The Vermont Academy of Medicine, located at Castleton, Vermont, received a charter from the Legislature of that State in 1818. The same year, a medical school was organized as a department of Transylvania University, at Lexington, Kentucky, chiefly through the influence of Drs. Dudley, Richardson, and Blythe. In January, 1819, the Legislature of Ohio passed an act incorporating the Medical College of Ohio, in Cincinnati. In 1820, the Medical School of Maine was organized as a department of Bowdoin College, at Brunswick, Maine, with a faculty consisting of Drs. Nathan Smith, John D. Wells, and Parker Cleaveland. In 1821, a medical school was organized in connection with Brown University, at Providence, Rhode Island, but was soon after discontinued. The medical school of the University of Vermont was established in Burlington, in 1822. A school called the Berkshire Medical Institute, was organized in Pittsfield, Massachusetts, under the charter of Williams College, in 1823. The Medical College of South Carolina was established in Charleston, in 1824, and supplied with a full corps of professors, including the well-known names of Drs. Ramsey, Holbrook, Dickson, Prioleau, Frost, Ravenal, and Elliot. The Medical Department of Columbian College, in the District of Columbia, was organized in 1825, chiefly through the influence of Drs. Thomas Sewall and Frederick May, of Washington.

In the winter of 1825, the Legislature of the State of Pennsylvania passed an act under which a new medical school was organized in Philadelphia, in connection with a literary institution called Jefferson College, at Canonsburg; and hence the new medical school took the name of Jefferson Medical College, and, in its progress since, has attained a prosper-

ity and influence equal to that of the best medical colleges in this country. The Medical Department of the University of Virginia, located at Charlottesville, in that State, was organized in 1825, and has continued to the present time, teaching the different branches of medicine in the same manner as the various branches of science are taught in other departments of the institution. The medical school of the Valley of Virginia was established at Winchester, Virginia, in 1826, on nearly the same plan as the Medical Department of the University, at Charlottesville. It attracted but a small number of students, and appears to have been discontinued several years since. In 1827, the Washington Medical College was organized in Baltimore, on the same plan as the majority of American medical colleges. The Medical College of Georgia, located at Augusta, was incorporated by an act of the Legislature of that State in 1830, and the first course of instruction was given in 1832–3. The Willoughby University, in Ohio, was chartered in 1834, and its first course of instruction commenced in the autumn of 1835. The Medical Department of the University of Louisiana, at New Orleans; the Medical Institution of Geneva College, at Geneva, New York; the Medical Department of Cincinnati College, Cincinnati, Ohio; and the Vermont Medical School, at Woodstock, Vermont, were chartered by the Legislatures of the respective States in which they were located in the year 1835.

The Medical Department of the University of Louisville, Kentucky, was organized in 1837, and the Medical Department of the University of the City of New York, the same year. The Medical Department of Hampden Sidney College, at Richmond, Virginia, was organized in 1838. The Albany Medical College, at Albany, New York, and the Medical Department of Pennsylvania College, at Philadelphia, in 1839. The Missouri Medical College, at St. Louis, Missouri, was organized in 1840, and the St. Louis Medical College, in the same city, in 1841. The Legislature of Illinois granted a charter for the Rush Medical College, in Chicago, in 1837, but a faculty was not organized for instruction until 1843. The Cleveland Medical College was organized at Cleveland, Ohio, in 1843. The Medical Department of the University of Buffalo was organized in Buffalo, New York, in 1846. The Starling Medical College and Hospital was organized at Columbus, Ohio, in 1847. The Medical Department of the University of Michigan, at Ann Arbor, was instituted in 1849. The Medical Department of the University of Nashville, at Nashville, Tennessee; the College of Physicians and Surgeons, at Keokuk, Iowa; and the Woman's Medical College of Pennsylvania, at Philadelphia, were organized in 1850.

The Medical College of Virginia, at Richmond; the Medical Department of Georgetown University, at Washington; and the Cincinnati College of Medicine and Surgery, at Cincinnati, were created in 1851. The Savannah Medical College, at Savannah, Georgia, and the Miami Medical College, at Cincinnati, were established in 1852. The Atlanta Medical College was commenced at Atlanta, Georgia, in 1855. The Medical College of the Pacific, at San Francisco, was organized in 1858. The Chicago Medical College, Medical Department of the Northwestern University, was organized in 1859. The Medical College of Mobile, Alabama, and the Long Island College Hospital, in Brooklyn, New York, were organized in 1860. The Bellevue Hospital Medical College was established in the city of New York in 1861. The Woman's Medical College of the New York Infirmary, located in the city of New York, was created in 1864. The Medical Department of Willamette University,

located at Salem, Oregon, was commenced in 1866. The Medical Department of Howard University was organized in 1867. The Medical Department of the University of South Carolina, at Columbia, and the Detroit Medical College, at Detroit, Michigan, were commenced in 1868. The Medical Department of the University of Wooster, at Cleveland, Ohio; the Kansas City College of Physicians and Surgeons, at Kansas City, Missouri; the Louisville Medical College, at Louisville, Kentucky; the Medical Department of the Iowa State University, at Iowa City; and the Medical Department of the Indiana University, at Indianapolis, all had their beginning in 1869.

The Woman's Hospital Medical College, at Chicago, and the Medical Department of Lincoln University, at Oxford, Pennsylvania, were organized in 1870. The Free Medical College for Women was organized in the city of New York in 1871. The Medical College of Evansville, Indiana, was chartered in 1846, and reorganized in 1872. The College of Physicians and Surgeons of Syracuse University, at Syracuse, New York, and the College of Physicians and Surgeons at Wilmington, North Carolina, were incorporated in 1872. The Medical Department of the University of California, at San Francisco, was first organized under the name of the Toland Medical College in 1864, and became a department of the University in 1873. The Medical College of the University of the State of Missouri, at Columbia, Missouri, and the Texas Medical College and Hospital, at Galveston, Texas, were organized in 1873. Medical schools have recently been established in Portland, Maine, and in Baltimore, Maryland, the exact dates of the formations of which are not at hand.

From the foregoing statistics we learn that, during the thirty years intervening between the close of the war for Independence and 1810, seven medical schools were organized; in the thirty years intervening between 1810 and 1840, twenty-six new medical colleges were added to the list; in the thirty-five years since 1840, the number of new medical schools created is forty-seven; making the whole number of medical educational institutions established in the United States, during the first century of our history as a nation, eighty. We have not included in these numbers five or six mere abortive attempts to form medical schools in different localities, but which were so transient as to leave a record difficult to trace. Of the eighty which have been established, seventeen have been discontinued or suspended, leaving, at this date, sixty-three medical colleges now in active operation in this country. Of these, Maine has two, New Hampshire one, Vermont three, Massachusetts one, Connecticut one, New York nine, Pennsylvania three, Maryland three, Virginia two, South Carolina one, Georgia three, Alabama one, Louisiana two, Texas one, Tennessee one, Kentucky three, Missouri three, California two, Oregon one, Iowa two, Illinois three, Indiana three, Ohio seven, Michigan two, and the District of Columbia three, leaving thirteen States without any medical college now in operation. Four of the schools included in the foregoing list, namely, two in New York City, one in Philadelphia, and one in Chicago, are established for the exclusive education of women in medicine. The whole number of students attending the medical colleges in 1810 was about 650, of whom about 100 graduated at the close of the college terms for that year. The population of the United States at that time was 7,239,881. In 1840, the whole number of students in the colleges was about 2500, of whom about 800 received the degree of Doctor of Medicine. The population of the United States

in that year was 17,069,453. During the college terms for 1875–6, the whole number of medical students in attendance on the colleges was 6650, of whom 2200 received the degree of Doctor of Medicine. The population of the States at this time is over 40,000,000. Without claiming absolute accuracy for the foregoing figures, they are sufficiently close for all the purposes of comparison. And they show clearly, notwithstanding all that has been said about the rapid multiplication of medical schools, and the increased numbers of students and graduates, that the colleges and the students, during the last thirty-five years, have increased in a ratio only about equal to the increase of the population of the country. The whole number of those engaged in teaching as professors in the several medical colleges, at this time, is about five hundred.[1]

As has been already stated, the origin of medical schools in this country was solely from individual efforts, put forth from time to time, as the ambition of the individuals or the supposed wants of the country demanded, and not from any well-digested scheme or official plan of professional education adopted either by the States or by the General Government. The same remark is true, not only of their origin, but also of their continuance and multiplication to the present time. Whenever and wherever the Legislature of any one of our States has been asked to grant a charter for a medical college, as a rule the request has been complied with. And in the few instances in which such requests have been denied, the parties interested have seldom found difficulty in forming a connection with some literary college or university, already having authority to grant degrees in medicine, as well as in general science. Some of the State Legislatures, in addition to liberally granting charters for medical colleges, have also occasionally appropriated a few thousand dollars to aid in the erection of suitable buildings, but rarely, if ever, to endow professorships, or to defray any of the current expenses of such institutions. In the State University of Michigan, and possibly in one or two other State Universities, the medical professorships, like those of the other departments, are sustained by the income from the general endowment, independent of the fees derived from the medical students. We may say, in general terms, therefore, that the whole system of medical education in this country, represented to-day by sixty-three medical colleges distributed among twenty-four different States, sustained by the active work of over five hundred professors, and annually aiding in the education of nearly seven thousand students, is the spontaneous outgrowth of the profession itself, self-reliant, and almost wholly self-sustained. Originating among a free people, under the *ægis* of various educationally independent States, apparently striving to keep pace in the increase of their number and efficiency with a population which, in one short century, has increased from three millions to forty millions, and extended over a territory from the Atlantic to the Pacific, and from the great interior Lakes to the Gulf, under circumstances of the freest competition, these schools constitute a subject worthy of the most careful study.

To what extent have these schools been adapted to the real wants of the profession and people of these States? Wherein have they failed to keep themselves adjusted to these needs or wants, as the country has

[1] In the above statistics we have not included colleges or schools for teaching exclusively dentistry or pharmacy, or exclusive dogmas in medicine. Of the first there are eleven; of the second, thirteen; of the third there are three, called Eclectic, and eight or nine, Homœopathic.

advanced to the present time? If failures in adjustment have taken place, in what do they consist, and how can they be remedied? Let me ask a continuance of your patient attention, while I answer as briefly and directly as possible these most important questions, and my task will be done.

We have already seen that our medical colleges had their origin at a time when medical science and art occupied a far narrower field than at present. A time when obstetrics was yet mostly in the hands of unlettered midwives; surgery an appendage to anatomy; organic chemistry, histology, and even physiology, as now recognized, hardly known. A time, too, when it was the universal practice for regular students of medicine to apprentice themselves to private preceptors of reputation, from whom they expected to receive the greater part of their professional education. The great and pressing need, at that time, was for schools in which, after gaining a knowledge of the text-books, the rude pharmacy of his preceptor's office, and the individual experience of the preceptor himself, during two or three of the first years of his study, the student could review the whole in connection with such experimental demonstrations as could be given only in the laboratory, the dissecting room, and the clinical wards of the public hospital. It was to supplement, not supersede, the work of the private preceptor.

Precisely this was what the medical schools were adjusted to do, and they fulfilled the object well. Hence, although originating in different States, wholly independent of each other, and in direct rivalry for patronage on which they depended for support, they were remarkably similar in their organization and requirements. At first, the number of professors was small, and the college term eight or nine months, offering the Bachelor's Decree after three years' study, and one college term, and the Doctor's Degree after one or two more years, and a second course of college attendance. But as most of the students lived in small towns and country districts remote from the colleges, making attendance on the lectures both tedious and expensive, only a part of those engaging in the study of medicine ever reached a medical college, and most of these, after taking the Bachelor's Degree, never returned to take a second course and the higher degree of Doctor. These circumstances constituted a strong inducement for the colleges to concentrate the annual period of instruction into as short a time as possible, both for the purpose of increasing the number of students who could afford the means of attending, and the number who would take the second degree. Under these influences, the first thirty years sufficed to cause the Bachelor's Decree to be abandoned by all the schools, the number of professors in each to be doubled, and the length of the annual college term to be shortened one-third. And in twenty-five years more, from the same causes, aided efficiently by the fact that the degree conferred by the colleges became practically recognized everywhere as a sufficient *license* to practise medicine in all its departments, the colleges, with but one exception worth noting, had each from five to seven professorships occupied by as many different professors, and an annual college term of only from twelve to sixteen weeks, during which the students, in one class, listened to from five to six lectures daily, on as many different topics, besides attending to dissections and clinical instruction when such were accessible.

The requirements for the Doctor's Degree were, three years study with some regular practitioner of medicine, including attendance upon two such courses of college instruction, the one being chiefly a repetition of

the other; the writing of a thesis on some medical subject; the possession of a good moral character; the attainment of twenty-one years of age; and the sustaining of a creditable examination in the several branches of medicine at the close of the second course of lectures. It will be seen that, in these requirements, there was no reference to any standard of preliminary education to be attained by the student before entering upon his professional studies, except the ability to write a thesis. The requirement of a knowledge of the ancient languages, and the writing of a thesis in the Latin language, were abandoned on the full reorganization of the University of Pennsylvania, in 1792. The relinquishment of natural philosophy, natural history, and botany, as requirements, soon followed, leaving only the single indirect trace of any non-professional education, to which we have alluded. Under these conditions and tendencies, by the end of the second thirty years of our history, the number of medical colleges had increased from five to forty-one; the number of students attending them from six hundred and fifty to twenty-five hundred, and the ratio of those graduating, each year, from less than one in six, to one in three.

Here we see a system of medical colleges originating spontaneously to supply the wants of a free and rapidly increasing people, and open to the most free and unrestrained rivalry, actively developing two apparently opposite results. In one direction, the schools justly vie with each other in increasing the number of their professors in full consonance with the rapid advancement of medical sciences; they sagaciously seek out and enlist the services, as teachers, of the most learned, eloquent, and industrious men to be found in the profession; they spend time and money freely in filling laboratories, anatomical rooms, and museums with all the means for efficient teaching and illustration. So far their free rivalry has reference only to their office as teaching bodies—institutions for imparting instruction—and is productive only of the highest good to the profession and the people. But the anomaly consists in the fact that, at the same time, the same institutions have been rapidly shortening their annual courses; cutting off all collateral requirements; failing to grade the branches of medical study, as they have increased in number and extent, so as to adapt them to the several years of pupilage; and even reducing the final examinations to the simple process of asking a few oral questions in the mysterious "green room." This most unfortunate tendency of our experiment in permitting the freest rivalry in the establishment of medical schools, results directly and necessarily from the fact that the degrees they confer, and the diplomas they give, have been permitted throughout the whole country, with only a few temporary exceptions, to have all the force and effect of a license to practise medicine. It requires but a moderate familiarity with the motives that govern human actions, to see clearly that in a country where there are no entailments of estates, and where the great body of young men who seek the profession are without pecuniary fortune, and largely dependent on their own industry for the means of education, as well as reputation and fortune in after life, the question "where can I get the degree of doctor, which is equivalent to a license to practice, and a full admission into the ranks of the medical profession in the shortest time, and consequently with the least expenditure of time and money?" exerts a very great, if not controlling influence, in determining where the student shall attend his college instruction.

Not that medical students are a whit less conscientious, in their desire

to fully qualify themselves for the responsible duties of our profession, than those who seek any other calling in life. But present necessity, or even convenience, easily controls when there comes with it the flattering thought that, at another time, after having earned a little money by practice, all deficiencies can be supplied by a season of reviewing in a school of the largest facilities. Just on this half-unconscious delusion, hundreds are induced to go where the requirements in time and money are least, regardless of all other advantages. The medical college in a country village, remote from all facilities for clinical instruction in hospital or dispensary, and but scantily supplied with subjects for dissection, can issue to its graduates just as large a diploma, couched in just as unintelligible Latin, and having much the same influence with the people, as the school in a metropolitan city, where its students can have the largest facilities for clinical and practical study. Hence it is not strange that, before the end of the seventh decade of the past century of our existence as a nation, about forty medical colleges had been organized, only sixteen of which were so located as to afford their students any proper facilities for clinical instruction; and that these sixteen were attended by little more than one-third of the whole number of those who attended medical colleges.

The general acceptance of the college diploma as full admission into the profession, thereby uniting in the hands of the same men the business of teaching and the power of licensing, has continued to the present day; and is wholly responsible for the fact that, while we have sixty-three medical colleges to-day, one third of them are so located that they can afford their students no advantages for clinical instruction worthy of mention; and all, except three or four, still attempt to crowd instruction in all the departments of medicine upon the attention of mixed or ungraded classes, in annual college terms of from sixteen to twenty weeks, and exact only two such strictly repetitional courses for graduation. This state of things, in regard to our medical schools, is made still worse by the fact that, during the century under consideration, the system of private medical pupilage has undergone a complete change. At the beginning of that period, as we have already seen, the private study, under a master, was a protracted and serious work, and the resort to the college was simply to review and more fully illustrate that work. But steadily, as medical colleges increased in number, as populations became more dense, and as steamboats and railroads increased a thousand fold the facilities for travel, the work of private pupilage relaxed. Indentures of medical students, as pupils to the more noted practitioners, long since ceased, and the relations of student and preceptor have become merely nominal in practice, in nine cases out of ten consisting in little more than the registry of the student's name in the doctor's office, permission to read the books of his library or not as he chooses, and the giving of a certificate of time of study for the student to take to the medical college where he expects to graduate.

The relative position of the private pupilage and the collegiate studies has undergone a complete practical reversal. The latter, instead of reviewing and supplementing the former, has become the student's chief reliance for the acquisition of medical knowledge; and hence, to have maintained its adaptation or adjustment to the needs of the profession, should have not only increased the number of its professors and its means for communicating knowledge, but also the length of its annual courses, and the division or gradation of its classes in accordance with their period of study, and in proportion to the greatly enlarged field of medical know-

ledge to be acquired. And such would have been, to-day, the grand result worked out by our experiment of self-originating, self-sustaining, and unrestricted competition in the establishment and maintenance of medical schools, had they been restricted to their only appropriate function as institutions for imparting medical instruction and advancing medical science, instead of being hampered and perverted from their natural course by assuming the office of licensing institutions. And could this incubus be removed to-day, another quarter of a century would not pass before every medical college in our country would have its annual course of instruction extended to six months; its curriculum and classes so graded that the attention of each student should be restricted to such branches as are adapted to his period of advancement in study; and nine-tenths of all our medical students would be in attendance on those colleges only that could afford proper facilities for full clinical and demonstrative instruction.

Let the only question presented to the mind of the student, when choosing the college he shall attend, be where can he most certainly obtain that amount and variety of medical knowledge which will enable him successfully to pass the examination of an independent board of examiners, acting under liberal and enlightened rules and modes of testing the student's knowledge, and we shall have nothing to fear either from the number, or the rivalry, of our medical schools. The injurious tendencies of our system of uniting the work of teaching and that of licensing to practise, in an unlimited number of independent medical colleges, was seen at an early period, and clearly pointed out; and by none more clearly and forcibly than by some of those engaged in the colleges themselves.

So true is this that the Legislatures of some of the States, in organizing and regulating their respective State Medical Societies, made some ineffectual attempts to lessen the evil by legislation. This was particularly true in South Carolina, Maryland, Delaware, Massachusetts, and New York. As early as 1839, at an annual meeting of the Medical Society of the State of New York, the following resolution was reported by a committee, and, after free discussion in the Society, was adopted by a vote of fourteen to four: "Resolved, That the right of teaching ought to be separated as much as possible from the power of conferring degrees or licenses." The following year a committee, consisting of Drs. James R. Manly, of New York City, T. Romeyn Beck, of Albany, and John McCall, of Utica, submitted to the same Society an able and interesting report on the whole subject of medical education, in which occurs the following cautious but significant language: "But in view of the diploma becoming depreciated by the rapid establishment of new schools, it may well become a question deserving serious consideration, whether at no distant period the rights of teaching and licensing should not be *disjoined*. An incidental difficulty to the adoption of this suggestion, is the fact that we are surrounded by institutions in other States, which might or might not follow it, and thus our students be induced to desert our own colleges."[1]

In 1837, the same view was advocated by some of the ablest members of the profession in Philadelphia, and they proceeded so far as to organize an institution for the purpose of examining candidates and of conferring degrees, wholly independent of the business of teaching. A petition

[1] See Transactions of the New York State Medical Society for 1840.

signed by one hundred and twenty-six physicians, residents of that city, was presented to the Legislature of the State, asking for a charter giving legal effect to their organization, but the charter was not granted and the project failed.

In 1844, the subject was again brought prominently under discussion in the meeting of the New York State Medical Society, by him who now addresses you. The discussion of the same subject was continued in the annual meeting of 1845, and resulted in the call issued by that society for a convention of delegates from all the medical colleges and societies in the United States, which was held in New York, in May, 1846, and from which originated the American Medical Association. I make these historical allusions to show that neither those engaged in medical teaching nor the profession at large have been unmindful of the evil to which I have attached so much importance. Yet it still exists in all its force. That the colleges have failed to keep themselves adjusted to the needs of the profession in regard to the length of their annual courses of instruction, the systematic classification of the branches included in their curriculums, the corresponding grading or division of their classes, and the exaction of a reasonable standard of preliminary education, has been still more fully appreciated and acknowledged.

Not only is this appreciation indicated by the criticisms in our medical periodicals and the discussions in our medical societies, and by the more general efforts of the colleges, since 1850, to increase the number of their professors, and the fulness of their curriculums, by piecing out their annual courses of instruction with two or three weeks of preliminary lectures at the beginning, and short spring courses at the end of the regular terms—all of which the student might attend or not as he chose—but in a still more formal manner by the proceedings of two or three conventions of delegates from the various schools, in which all the defects here stated relating to preliminary education, inadequate length of the annual courses of instruction, and the urgent need of a systematic division of the branches taught into groups appropriate for each year of study, and the consequent grading of the classes, with annual examinations of each class, have been fully stated, and a thorough plan of remedying them devised and urgently recommended to the schools for their adoption.

The first of the conventions to which I allude was held in Cincinnati, in May, 1867, and was presided over by the learned Prof. Stillé, of the medical faculty of the University in whose buildings we are assembled to-day. The second was held in the city of Washington, in May, 1870, and was presided over by the justly distinguished head of the faculty of the Jefferson Medical College of this city. A third convention of less formal character was held in this city, in June of the present year. If you ask me why these reasonable and highly important recommendations have not been adopted by the greater part of the schools, I can only point you for answer to the paragraph already quoted from the report on medical education made to the New York State Medical Society, in 1840, by that learned trio composed of Beck, Manly, and McCall. Or, more directly, to the fact that while the faculty of each school frankly acknowledges the defects in adaptation to the present enlarged field of medical science and art, and the urgent needs of the profession, each waits for the other to move first, lest, by placing higher requirements upon the time and resources of the student, it should cause its own halls to be deserted for those of its less exacting neighbor.

The efforts in this direction, however, have not been entirely fruitless

of practical results. In 1859, the Chicago Medical College, now the Medical Department of the Northwestern University, was organized for the express purpose of testing the practicability of establishing a school with a thoroughly graded and consecutive system of instruction. Its curriculum was made to embrace thirteen professorships, arranged in three groups, one appropriate for each of the three years of study required. The students attending were correspondingly divided into three classes, junior, middle, and senior. Each class was required to devote its time thoroughly to the group of branches and lectures belonging to its year of advancement in study, and to be examined fully in those branches at the end of the college term. Each of the three courses was continued six months of the year, and actual attendance on hospital clinical instruction, and practical work in the chemical, anatomical, and microscopic, or histological laboratories, was added to the requisites for eligibility to graduation. The very satisfactory success of this institution during the past fifteen years, and its present prosperity, certainly demonstrate the practicability of the scheme.

In 1871, the medical school of Harvard University, one of the oldest and most influential medical institutions in our country, also adopted a fully graded system of instruction, dividing her classes, and extending her courses of instruction throughout the collegiate year, and has continued this plan to the present time, adding annually to the perfection of its details, and adding also to her own prosperity and influence. The new medical school of Syracuse University, New York, has practically adopted the same scheme; and the annual announcements of several other medical schools, for the present year, including some of the most influential and important institutions in the country, give unmistakable evidence of their having taken initial steps in the direction of this most desirable change.

But our medical schools, aided by the work of the private preceptor, do not constitute the whole educational force or influence operating upon the profession in this country. Our social or society organizations, City, County, District, State, and National, have exerted, throughout the whole period of our history, a most potent influence over the educational interests of the profession. We had intended to sketch briefly the history of the more important of these organizations, but must not trespass further upon your time and patience on this occasion. It is sufficient to say that these society organizations, by their annual reunions of the profession in almost every State and important city in our wide domain; by the opportunities they afford for the presentation of papers, and comparison of views in free discussion, not only in relation to scientific and clinical matters, but in relation to every interest whether of medical science or medical polity, have exerted indirectly an influence over the progress, the education, and the general character of the profession, hardly second to that of the organized schools. And if the profession, as represented in these social organizations, would boldly move on in the exercise of its own most important right and duty—by fixing for itself both the standard of preliminary and professional education necessary for young men to attain before admittance into its ranks, and by appointing the necessary tribunals for enforcing that standard, thereby leaving the colleges to their only legitimate work, that of imparting instruction—the latter would speedily adjust themselves perfectly to the actual educational needs of the profession.

Because we have endeavored, in reviewing the progress of our educa-

tional institutions during the past century, to indicate what we regard as their most important defects, let no one infer that we belong to that class, who, in any degree, undervalue the past work or present influence of our medical schools. We do not agree with those who would compel every young man who desires to spend his life in alleviating the sufferings of the sparse populations of our mountains and prairies, to spend the best part of his youth in the vain endeavor to become an adept in the native languages of Cicero and Hippocrates; nor would we discourage the laudable ambition of that large and most valuable class of our young men, who are largely dependent on the proceeds of their own labor for their education, as well as for their advancement in after life, nor foster the indolence of the more wealthy by copying the five, six, or seven years of professional collegiate study from the institutions of Europe. Neither do we agree with those who regard the establishment of a National professional school by our general government, or the endowment of State colleges, with professors salaried out of public funds, as the panacea for our ills. To invoke the patronage of either National or State Governments, in this country, in the regulation and support of medical schools, is simply equivalent to asking that the lobbies of every legislative body in the country shall be annually filled with the log-rolling satellites of every "pathy" and "ism" of the day, that the professorships of our colleges shall be transformed into political sinecures, and that the colleges themselves shall become the foot-balls of partisan politicians.

On the contrary, we believe that our system of medical college instruction, with all its defects, has developed in strict consonance with the spirit of a free and enlightened people, in the comparatively short period of one hundred years, a body of medical men as enlightened, enterprising, industrious, and self-sacrificing, as faithful to the interests of the people, and animated by as noble an *esprit de corps*, as is to be found in any country on the globe. And not only so, but that these same schools have done their full share in creating and sustaining that progress in medical science and literature, which has been demonstrated to this Congress so fully in the most intersting Addresses to which we have listened, on the various departments of medical science and art.

Let us, then, in the same self-reliant, independent spirit, which actuated those who founded, and with the same untiring zeal and generous emulation which characterized the host of others who have sustained and developed, the medical institutions of our country thus far, endeavor to manage wisely the high trust they have left us. Let us neither be blinded by reverence for the past, nor be fretful with impatience because clearly perceived evils will not flee at our bidding, nor yet, with childish weakness, call on the Hercules of government to do our work for us; but let us with boldness yet persevering steadiness of purpose, carry forward our educational organizations, both collegiate and social, enlarging their foundations, improving their adjustments, and adding to their superstructure; and we shall thereby most certainly enable those who come after us in celebrating the next centennial anniversary of our national progress, to review our work with the same pleasure and profit that we derive to-day, in contemplating the works and characters of those whose names are an honor to the century which has just passed.

ADDRESS ON THE MEDICAL STAFF OF THE UNITED STATES ARMY, AND ITS SCIENTIFIC WORK.

BY

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GENTLEMEN: The Centennial Medical Commission having requested the Surgeon General of the Army to depute an officer of the Medical Staff to occupy an evening during the session of the International Medical Congress by giving some account of the scientific work of the Medical Department of the Army, the Surgeon General has designated me as the officer to undertake the task, and, in obedience to his commands, I am here to-night to comply with the invitation of the Commission.

In the discharge of this duty, I wish first of all to explain, as briefly as possible, how it happens that a certain class of scientific work, intimately connected in its nature and results with the progress of practical medicine and surgery, is undertaken by the Medical Bureau, and I hope to make it clear to you that the kind of work to which I refer is of national importance, and that it can be better and more economically done by the Medical Bureau of the Army than in any other way. I shall, then, endeavor to lay before you in a sketchy way, for a single evening will not suffice to go into any branch of my subject in detail, the character of the work already done, and of that actually in progress, with some hints as to matters which might profitably be undertaken in the future, did the means placed at our disposal permit.

It is the duty of the Surgeon General to provide for the maintenance of the health of the Army, as well as for the humane treatment of its sick and wounded. The first duty is, if possible, more important and more delicate than the second, and both are rendered much more difficult to discharge efficiently than might be anticipated in view of the small number of men in our little army, by the comparatively great number of separate military posts, and by the vast extent of the territory over which they are scattered.

The total strength of the Army, officers and men, is, after all, less than twenty-eight thousand, hardly the strength of an army corps of the army of the great German Empire, but it is broken up into little detachments, so that there are nearly two hundred permanent garrisoned posts, and almost half as many more detached parties, of variable strength, requiring medical care. I think it quite safe to affirm that the actual strength of the army might be increased tenfold without materially increasing the labor of its medical administration, provided the number of posts and detachments remained the same as now.

These posts and detachments are scattered over all parts of our wide territory. They are most numerous in the great interior wilderness, remote from the comforts and facilities of more settled communities. Many of them on the southern coast and the Mexican frontier are exposed to frequent visits of epidemic yellow fever, the scourge of our southern

seaboard, which, as late as 1867, destroyed in a single season nearly one per cent. of the whole strength of the army of that year. Moreover, the troops stationed on the Indian frontier are most of the time exposed to all the hardships and privations of a state of actual war. I know that a great statesman has recently declared that Indian conflicts are not war, and that it has been decided that gallantry in these bloody struggles shall no longer receive the poor reward of brevet rank; but the bullet wounds that are received are none the less dangerous because they are inflicted by a savage foe, and the privations and hardships of campaigns in the wilderness against a well-armed enemy who greatly out-numbers our little force, have precisely the same tendency to produce disease and death that the same conditions would exert could they exist in a war against a civilized foe. In a recent disaster, which is only too fresh in the memory of my hearers, about one per cent. of the whole strength of the army, among them two medical officers,[1] perished in a single day. Let us hope that such a catastrophe may never occur again; but similar incidents, though the total loss in each case was less, have happened before, and almost every year since the close of the civil war has had its share of victims fallen in obscure but bloody conflicts.

Now, it is self-evident that the officer charged with the medical and surgical care of any army must keep himself informed as to its sanitary condition; as to the number and character of the cases of disease actually occurring, and the circumstances under which they are developed; as to the number and character of the wounds, and the circumstances under which they are sustained. This information would be indispensable to the intelligent action of the Surgeon General of the Army, even were he charged with no other duties than the assignment of the medical officers and the purchase and distribution of the medical supplies required for the treatment of the sick and wounded; it is still more necessary if he is expected to supervise the sanitary condition of the army with a view to the suppression, as far as possible, of all preventable diseases. In the army of every civilized nation in modern times, therefore, the medical officers actually serving with troops are required to keep a record of the cases of sickness and wounds under their care, and to report them at stated intervals to a central bureau. It is, furthermore, self-evident, that if these reports are at all numerous or voluminous, as they must be, either if the army is large, or if it is scattered in numerous detachments, the Surgeon General will be physically unable to perform the task of personally examining them with any thoroughness, and must assign this duty to others, distributing it among a sufficient number of competent assistants, who must analyze the reports received, and bring to the attention of the Surgeon General such facts and deductions as require his official action. In other words, the Surgeon General of an army, to act efficiently, requires a personal staff of medical officers who enjoy his confidence, and who must be competent to analyze thoroughly the various reports assigned to them, and, if necessary, to advise intelligently on any matters as to which their opinions may be required by their chief.

In our own army, the system of reports established for the purposes just indicated is briefly as follows:—

The medical officer of each post or station is required to keep a register of the sick and wounded under his care, in which each patient is entered

[1] Assistant Surgeon George E. Lord, United States Army, and Acting Assistant Surgeon James M. De Wolf.

by name, with the date at which he is taken on sick report; the diagnosis of his disease or injury, and the termination of the case, being subsequently entered in appropriate columns. From this register a monthly report of sick and wounded is prepared, and forwarded to the Surgeon General from each post or station. It contains a statistical statement of the number of cases of each kind of disease or injury under treatment at the commencement of the month, the number taken on sick report during the month, and the number of each recovered, died, discharged the service, or otherwise disposed of, and the number remaining under treatment at the close of the month. The report also includes a list of the deaths and discharges from the service on surgeon's certificate of disability, giving in each case the name of the soldier and the cause of death or discharge, and has a place reserved for any necessary explanatory remarks.

These reports, consolidated and analyzed at the Surgeon General's Office, keep the Surgeon General constantly informed as to the health of each individual post, of each military department, and of the whole army; acquainting him with the actual amount of sickness, its nature, mortality, and relationship to strength, with the number of wounds and injuries, their character, mortality, and the localities at which they have occurred. The deaths and discharges for disability are recorded in alphabetical registers for convenient reference, and subsequently serve to give necessary information to the accounting officers of the Government, and especially to the Commissioner of Pensions, in case discharged soldiers, or the heirs of the deceased, make application for pensions or other allowances due under the laws of the land. The medical officer of each post or station is required, besides, to make special reports of interesting cases, giving the full histories of such as appear to him of sufficient importance, and in case of the outbreak of epidemic diseases, giving monthly, besides the usual statistical report, a list of all the soldiers attacked, with the date of attack and the date of recovery or death. In such epidemic outbreaks the medical officer is required to investigate the cause of the outbreak, to ascertain, if possible, the mode of introduction, if the disease is introduced from without, and to record the circumstances under which it has originated if it appears to have resulted from local causes. These investigations serve as the basis of a special report, which is required of the medical officer in charge of any post or detachment in which an epidemic outbreak may occur.

As intimately connected with the question of the causes of disease, the medical officer in charge of each post is required to keep a meteorological register, a transcript of which must be furnished to the Surgeon General at the close of each month. Moreover, at the close of each year the medical officer of each post is required to make to the Surgeon General a general sanitary report on the health of the post, with remarks on the diseases which may have prevailed, and suggestions, if any occur to him, as to the improvements or changes which in his opinion may be advantageously introduced, with the view of improving the health of the garrison.

Injuries, wounds, and surgical operations are made the subject of separate special reports, giving the names of the soldiers, and the particulars of each case.

Medical officers examining recruits are obliged to record the name and physical description of each recruit examined, and in the case of

rejected recruits, the cause of rejection. A transcript of this record is furnished to the Surgeon General at the close of each month.

Now, I think, it will be understood without argument, that such a mass of reports as I have just indicated, embracing annually the observations and experience of several hundred well-educated medical men, stationed at as many different places, must contain a great deal of information which, besides its administrative value, must have no little significance in connection with the interests of progress in scientific medicine and surgery. I wish, on the one hand, to insist that all these reports are indispensable from the mere administrative point of view. Without them, and especially, I may add, without their intelligent analysis, the Surgeon General of the Army, whatever his personal ability, would be but a figure-head, powerless for any efficient interference in behalf of the health of the army. On the other hand, I wish also to insist on the fact, that by a very small additional expenditure of labor, clerical force, and money, beyond what would be indispensable for the intelligent administrative use of the facts collected, they can be made to subserve a much wider purpose, and the usefulness of the work, which is indispensable to the army itself, for the sake of which primarily it is done, can be vastly extended so as to benefit the medical profession at large, and, through the medical profession, the sick and injured in every walk of life.

These considerations would afford a sufficient motive for much of the scientific work that has been done by the Medical Bureau in the past, and for much that is still in progress; but there are other circumstances to be considered of equal, indeed, as I think, of even greater significance.

A few years ago our country was convulsed by a great civil war which lasted for four long years. During this time about a million of men were constantly under arms in defence of the national flag; about two hundred thousand of them died of disease, about one hundred thousand of wounds. The Confederate armies, though somewhat smaller, were numerous enough to resist effectively till the very close of the struggle. Their losses can hardly have been much less than those of the national armies. After the close of the war the national armaments were disbanded; the great general hospitals, several hundred of which were in operation, were closed as rapidly as the patients under treatment could be provided for, and it became necessary, for administrative reasons, to order that as each was closed its books and records should be sent to the Surgeon General's Office in Washington. In that office there had also accumulated during the war a vast quantity of reports of sick and wounded, and other records which had been required for administrative purposes during the war. The records thus accumulated embraced over sixteen thousand folio volumes of manuscript record books, and, I suppose, several tons of manuscript reports and papers. Urgent administrative reasons demanded that these records should be examined, classified for convenient reference, and, to a certain extent, that their contents should be analyzed. The pension laws of the United States are so framed that it became necessary, in almost every one of the hundreds of thousands of applications for pensions, to apply to the Surgeon General for the hospital history of the soldier concerned. Subsequently, liberal laws were adopted granting artificial limbs and other prothetic apparatus to those who had lost limbs or been otherwise mutilated, and trusses to those who had been ruptured during the war. This bounty was to be dispensed under the direction of the Surgeon General, and again the records of his office came into requisition, both to protect the

Government against fraudulent claims and to secure the rights of honest applicants. For all these strictly administrative purposes a vast amount of clerical work became necessary, and that this work might effect its end, it was absolutely necessary that it should be directed at every step by competent medical officers.

Now, it became evident from the very first, that by the expenditure of a comparatively small amount of additional labor in the supervision of this work, by a comparatively small increase of the clerical force, and other expenditures absolutely required for mere administrative reasons, results might be attained the publication of which would be beneficial to mankind, and it seemed as though it would have been indeed a great national crime to have lost this opportunity to utilize, in the service of humanity, the experience which it had cost so much blood and so many tears to acquire.

The work of analyzing these records for the double purpose just indicated was entrusted to the very same medical officers who have charge of the current work of the bureau to which I referred a little while ago. Even if no intention of publication had been entertained, these officers would have required for the efficient discharge of their duties, supposing them to have been limited to mere administrative ends, the use of a good consulting library; and if they had simply preserved for subsequent comparison the pathological specimens with which special medical and surgical reports were frequently accompanied, and without which these reports would often be only partially intelligible, an Army Medical Museum would have gradually been built up, however slow the process might have been.

At the time the war broke out nothing deserving the name of a Medical Library existed in Washington; and the incipient medical libraries in other cities, even at such great medical centres as Philadelphia, New York, and Boston, were exceedingly deficient in the very class of books which were most frequently needed for consultation. Indeed, these medical libraries, built up by the voluntary contributions of a generous profession which numbers but few wealthy men in its ranks, were deficient in every direction, and, had the best of them been situated in Washington, it would but imperfectly have satisfied the necessities of the case. There was, therefore, no hope of supplying the want unless the Surgeon General's Office should be able to accumulate a library of its own. In like manner, the correct interpretation of many of the pathological specimens received implied the ability to compare them with others, such as ought to be found in a General Pathological Museum. But no considerable pathological museum existed in America. Small museums had indeed been created by the industry and liberality of the professors of several of the medical schools in the great northern cities, but these had been founded with special reference to the needs of the elementary instruction of medical students, and contained no wealth of accumulated material such as is often needed for purposes of comparison in arriving at the intelligent interpretation of an individual case. Such museums had been built up in the older civilized countries of Europe for the most part by Government aid, and without Government aid it was and still is impossible that any considerable collection of the sort should ever grow up in America. It was evident, therefore, that if the officers on duty in the Surgeon General's Office were to enjoy the advantages of consulting such a collection, it must be created by the office itself. Now, here again, it was clear from the first that by a small addi-

tional expenditure beyond what would have been necessary to create such a library and museum as would be thoroughly efficient for the merely practical purposes of the Medical Bureau, a National Medical Library and a General Pathological Museum might be created, which would meet a want long felt by every medical man in America who has ventured into the domain of original research.

The considerations hitherto presented are, let me hope, sufficient to make it plain that it was the bounden duty of the Surgeon General's Office to undertake that scientific work of which enough has already been published to enable the medical profession throughout the world to form a judgment as to its character, and as to the fitness of the medical officers to whom it has been intrusted to accomplish the task which they have undertaken. The medical criticism of the old world has already proclaimed its verdict as to both points in an outspoken manner, and I will not be deterred by any false modesty, on behalf of my colleagues or myself, from making the statement that the verdict has been altogether favorable. As for the medical profession of our own country, my fellow-citizens—so many of whom I see around me to-day—how shall I find words to express my feelings? Your generous sympathy and encouragement have sustained us from the very first. Many of you served with us during the war, and aided in accumulating those vast stores of experience which we are now endeavoring to utilize. We were so fortunate as to secure your co-operation at the beginning, and your support has never failed us yet; I believe it never will, if we continue to deserve it. It is largely owing to your influence that the National Legislature has been induced to supply us with the funds, without which our work could not have been carried on; and however scanty these funds have appeared to some of us, when compared with the sums devoted by other civilized governments to similar purposes, we have never forgotten how liberal the appropriations have been when compared with any ever made before by our own National Government for any medical purpose. Not merely have you constantly strengthened our hands, and supported us in our endeavors to accomplish our work, but, ever since the close of the war, you have labored with the National Legislature to secure for the officers of the medical staff a reasonable share of rank and pay, and these efforts were at last, during the recent session of Congress, successful in securing legislation which, if it did not do for us all you had hoped, at least placed us on a better footing than we had ever hitherto enjoyed. It is, therefore, altogether an appropriate thing that I should explain, as I am now doing in your presence, the character of the work we are endeavoring to perform.

I have now to present to you, as briefly as possible, some details with regard to the nature and extent of the scientific work of which I have been speaking; but first, let me remark that the observations and reports which form the basis of this work are due to the intelligence and industry of the whole medical staff. This fact must never be lost sight of in estimating the character and importance of the work. It represents no mere individual observations and opinions, but the conjoined and systematic labor of a considerable body of trained and trustworthy medical men. The officers entrusted with the labor of digesting these observations, and preparing them for the press, have constantly endeavored, in all their publications, to give full credit to their brother officers at the various military posts, whose original labors are the foundation of all the scientific work that has been done, and of all that can be done at the Surgeon General's Office. I refer you confidently to any of our publications in

illustration of this statement. This work of the officers of the Medical Staff deserves the appreciative recognition of all who are interested in the progress of Medical Science. Not merely have they made with alacrity the observations and reports required of them in the discharge of their military duties, but their voluntary labors have been meritorious and important. Those of you who practise medicine in cities, and enjoy all the stimulus of converse with intelligent fellow-laborers and of competition for success, can hardly realize the difficulties under which most of the work of these medical officers is done, at remote and usually isolated stations, or amidst the hardships and perils of campaigns in the wilderness against the Indians, with no special stimulus to investigation except the love of science and desire for usefulness.

Next, I have to remind you that the work of analyzing these observaions, comparing them with each other and with similar observations in other lands, preparing the results of these studies for publication, and seeing them through the press, has been from the beginning to the present time, with the exception of two or three publications, to which I will refer specifically hereafter, performed by but three medical officers; and it may be of interest to those who love to see economy in administration above all other things, for me to state that these three officers have received as remuneration for their labors during the last ten years nothing more than the modest emoluments belonging to the rank of Assistant Surgeons in the Army. One of them was, it is true, promoted to the grade of Surgeon a few months ago, but this was simply the accidental consequence of his place on the Army Register; the others remain as before. The three officers to whom I refer are my colleagues, Drs. Billings and Otis, and myself.

Now, it would not be just to these officers if I did not state that they have never been, and are not now, free to devote their whole time to the scientific work entrusted to their care—each is also charged with certain administrative duties which demand a portion of his time and attention.

Thus, my colleague, Dr. Billings, besides the charge of the Library and various special duties connected especially with the sanitary condition of barracks and hospitals, has always had to do a good deal of administrative work in connection especially with the purveying of medical supplies and the accountability of medical officers for supplies issued to them.

My colleague, Dr. Otis, besides his duties as Curator of the Army Medical Museum, as the officer in charge of its Surgical, Anatomical, and Miscellaneous sections, and as the officer entrusted with the preparation of the Surgical History of the Civil War, has had administrative work to do in the way of furnishing information from the Surgical records with regard to the numerous applications for artificial limbs and prothetic apparatus, and other current work connected with the Division of Surgical Records.

For myself, besides the charge of the Medical, Comparative Anatomy, and Microscopical sections of the Museum, the Chemical Laboratory of the Surgeon General's Office, and the preparation of the Medical History of the Civil War, I have had charge, since its organization, of the Record and Pension Division of the Surgeon General's Office, a purely administrative bureau, which for several years employed about one hundred clerks, and which still has thrust upon it ample work to employ the same number, although parsimony in legislation has reduced the force to less than half, so that the work is rapidly falling behindhand. Some notion

of the responsibility attaching to this bureau can be formed when I state that, since 1865, the hospital histories of about three hundred thousand deceased or disabled soldiers have been traced through the record books and furnished to accounting officers of the Government, especially to the Commissioner of Pensions, and the insufficiency of the clerical force now authorized, will be appreciated, when I state that between twelve and thirteen thousand such applications are lying unanswered on the table of my chief clerk as I speak these words.

Now, you will readily understand that the administrative duties to which I refer must not merely increase the responsibilities, but must necessarily occupy a portion of the time of the officers concerned, with the inevitable effect of diminishing the amount of scientific work which it is possible for them to perform. Yet my experience in the office has convinced me that it would not be wise to relieve these officers of this administrative responsibility and throw it upon a different set of officers. The same records are to be used both for administrative and scientific purposes, and the double work can best be performed under the same heads. The most economical, and at the same time the most efficient means of diminishing the burdens of these officers, and of increasing both the quantity and the value of the scientific work which it is possible for them to perform, would be to provide them with a really adequate clerical force, and with a sufficient number of competent assistants. I do not speak to-night in any complaining spirit, but it would be worse than useless to pretend that adequate clerical and other assistance has ever been supplied to us. This is a naked fact, which it is my duty to make known, and which you must constantly bear in mind in appreciating the amount of work which we have as yet published. That under the circumstances we should have been able to do as much as we have—that since the close of the war we should have been able to prepare for publication the various works with which you are acquainted—the publications of the Surgeon General's Office, edited by us and already issued, constituting over six thousand quarto pages[1]—could only have been achieved by tireless industry and a genuine devotion to our task.

Coming next to the details of this scientific work, I may remark that a portion of the reports received at the Surgeon General's Office which are susceptible of scientific treatment, refer to the conditions under which the soldier lives. Here belong the meteorological reports and a large portion of the annual sanitary reports.

Prior to the civil war, the meteorological observations made by the medical officers of the army were published from time to time by the Surgeon General's Office, the greater portion having been edited by the lamented Coolidge. All students of the Climatology of the United States are familiar with these works, which long furnished the only reliable information with regard to the climate of our western territories, and, indeed, contain information with regard to many districts which is still the only information printed.

During the civil war the fierce necessities of the struggle caused the suspension of these observations, but they were resumed after its close, and, at the present time, observations of the temperature three times a day, the rain-fall, appearance of sky, and direction and force of the winds, are recorded under the direction of the medical officer of every permanent military post, and barometrical, and other special observations, at

[1] See Appendix.

selected posts. Since the Signal Service has been conducting its extensive meteorological observations in the more settled portions of the country, however, it has been thought by the Surgeon General that the interests of science, as well as of economy, would be best subserved if he should discontinue the publication of the meteorological observations made by the medical officers of the army, and accordingly an arrangement has been made by which the monthly means and other portions of the meteorological reports needed for reference in the Surgeon General's Office are briefed in convenient books, and the original reports are then turned over to the Signal Office, for use in conjunction with the observations made under the supervision of that bureau.

The annual sanitary reports received from the several military posts, since the close of the war, have served as the basis for the preparation by my colleague, Dr. Billings, of two bulky volumes, which contain descriptions of the several military posts, their barracks and hospitals, embracing a good deal of new information with regard to the medical topography of many parts of our country, especially the western territories, and also containing abstracts, prepared under my own supervision, of the sickness and mortality, and of the meteorological conditions, of the various posts for several years.

Another portion of the reports contains information with regard to the physical characteristics of the men who become soldiers; I refer to the monthly reports of the officers entrusted with the physical examination of recruits. These reports contain the name, nativity, former occupation, age, social condition, height, weight, chest measurement, complexion, and color of eyes and hair of each recruit examined, and in the case of rejected recruits, the nature of the disability on account of which enlistment is refused. These reports, if subjected to scientific treatment at stated intervals, say every five or ten years, would give valuable hints as to the improvement, deterioration, or stability of the physical characteristics of our population. I know that a very great number of these recruits are of foreign birth, but, as this is carefully specified in each case recorded, there would be no difficulty in the way of the separate analysis of the facts with regard to the native born, and the total number of observations which have accumulated since the war is large enough to be of considerable importance. The two splendid volumes recently published by Colonel J. H. Baxter, U. S. A., recently Chief Medical Officer of the Provost Marshal General's Bureau, and now Chief Medical Purveyor of the Army, contain an analysis of a vast number of similar facts observed by the examining surgeons in the course of the several drafts made during the civil war. I am clearly of the opinion that the analysis of the subsequent observations would possess sufficient scientific value to warrant the comparatively small amount of labor and expense requisite to do the work and publish it; but I regret to say that the office has not yet been able to secure enough clerical aid to assign the necessary force to this work, and nothing has as yet been done in this direction. I must hope that it may be possible in the near future to undertake the analysis of the reports received during the last ten years.

Still another portion of the reports received contains information with regard to the diseases and injuries from which our soldiers suffer while in actual service. Here belong the monthly reports of sick and wounded, the special reports of cases, the reports relating to epidemic diseases, and the various reports of wounds and surgical operations.

The monthly reports of sick and wounded are analyzed and consoli-

dated by districts as soon after their reception as possible, and constantly furnish information as to the actual condition of the health of the army. From the statistical tables produced by their consolidation the brief statements made by the Surgeon General in his annual reports are prepared. They have also furnished the data for the abstracts published in the volumes prepared by Dr. Billings, mentioned a few moments ago.

With these exceptions, these valuable statistical tables remain unpublished. I am decidedly of the opinion that they contain a great deal of information much too useful to be lost. Indeed, I think, if the Surgeon General's Office could obtain the necessary force of clerks and assistants to make an annual publication of these reports, accompanied by the accounts of interesting medical and surgical cases received during each year, it would be an important contribution to medical science; but with the means at our disposal, and the other demands made upon us, this has been hitherto quite out of our power. The special reports upon epidemics have, however, been utilized to some extent in the publication of two works prepared by myself, on the Epidemic Cholera and Yellow Fever in the Army during the years 1866 and 1867, and my colleague, Dr. Otis, has prepared a large volume, published in 1871, containing a report of 732 surgical cases treated in the Army from the close of the war to the date of publication. Thus the material in question has at least been partially utilized, but I think that the portion which as yet remains unpublished fully equals in value, and certainly exceeds in quantity, that which has thus far been printed. In connection with the subject of epidemic diseases, just alluded to, I ought also here to mention the investigation of the Epidemic Cholera which in 1873 afflicted the civil population of the United States. By an act of Congress the Surgeon General was directed to detail a medical officer of the army for this investigation, and Assistant Surgeon (now Surgeon) Ely McClellan, U. S. Army, was designated for this purpose. This officer made an elaborate report, which was published as a Congressional document. With it was printed a bibliography of cholera, prepared at the Surgeon General's Office by Dr. Billings, which is, I believe, the most complete list of the works on this subject that has yet appeared.

Perhaps it would be most convenient, in this connection, to say a few words with regard to the Chemical Laboratory of the Surgeon General's Office. The creation of this Laboratory was one of the necessities of the civil war. Immense purchases of medical supplies were being made, and it very frequently happened that efforts of the most ingenious kind were employed to defraud the Government. Moreover, among the great number of medical officers in service, there were many who were inexperienced, and some who were ignorant, and complaints were often made as to the quality of the medical supplies issued which were not justified by the facts. Under these circumstances it was constantly necessary that the Surgeon General should have the opinion of a chemical expert on whose advice he could rely, and it was this that led to the organization of the Chemical Laboratory. Soon other bureaus, especially the Subsistence department, began to apply to the Surgeon General for advice in the matter of supplies purchased or about to be purchased, and it is safe to affirm that the Laboratory of the Surgeon General's Office was the means of saving hundreds of thousands of dollars to the Government. The Laboratory was established on the most economical basis, its personnel never having exceeded two individuals, a chemist and an assistant, both employed as acting assistant surgeons. Their time has been almost

exclusively occupied in the examination of supplies about to be purchased for the army, or of supplies already purchased, with regard to which complaints had been made. If this Laboratory could be more liberally provided for, it might have a much wider field of usefulness. Scientific questions of a chemical character constantly arise in connection with the medical service of the Army, which might then profitably be investigated.

I have next to present a few remarks with regard to the Medical and Surgical History of the late Civil War. The project of such a work was conceived early during the struggle, suggested, no doubt, by the volumes published by the British Government on the Medical and Surgical History of the War in the Crimea. As early as June 9, 1862, the intention to prepare such a work was announced to the Medical Staff by a circular from the Surgeon General's Office, and all medical officers were requested to co-operate in the undertaking by collecting details of cases, and other material for the work. The supervision of the work of collecting surgical material was assigned, by the same circular, to Dr. J. H. Brinton, at that time Surgeon U. S. Volunteers; that of collecting medical material was assigned to me. Dr. Brinton was relieved from this duty in October, 1864, and his place supplied by my present colleague, Assistant Surgeon Otis, U. S. Army, then Surgeon U. S. Volunteers, who, for the three years previous, had served continuously in the field as Surgeon of the 27th Mass. Regiment of Infantry. Both my colleagues and myself did our best to collect material for the work in view, and our efforts were constantly sustained and encouraged by the Surgeon General.

In November, 1865, the Surgeon General issued a document, now well known as Circular No. 6, which was accompanied by two somewhat detailed reports, prepared by Dr. Otis and myself, "On the extent and nature of the materials available for the preparation of a Medical and Surgical History of the Rebellion." The object of this circular was to acquaint the medical profession with the character of the materials collected, and to secure, if possible, favorable public opinion in behalf of the legislation necessary to provide means for preparing the work and completing its publication. This merely preliminary document was widely circulated, and effected the end for which it was designed, Congress, in July, 1866, making an appropriation for the preparation of plates and illustrations for "A first part of the Medical and Surgical History of the Rebellion;" but it was not until March 3, 1869, that Congress at length authorized the actual printing of five thousand copies of the first part of the work.

Meanwhile, anxious that the information collected on some of the more important surgical points should be made public, the Surgeon General published, in 1867 and 1869, two important monographs prepared by Dr. Otis; the first on Amputations at the Hip-joint, the second on Excisions of the head of the Femur for shot injury.

The labor of preparing the first part of the Medical and Surgical History for publication had now so far advanced that it went to press soon after the necessary authority was obtained, and in December, 1872, the first part of the Medical and Surgical History was issued from the Government Printing Office. This part consisted of a medical volume, a surgical volume, and an appendix, the whole occupying 1800 pages quarto. The Medical volume, edited by myself, was devoted to the medical statistics of the war. Some necessary explanatory remarks were offered in the introduction to this volume, but detailed comments on the

statistics were reserved for the second part of the work. The appendix consisted of the reports of the Medical Directors of armies and departments, and of other medical officers, which were believed to be of historical value. The Surgical volume, edited by Dr. Otis, contained a chronological summary of losses in battles and engagements, and a detailed discussion of the statistics and reports relating to special wounds and injuries of the head, face, neck, spine, and chest.

It was not until June, 1872, that legislation was obtained for continuing the publication of the work. Congress then passed an act for its completion and publication, in two parts, of eighteen hundred pages each, in addition to the parts already published.

In accordance with this legislation the publication of the second part of the work was commenced, and the Surgical volume of my colleague, treating of special wounds and injuries of the abdomen, the pelvis, the back, and the upper extremities, has, as you know, been brought successfully to its completion, and was issued from the Government Printing Office during the present summer. This volume contains 1024 pages quarto. I had hoped that its companion Medical volume, which will embrace a discussion of the symptoms, pathology, and treatment of those diseases which were the chief causes of the sickness and mortality among our soldiers during the war, would have appeared simultaneously. In this hope I was doomed to disappointment. In the winter of 1873 and 1874 my health unexpectedly gave way, and for several months I was incapacitated for any literary work. For a long time subsequently, I was obliged in accordance with the counsel of my medical advisers, to limit my hours of labor to a minimum, so that, with all my other duties, I made during the two years which followed my first illness but little progress on the Medical History. I am thankful to say, however, that my health appears at length fully restored, and I trust, when my labors in connection with the Centennial come to a close, that I will be able to resume the printing of the volume and to bring it to a conclusion in a reasonable time.

The third part of the History is expected to consist, like the others, of two volumes. The Surgical volume will treat of the wounds and injuries of the lower extremities; of luxations and fractures from other causes than gunshot; of burns, scalds, and frost-bites; of generalities on gunshot wounds, amputations, etc.; of the use of anæsthetics; of the materia chirurgica, and of the transportation of the wounded. The Medical volume will treat of the hospital system inaugurated during the war, the system of medical supplies, and other kindred matters.

Before passing to another topic, I desire to say a word with regard to the work of my colleague, Dr. Otis, on the Surgical History. How painstaking and accurate it has been, how laboriously he has collected the former experience of other lands to throw light on our own, and to aid in its interpretation, is well known to all of you who have looked into his volumes. But I wish to remind you, in connection with the statements I have made, as to the time required for the preparation of this work, of the extent of the data to be handled as compared with those of any former war. The previous surgical histories of wars, published under government auspices, were that prepared by Dr. T. P. Matthew, on the Surgery of the English Army in the Crimean War, and those of M. Chenu on the Surgery of the French Army in the Crimea, and during the Italian War. Dr. Matthew had to deal with 12,166 surgical cases; Chenu with 40,586 for the Crimean, and 19,590 for the Italian war.

The reporter for the American war had to deal with the reports of over 270,000 cases of wounds. Moreover, my colleague undertook a task comparatively new in military surgery, which certainly appears to me of the greatest importance. In the British and French reports, when a wounded man was invalided or pensioned his history was terminated. Dr. Otis attempted to trace all the more important cases down to the date of publication, and this the organization of the Pension Bureau enabled him to do successfully in the majority of instances. At the time this plan was determined upon, it was believed to be altogether new. It is now known, however, that Professor Hannover, of Copenhagen, had conceived a similar plan, and applied it in the case of the invalids of the war of 1864.

Similar investigations have since been made by Lœffler, Mossakowsky, and Berthold, though in connection with a much smaller number of cases than occupied my colleague.

I cannot dwell longer to-night on the Medical and Surgical History of the War of the Rebellion. I pass to a few remarks concerning the Library of the Surgeon General's Office. I have already indicated the reasons for the commencement of this Library. The first appropriation of money for the purpose was made by Congress not long after the close of the war. For two or three years ten thousand dollars, subsequently, however, only five thousand, were appropriated annually. This has been expended with great economy and discretion, under the supervision of my colleague, Dr. Billings. A great many books have also been acquired by exchange, or have been presented to the library. As a result, this library now contains about 40,500 bound volumes and 41,000 pamphlets. Although, for obvious reasons, under the charge of the Surgeon General, this library is essentially the Medical Section of the Library of Congress. For several years the Librarian of Congress has not purchased medical books, leaving that branch of the subject to the Surgeon General's Office. In 1874, the Government Printing Office issued a small edition of a Catalogue of Authors, prepared by Dr. Billings, which formed three quarto volumes of about 2000 pages, similar in style to the Catalogue of the Congressional Library. This Catalogue is in the possession of every public medical library in the country, and is very useful, as far as it goes, to students who desire to avail themselves of the resources of the library in connection with medical researches of any kind. The library itself is thrown open to all medical men who choose to use it.

Although far from complete, this library is now one of the great medical libraries of the world; and right here I wish to say, that even double the money expended upon it would not have made it as valuable as it now is but for the generous assistance of the profession of this country. Many of our physicians have ransacked their own libraries to supply us with works which, in many instances, we could not otherwise have obtained, especially the older American books and journals. The extent to which this has been done is the best proof of the general desire of the medical profession of the United States that this library should speedily become a complete library of reference on medical subjects.

I know that a few individuals have objected that Washington is not the best place for a great medical library, and seem to think it a pity that it cannot be established in Philadelphia or New York, where the number of physicians is so much larger; but the usefulness of such a library is not to be measured by the number of casual readers, but by the number of original investigators who consult it in connection with their own work.

It is only through the work of these men that a great library becomes generally useful to the whole profession. These original investigators are comparatively few in any city, but almost every city has some of them. Wherever the National Medical Library might be, the majority of the medical investigators of the country would have to travel to consult it. Until very lately, such men had to travel to Europe for the same purpose. It is important that this should no longer be necessary. It is important that there should be in at least one place in our country a complete medical library, and this we will make at the Surgeon General's Office, if we can get the means. There is no probability that such a library will ever be built up in the United States except by Government aid. This is also what has happened in all other countries. If we could secure for the purposes of the library exclusively an annual appropriation of ten thousand dollars, we could in a very few years make this library the most complete in the world.

One point further I desire to mention. To give the library already collected the highest degree of usefulness, a subject catalogue is now imperatively demanded. Dr. Billings has undertaken such a work. With dauntless industry he has not merely arranged the cards representing the separate treatises in the library according to their subjects, but he has undertaken to make separate cards for all the original essays contained in all the medical periodicals in the library. About 275,000 such cards have already been prepared. He now desires to obtain the authority to have the Government Printing Office publish a Subject Catalogue prepared from these cards. A specimen fasciculus of this catalogue has already been issued, and most of you who are specially interested in medical bibliography have already seen it. Dr. Billings estimates that the catalogue, if completed on the plan of this fasciculus, would make about five volumes of a thousand pages each. He is abundantly willing to undertake the labor if Congress can be induced to authorize the printing of the work. An appeal will be made to Congress on this subject at its next session, and if the medical profession of the country agree with me, that the publication of such a catalogue would be a work useful to every medical investigator in the land, I feel sure that by an expression of their views they can secure the success of the undertaking.

Lastly, I have to say a few words with regard to the Army Medical Museum. I have already indicated the circumstances under which it originated. It has now acquired very considerable extent, containing very nearly 19,000 specimens. The specimens collected during the civil war were almost all illustrations of military surgery and of camp diseases. But, since the war, it has begun to acquire a broader scope, and we now aim to make it a National Medical Museum. The collection now embraces, besides the Medical and Surgical sections, a Microscopical section, sections of Human and Comparative Anatomy, and a Miscellaneous section.

The section of Comparative Anatomy is yet in its infancy, and almost all the specimens have been collected by medical officers of the army, so that it has cost altogether but a few hundred dollars. There are about 1500 skeletons and crania, almost all of American vertebrates, in this section. A check list of them, prepared by Dr. H. C. Yarrow, has recently been published in pamphlet form. Those interested in this special subject can obtain copies of it at the Hospital of the Medical Department of the Army at the Centennial grounds.

The section of Human Anatomy is chiefly remarkable for its collection

of human crania, which now number about 1600. These chiefly represent the native American races, including the prehistoric mound-builders, and the various modern tribes, from the Esquimaux at the North to the Patagonians at the southern extremity of South America. The careful measurement of these crania, under the supervision of Dr. Otis, constitutes a genuine addition to the science of ethnology. An abstract of these measurements has been prepared by him, and recently published in pamphlet form. Ethnologists can obtain copies at our hospital on the Centennial grounds, or of the Surgeon General.

Catalogues of the Medical, Surgical, and Microscopical sections were published in 1867. That of the Surgical section was prepared by Assistant Surgeon A. A. Woodhull; that of the Medical section by myself; and that of the Microscopical section by Assistant Surgeon E. Curtis. Since these catalogues were published the collections have doubled in size. The publication of a catalogue of the additional specimens, or a revised catalogue of the whole, would be a useful work.

It gives me pleasure to state that the interest taken in this collection by the medical profession of the country is being annually more and more frequently displayed by the presentation of valuable medical and surgical specimens from all parts of the country. Such specimens are always acceptable, and especially, I may remark, that those which have served as the basis of original communications published in the medical journals have especial interest. You can all appreciate how useful it would be; how many misunderstandings would have been prevented if the actual specimens which have served for certain pathological descriptions could have been somewhere preserved and accessible to the criticism of subsequent investigation. But no medical museum that is not national in its character will ever serve as the central point for such a collection.

The actual appropriations of money for our Museum have never exceeded five thousand dollars a year. The sum is not a liberal one, and much more could be advantageously expended if it could be obtained.

The Centennial Medical Commission, in giving the invitation to which I have responded to-night, expressed the wish that I would endeavor to represent to your eyes in some way the character of some portion of our collection. In compliance with their request, I have naturally selected my illustrations from the Medical and Microscopical sections, which are under my own immediate direction. I have made use of photography for the purpose, and Mr. Edgerton, of J. W. Queen & Co., 924 Chestnut street, has kindly undertaken to exhibit the pictures on the screen. First, I exhibit a series of photographs, novel, I think, in character, representing certain pathological conditions of the intestinal canal.

[Twenty-five lantern slides, representing pathological alterations of the intestinal mucous membrane, were here exhibited, and briefly commented on.]

My other illustrations are taken from the Microscopical section. This section was originally established for the purpose of preserving the specimens acquired in the study of the pathological anatomy of some of the diseases of soldiers. Sections of morbid growths, and other specimens sent to the Surgeon General's Office by the medical officers of the Army for an opinion as to their nature, have been added, with a certain number of other specimens, especially in the direction of normal and pathological histology. The collection now contains about 7000 permanently mounted microscopical slides. A single assistant, paid as a clerk of the first class,

constitutes, under my direction, the whole personnel available for the work.

The process by which the photographs I am about to exhibit were prepared, was, as most of you are aware, brought to its present state of perfection by my own original investigations. A complete misconception exists in certain quarters as to the time I am able to devote to work of this class. It has been simply my amusement—my relaxation from the daily routine and toil of the other labors which I have outlined to you to-night. For years all the work of this kind I have been able to accomplish has been done on Sundays and holidays.

A part of this work was undertaken for the sake of the Medical History of the War.

[Twelve photo-micrographs were here shown, exhibiting perpendicular sections of diseased intestines, the magnifying power with which the slides were taken ranging from 50 to 200 diameters. This was multiplied by 50 by the lantern projection.]

Another portion represents incidental histological work.

[Here fifteen micro-photographs were shown, including nine of blood-corpuscles, to demonstrate the impossibility of discriminating between the blood of man and certain other mammals. These slides were taken with 150 to 1000 diameters.]

Still another portion represents work undertaken in connection with the investigation of attempts to defraud the Government.

[Here fourteen slides, each magnified 150 diameters, were exhibited, illustrating an investigation into the so-called "calf-hair goods," undertaken at the request of the Honorable Secretary of the Treasury.]

In connection with these investigations, the question of the scientific certainty of microscopical appearances has constantly been thrust upon me, and I have been forced to the conclusion that not unfrequently, especially when high powers are used, the false appearances resulting from diffraction and interference have been mistaken for real structure. This would be a melancholy conclusion, were I unable to point out how such misinterpretations may be avoided. It is impossible to do this in detail on such an occasion as this, but I will show you a few pictures in illustration of my meaning.[1]

[The address was concluded by the exhibition of a series of slides representing the real and spurious appearances of certain diatoms often employed by microscopists as test objects, the slides being magnified from 500 to 2500 diameters, and this increased by the lantern fifty times.]

APPENDIX.

Publications of the Surgeon General's Office.

[1863–1876.]

Circular No. 9. War Department, Surgeon General's Office, July 1, 1863. Consolidated Statement of Gunshot Wounds. By Surgeon J. H. Brinton, U. S. Volunteers, 11 pp. 8vo.

Circular No. 15. War Department, Surgeon General's Office, Washington, September 8, 1863. Sickness and Mortality of the Army during the first year of the War. By J. J. Woodward, Assistant Surgeon, U. S. Army, pp. 8, 8vo., with 6 diagrams.

[1] [The descriptions of the several slides would require for their clear understanding the reproduction of the pictures referred to, and are therefore, at the request of the author, omitted.—Editor.]

Circular No. 6. War Department, Surgeon General's Office, Washington, March 10, 1864. Reflex Paralysis. By Acting Assistant Surgeons S. Weir Mitchell, George R. Morehouse, and W. W. Keen, Jr., pp. 17, 16mo.

Circular No. 1. War Department, Surgeon General's Office, Washington, June 10, 1868. Report on Epidemic Cholera and Yellow Fever in the Army of the United States during the Year 1867. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, pp. 156, 4to.

Circular No. 2. War Department, Surgeon General's Office, Washington, January 2, 1869. A Report on Excisions of the Head of the Femur for Gunshot Injury. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, pp. 141, 4to.

Circular No. 3. War Department, Surgeon General's Office, Washington, August 17, 1871. A Report on Surgical Cases treated in the Army of the United States from 1865 to 1871. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, pp. 296, 4to.

Circular No. 4. War Department, Surgeon General's Office, Washington, December 5, 1870. Report on Barracks and Hospitals, with Descriptions of Military Posts. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army, pp. 494, 4to.

Circular No. 5. War Department, Surgeon General's Office, Washington, May 4, 1867. Report on Epidemic Cholera in the Army of the United States during the Year 1866. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, pp. xviii., 65, 4to.

Circular No. 6. War Department, Surgeon General's Office, Washington, November 1, 1865. Reports on the Extent and Nature of the Materials available for the preparation of a Medical and Surgical History of the Rebellion. Medical Report by Brevet Major J. J. Woodward, Assistant Surgeon, U. S. Army; Surgical Report by Brevet Lieutenant Colonel G. A. Otis, Surgeon, U. S. Volunteers, pp. 166, 4to.

Circular No. 7. War Department, Surgeon General's Office, Washington, July 1, 1867. A Report on Amputations at the Hip-joint in Military Surgery. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, pp. 87, 4to.

Circular No. 8. War Department, Surgeon General's Office, Washington, May 1, 1875. A Report on the Hygiene of the United States Army, with descriptions of Military Posts. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army, pp. lix., 567, 4to.

Catalogue of the Surgical Section of the United States Army Medical Museum. Prepared, under the direction of the Surgeon General, by Brevet Major A. A. Woodhull, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1866, pp. 664, 4to.

Catalogue of the Medical Section of the United States Army Medical Museum. Prepared, under the direction of the Surgeon General, by Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1867, pp. 136, 4to.

Catalogue of the Microscopical Section of the United States Army Medical Museum. Prepared, under the direction of the Surgeon General, by Brevet Major E. Curtis, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1867, pp. 161, 4to.

[For Catalogues of the section of Human and Comparative Anatomy, see Centennial pamphlets *infra*.]

Catalogue of the Library of the Surgeon General's Office. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1872, pp. 454, 4to.

Catalogue of the Library of the Surgeon General's Office. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army. Vol. 1, A—L, pp. 1193, 4to. Vol. 2, M—Z, pp. 956, 4to. Supplement, pp. 319, 4to. Washington: Government Printing Office, 1873–4.

Specimen Fasciculus of a Catalogue of the National Medical Library under the direction of the Surgeon General of the U. S. Army, at Washington, D. C. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1876, pp. 72, 4to.

A Medical Report upon the Uniform and Clothing of the Soldiers of the United States Army. By Brevet Major A. A. Woodhull, Assistant Surgeon, U. S. Army. Washington: Surgeon General's Office, 1868, pp. 26, 8vo.

Report on the Pathological Anatomy and Histology of the Respiratory Organs in the Pleuro-pneumonia of Cattle, with 6 lithographs from photo-micrographs. Washington, June 15, 1870. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, pp. 9, 4to. In the Report of the Commissioner of Agriculture on the Diseases of Cattle in the United States. Government Printing Office, 1871.

Report of Results of Examinations of Fluids of Diseased Cattle, with reference to the presence of Cryptogamic Growths. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army, and Brevet Major E. Curtis, Assistant Surgeon, U. S. Army, pp. 12, 4to. In the Report of the Commissioner of Agriculture on the Diseases of Cattle in the United States. Government Printing Office, 1871.

Photographs of Surgical Cases and Specimens, taken at the Army Medical Museum, with histories of 296 cases. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army. Washington, 1866–71. 6 vols., 4to.

Reports accompanied by Photographs of Microscopic Objects. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, viz.:

Report on the Magnesium and Calcium Lights as applied to Photo-Micrography. 6 pp., 4to., 11 photographs. Surgeon General's Office, 1870.

Report on the Oxy-calcium Light as applied to Photo-Micrography. 3 pp., 4to., 2 photographs. Surgeon General's Office, 1870.

Report on an Improved Method of Photographing Histological Preparations by Sunlight. 10 pp., 4to., 11 photographs. Surgeon General's Office, 1871.

Report on the Histology of Minute Bloodvessels. 8 pp., 4to., 11 photographs. Surgeon General's Office, 1870.

Report on the Minute Anatomy of Two Cases of Cancer. 10 pp., 4to., 2 photo-lithographs. Surgeon General's Office, 1872.

Memorandum on Pleurosigma Angulatum and Pleurosigma Formosum. 4 pp., 4to., 8 photographs. Surgeon General's Office, 1871.

Memorandum on Surirella Gemma. 1 p., 4to., 2 photographs. Surgeon General's Office, 1871.

Memorandum on the Test Podura. 3 pp., 4to., 5 photographs. Surgeon General's Office, 1871.

Memorandum on Amphipleura Pellucida. 1 p., 4to., 2 photographs. Surgeon General's Office, 1871.

Memorandum on the Nineteen-band Test Plate of Nobert. 4 pp., 4to., 9 photographs. Surgeon General's Office, 1872.

Four Letters to the Surgeon General, accompanying Photographs of the Mosquito, certain Parasites, the Proboscides of certain Flies, and miscellaneous photographs of Insects and parts of Insects. 8 pp., 4to., 35 photographs. Surgeon General's Office, 1872.

The Medical Department of the United States Army from 1775 to 1873. By Brevet Major H. E. Brown, Assistant Surgeon, U. S. Army. Washington: Surgeon General's Office, 1873.

The Medical and Surgical History of the War of the Rebellion, 1861–65. Prepared under the direction of the Surgeon General. Part I., Vol. I., Medical History, by Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, pp. xliii., 726; Vol. II., Surgical History, by Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, pp. clv., 650. Appendix, containing Reports of Medical Directors, etc., pp. 365. Washington: Government Printing Office, 1870. [Actually issued in December, 1872.] Part II.,

Vol. II., Surgical History, by Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, pp. 1024. Washington: Government Printing Office, 1876.

A Report on a Plan for Transporting Wounded Soldiers by Railway, etc. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army. Surgeon General Office, 1875, 56 pp. 8vo.

Pamphlets issued in connection with the exhibit in the Hospital of the Medical Department, U. S. Army, International Exhibition of 1876, viz.:

No. 1. List of Skeletons and Crania in the Section of Comparative Anatomy of the U. S. Army Medical Museum, for use during the International Exhibition of 1876. By Dr. H. C. Yarrow. Washington, 1876, pp. 52, 8vo.

No. 2. Description of the Models of Hospital Cars. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 10 pp., 8vo.

No. 3. Description of the Models of Hospitals. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 22 pp., 8vo.

No. 4. Description of the Models of Hospital Steam-Vessels. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 12 pp., 8vo.

No. 5. Description of Perot & Co.'s Improved Medicine Wagon. By the manufacturer, 16 pp., 8vo.

No. 6. Description of the U. S. Army Medicine Transport Cart, Model of 1876. By Brevet Lieutenant Colonels D. L. Huntington and G. A. Otis, Assistant Surgeons, U. S. Army, 16 pp., 8vo.

No. 7. Description of Selected Specimens from the Surgical Section of the Army Medical Museum. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, 22 pp., 8vo.

No. 8. Check List of Preparations and Objects in the Section of Human Anatomy of the U. S Army Medical Museum, for use during the International Exhibition of 1876. By Brevet Lieutenant Colonel G. A. Otis, Assistant Surgeon, U. S. Army, Washington, 1876, pp. 135, 8vo.

No. 9. List of Selected Microscopical Preparations from the Army Medical Museum. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 7 pp, 8vo.

No. 10. Description of Selected Specimens from the Medical Section of the Army Medical Museum. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 21 pp., 8vo.

No. 11. Typho-Malarial Fever: Is it a Special Type of Fever? Being remarks introductory to the discussion of the question in the Section of Medicine, International Medical Congress. By Brevet Lieutenant Colonel J. J. Woodward, Assistant Surgeon, U. S. Army, 44 pp., 8vo.

Other Official Publications of the Medical Staff.

Report on Quarantine on the Southern and Gulf Coasts. By Brevet Major H. E. Brown, Assistant Surgeon, U. S. Army. Washington: Government Printing Office, 1873, 42d Congress, 3d Session, Senate, Executive Document No. 9.

Cholera Epidemic of 1873 in the United States. Reports prepared under the direction of the Surgeon General of the Army. A. History of the Cholera Epidemic of 1873 in the United States. By Brevet Major E. McClellan, Assistant Surgeon, U. S. Army, 513 pp., 8vo. B. History of the Travels of Asiatic Cholera. By John C. Peters, M.D., of New York, and Brevet Major E. McClellan, Assistant Surgeon, U. S. Army, 192 pp., 8vo. C. Bibliography of Cholera. By Brevet Lieutenant Colonel J. S. Billings, Assistant Surgeon, U. S. Army, 320 pp., 8vo. Washington: Government Printing Office, 1875, 43d Congress, 2d Session, House of Representatives, Executive Document, No. 95.

Statistics, Medical and Anthropological, of the Provost Marshal General's Bureau, etc. By Colonel J. H. Baxter, Chief Medical Purveyor, U. S. Army, Washington: Government Printing Office, 1875, Vol. I., pp. lxii., 568; Vol. II., pp. xxviii., 767.

SECTION ON MEDICINE.

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TYPHO-MALARIAL FEVER: IS IT A SPECIAL TYPE OF FEVER?

BY
J. J. WOODWARD, M.D.,
SURGEON, UNITED STATES ARMY.

SINCE the earliest times Pestilence has followed in the footsteps of War. It has been the consequence of Ignorance as much as of Necessity. Its causes are to be sought, not merely in fatigues, exposures, and privations necessarily incurred during the performance of heroic deeds; not merely in the morbific influences of strange climates; not merely in the miseries of besieged places; they are equally to be sought in the thousand preventable abnormal conditions to which armies are exposed when huddled together in ill-selected, over-crowded, and filthy camps, fed by ignorance or cupidity on scanty, improper, ill-cooked food, drinking water contaminated by human excretions, and breathing air poisoned by human effluvia. Hence arises a brood of camp diseases more destructive than the improved small-arms or rifled cannon of any foe; which, on memorable occasions in times past, have annihilated whole armies, and yet more frequently decided the fate of campaigns in spite of the military genius of generals or the heroism of their followers.

The humane spirit of modern civilization, revolted at horrors which modern science has shown to be to a great extent preventable, has made of late years earnest efforts at prevention, and the intelligence of modern administration has perceived that the success of such efforts is an indispensable condition to success in war. It is this double motive that has called into existence the Medical Staff, which has become of late years an essential component part of the army of every civilized nation. The work to be done cannot be performed by a band of hired barber-surgeons employed to bind up rudely the wounds received in battle; it cannot be performed by the voluntary assistance of the crowd of educated civil physicians who may be led by motives of humanity to the

scene of suffering, after the mischief is done and pestilence has actually broken out; for its efficient performance it requires the existence of a trained Medical Staff.

To possess the desired efficiency such a staff must be a component part of the army in time of peace. It must enjoy such rank and pay in its higher grades, and offer to its lower grades such prospects of reasonably rapid promotion as to secure the life-long services of the ablest medical men. These must devote themselves in times of peace to the study of the sanitary conditions which affect the health of armies, and war must find them ready to point out with authority the precautions which cannot be neglected without peril. The demands of philanthropy, the impulses of humanity, are best fulfilled by such a staff when it best discharges its military duty, which is to see to it that of a given number of soldiers on the rolls, the largest possible number shall always be ready for duty.

This proper duty of the Medical Staff is as much a military duty as that of the artillery or the infantry. It is the business of these to maim and destroy till the strength of the enemy is so diminished that he can no longer resist; it is the business of those to aid in keeping the fighting ranks full until their work is done. For the efficient discharge of this military duty the medical staff requires military rank as much as any other branch of the army service. It requires it, not merely because it is otherwise impossible to secure the continuous service of an efficient personnel, but because, without military rank, no personnel, whatever its qualifications, can be really efficient. It requires rank, because it requires authority. If its wise counsels can be thwarted at every step by the obstinacy of ignorance, it becomes a mere witness of evils which it is impotent to prevent.

This is not the time or place to discuss the question as to whether the conditions just indicated as necessary to secure the most efficient medical staff possible have yet been completely fulfilled in any army. I must leave that question to the thoughtful consideration of my hearers of all countries. It is enough for my present purposes to point out that coincidently with the elevation of the medical staff in modern times the camp diseases of modern wars have notably diminished in malignancy. We no longer hear of armies destroyed by pestilence within the first few weeks or months after taking the field. The old bubo-plague and spotted typhus no longer figure in the history of wars between civilized nations.

I admit that much remains to be done by Preventive Military Medicine. I admit that much remains to be learned with regard to the causes of disease. But this is no reason why armies should not enjoy to the uttermost the knowledge we already possess. And it is after all but a partial application of this knowledge which has brought about the amelioration to which I have just referred. I think it is safe to affirm that no army ever yet took the field, even in the most modern times, that did not campaign in daily violation of well-established hygienic laws. And hence it will be found that if in any modern war the mortality from disease has been very small, it has been because the struggle has been very brief. We shall deceive ourselves if we overlook the element of time in the production of the diseases of modern armies. An army may very well escape with trifling mortality from an exposure of a few weeks or months to influences which would have proved fearfully destructive if continued for a year or longer.

This was well illustrated by the experience of the Armies of the United States during the civil war by which this country was recently convulsed.

Notwithstanding the want of discipline among the newly-levied troops; notwithstanding the lack of experience in military medicine on the part of the newly-enrolled medical officers; notwithstanding all the faults of administration and the necessity of reform, of which so much has been said in certain quarters, the mortality from disease did not exceed 2.2 deaths per 1000 of strength monthly, during the first six months of the struggle[1]—a death-rate which, if it had continued, would have represented an annual loss from disease of 26.4 per 1000 of strength. But in point of fact a rapid increase in the mortality took place after the first of November, 1861. The deaths from disease during the year ending June 30, 1862, were at the rate of 49.3 per 1000 of strength, and, in spite of increased discipline and increased experience, the reorganization of the Medical Staff, and other reforms, rose to 63.2 per 1000 of strength during the year ending June 30, 1863. During the year ending June 30, 1864, the improved discipline and sanitary management of the white troops were at length accompanied by a diminished mortality, and the death-rate from disease fell to 48.2 per 1000 of strength, or very nearly what it was during the first fiscal year of the war. Subsequently, however, it rose again, in spite of every effort, and was 56.5 per 1000 of strength during the year ending June 30, 1865. These figures, it must be remembered, refer only to the white troops, who constituted the bulk of our forces. They include both the regular army and the volunteer troops. And I may remark that I have shown in the introduction to the first volume of the Medical History of the War,[2] after an analysis of the records of the Surgeon General's office, and a careful comparison with the records of the Adjutant General and the Quartermaster General, that the average annual mortality from disease among the white volunteers, during the four years of the struggle, must be fixed at 55 per 1000 of mean strength, while it was only 32 per 1000 of strength for the small regular army during the same period.

But what shall I say of the mortality among the colored troops? The statistical tables in the first volume of the Medical History show it to have been 211 per 1000 of mean strength during the year ending June 30, 1864, and 139.8 during the year ending June 30, 1865. Even if we extend the view to cover the whole period from the day the first volunteer colored regiment was mustered into service to the day the last was mustered out, a period of five years and four months, and distribute the mortality over the whole term, it will be found, as I have shown in the Introduction to the first volume of the Medical History,[3] that it represents an average annual mortality from disease of 133 per 1000 of mean strength—a proportion which it is impossible to consider without emotion.

It is only just to express my conviction that a large part of this excessive mortality was due to circumstances from which the colored men suffered before they were enlisted, rather than to mismanagement or maltreatment afterwards. It must be remembered that a large part of these colored soldiers were fugitive slaves. They fled into our lines literally naked and starving. The diseases which destroyed them were to a great extent engendered by the miseries they had suffered before they found a refuge under our flag.

[1] These figures and those in the following paragraphs are deduced from the Statistical Tables of the first Medical volume of the Medical and Surgical History of the War of the Rebellion (1861–5). Washington, Government Printing Office, 1870.

[2] Op. cit., Introduction, p. xli.

[3] Op. cit., Introduction, p. xl.

In all—embracing in the count all arms and all colors, officers and men—the total mortality of the armies of the United States from disease during the war (including the deaths during the year following, in which a large part of the mortality was from diseases contracted during the struggle), may be fixed at a little over 200,000 men, while battle and wounds destroyed rather more than 100,000.[1] This estimate of 200,000 men dead of disease embraces, it must be remembered, not only about 30,000 of the colored soldiers, upon whose mortality I have commented, but a nearly equal number of white soldiers, who died while prisoners of war in the hands of the enemy, under circumstances upon which I refrain from comment at this time, but for which the Medical Administration of the United States army can assuredly in no wise be held responsible.

These circumstances no doubt increased the mortality of our armies greatly beyond what it would otherwise have been. On the other hand, it must not be forgotten that discharges from the service for disability took place with a freedom—perhaps in strict justice I ought to say, with a recklessness—never before exhibited in any army. The total number of soldiers thus discharged may be estimated as not far short of three hundred thousand.[2] No doubt very many of these men only went home to die. No doubt, also, many thousands of them, epecially those suffering with lame backs and general debility, rheumatic pains, and cardiac palpitations, needed only a short sojourn in a northern climate, with a generous diet, to have fitted them again for the field. I cannot dwell here on this shameful story. It is one of the scandals of the war. I have never been able to collect data to justify even an approximate estimate of the proportion of these men who died. I believe it to have been comparatively small. Removed from the theatre of war, returned to the healthier circumstances of their homes, I doubt not that a large proportion of these men recovered their health, and that thousands of them, induced by the large bounties which were offered for recruits, found their way again into the ranks.

Certainly the statistics I have cited afford food for earnest thought. But if their study is to be profitable, it must be undertaken with a careful consideration of all the circumstances. Above all, when these figures are compared with the results of other wars, the element of time must be taken into account, or the gravest misunderstandings will arise. A striking illustration of this possibility has been brought to my notice.

In an oration delivered on the 2d of August, 1874, on the anniversary of the Military Medical School of Berlin,[3] Professor Rudolph Virchow,

[1] In the Introduction to the first Medical volume of the Medical and Surgical History of the War, I have fully discussed the various official records, and arrived (see p. xxxviii.) at the following results, which are probably very near the truth:—

Deaths from battle, wounds, and other violence	93,969
Deaths from disease	186,216
Deaths, the causes of which are not recorded	24.184
	304,369

On the reasonable supposition that those deaths in which the causes are not recorded were distributed between wounds and disease in the same proportion as those whose causes are recorded, we shall arrive at the results expressed in the text.

[2] See Introduction to the first Medical volume just cited, p. xlii.

[3] Die Fortschritte der Kriegsheilkunde, besonders im Gebiete der Infectionskrankheiten. Rede gehalten zur Feier des stiftungstages der militar-ärztlichen Bildungs-Anstalten, am 2, August, 1874, von R. Virchow. Berlin, verlag von A. Hirschwald, 1874.

one of the ablest medical thinkers in Europe, alluded to this enormous mortality from disease during our war, and contrasted it with the comparatively small losses of the German army during the late war with France. After reciting the American figures, and, I may add, after speaking of the publications of the Surgeon General's office in language so complimentary that, as one of the officers concerned in their preparation, my modesty will not allow me to repeat it, the distinguished orator continued as follows: "The German army had, during the last French war, out of a strength of 913,967 men, a total loss of 44,980. Of these, 17,572 fell before the enemy, 10,710 died later of their wounds, 12,253 fell a sacrifice to disease and pestilence; certainly a very favorable proportion. But we had before us the experiences of two recent wars, which had been well discussed and taken advantage of, both scientifically and administratively. We possessed the inestimable experience of the Americans, and, finally, we had German science."[1]

Truly, I will not yield even to Virchow himself in my appreciation of German science. I know well the debt of gratitude which modern medicine owes to German investigation. For myself, I have drawn much of the knowledge I value most from German sources. I admire German science for its industry, which is without parallel; for its originality, which has already pushed discovery so far; but most of all, for the truly catholic spirit which knows no nationality, and for the manly independence, which never hesitates to weigh authority by a comparison with the actual facts.

Something of this spirit I have tried to catch, and I will not therefore allow myself to be misled as to the matter under discussion, even by the authority of Virchow. I cannot avoid interpreting the facts otherwise than he has done, and I feel that I am but performing a necessary duty when, standing here before you to-day, I declare it to be my opinion that not even German science would have sufficed to save the German army from a greatly increased percentage of mortality if the war had lasted a few years instead of a few months.

The figures given by Virchow are from the official report of Dr. Engel.[2] They represent a period of seven months. They must not be compared with the losses of our whole civil war, but with the losses of the first seven months. Let us make the comparison. Virchow's figures represent a mortality of 13.4 + per 1000 of strength for seven months, or 1.9 + per 1000 monthly. Now I have already shown you that the mortality from disease in our armies during the first six months of the war averaged 2.2 per 1000 monthly. During the following month, viz., November, 1861, the mortality increased very greatly, so that, indeed, almost as many died during that month as during the whole six months preceding. Including these deaths, I find, on a discussion of the official data, that during the first seven months of the civil war, viz., from May 1 to November 30, 1861, the mortality from disease was at the rate of 18.8 per 1000 of strength, or 2.7 per 1000 monthly.[3] You see that the

[1] Op. cit., S. 7.

[2] Zeitschrift des Königlich Preussischen Statistischen Bureaus. Redigirt von dessen Director Dr. Ernst Engel. Zwölfter Jahrgang, 1872. Berlin, 1872, S. 293.

[3] These ratios are deduced from the Statistical Tables in the first Medical volume of the Medical and Surgical History quoted above. I may add here, that the total number of deaths from disease from the commencement of the war to November, 1861, recorded on the alphabetical registers of the Surgeon General's Office, is 3206. There are also 160 deaths recorded during the same time, the causes of which are not specified. If we sup-

mortality of our army from disease, during equal times, was really but about one-third greater than that of the German army in the French war; whereas, as Virchow presents the subject, it is made to appear more than fifteen times greater.

Now, I am perfectly willing to admit that a part of the actual difference in favor of the German army was really due to better discipline and a wiser application of preventive medicine. But I do not believe that this is the only reason for the difference. The raw levies of the Northern States were sent at the very first to the South, and campaigned or lay in camp during the time in question in the miasmatic valleys of the Mississippi, the Ohio, and the Potomac, while the German army did its work on the comparatively salubrious plains of France. What would have happened had the scene been exchanged? How much of the difference was really due to the wisdom of man, how much to a more favorable climate, and the absence of the intense malarial poison to which all were exposed who bore our arms?

This malarial influence, and the pathological processes to which it gives rise, demand the most careful study of those who would endeavor to comprehend the problem of the health of armies in America. It is not merely manifested by the frequency of ordinary ague, of which very nearly a million cases[1] were officially reported in our armies during the war; it colors and complicates other diseases to an extent which can hardly be credited by those who have not been eye-witnesses to its effects. Especially does it demand the consideration of those who may attempt the study of the fatal continued fevers which have been, and will be, the

pose all of these to have been deaths from disease, the number will be 3366. Of this number, 1457 died during the month of November, viz.: 1413 of disease, and 44 of causes not specified. The absolute number is but a little more than one-fourth of the German figures; but the average strength of the troops in service during the time referred to was also about a fourth of the German army, so that the ratios are as stated in the text. The comparatively small mortality at the commencement of our civil war is strikingly illustrated by the Medical History of the three-months' volunteers. It will be remembered that at the commencement of the struggle President Lincoln called out 75,000 men to serve for three months. This call was issued April 15, 1861. According to the records of the Adjutant General's Office, the actual number of men who were mustered into service on this call was 91,816. Through the kindness of Assistant Adjutant General T. M. Vincent, I have ascertained from the death and disability records of the Adjutant General's Office that the actual mortality of these troops during their three-months' service was as follows:—

Deaths from battle, wounds, and other violence	221
" from disease	151
" from causes not recorded	231
Total	603

If all the deaths of which the causes were not recorded were from disease, the total number of deaths from disease would be 382, or 4.2 per 1000 of strength for the three months. This would correspond to a mortality of 1.4 per 1000 monthly, or less than three-fourths of the monthly rate which I have shown in the text for the first six months of the war.

[1] The number of cases and deaths reported as due to ague was as follows:—

	White Troops.		Colored Troops.	
	Cases.	Deaths.	Cases.	Deaths.
Quotidian intermittent fever	447,258	452	63,992	58
Tertian intermittent fever	375,170	381	51,045	54
Quartan intermittent fever	41,223	84	3,923	15
Congestive intermittent fever	13,673	3,370	2,536	794
Total	877,324	4,287	121,496	921

The grand total for both white and colored troops is 998,820 cases, and 5208 deaths. See the first volume of the Medical History, cited above.

scourge of the camps of every army that operates in the valleys of the Mississippi and its tributaries, or of the rivers that flow into the Gulf of Mexico, and into the Atlantic ocean south of the fortieth parallel.

These camp fevers occupy a conspicuous place among the diseases which produced the mortality of our armies during the civil war. They caused more than one-fourth of all the deaths from disease. In fatality they proved second only to diarrhœa and dysentery. I suppose it is safe to estimate that diarrhœa and dysentery produced about 60,000 deaths in the armies of the United States during the civil war, and camp fevers rather more than 50,000.[1] A right comprehension of these two groups of diseases must therefore be regarded as the most important task of Military Medicine in America.

It is my purpose in the present discourse to discuss certain points connected with the pathology of these camp fevers, and especially the complication of the typhous process by the malarial influences to which I have just referred.

Already in the fall and early winter of 1861 reports began to come in from various quarters that a new form of fever was prevailing in our camps. The great majority of our army surgeons at that time were fresh from civil life. Many of them had been engaged in extensive private practice. Almost all of them were well acquainted with ordinary typhoid fever (enteric fever, typhus abdominalis) as it annually prevails in the Northern States, and it was precisely these men who first called attention to the fact that the fevers they were now called upon to combat differed in many important particulars from those to which they were accustomed at home.

This circumstance was noticed in both the Western and the Eastern armies; but, so far as I have been able to ascertain, official attention was first directed to it in the Army of the Potomac, then encamped just beyond the banks of the river in front of Washington. By an order from the Adjutant General's office, dated December 6, 1861, a Board of Medical Officers was convened for the purpose of visiting the camps of the Army of the Potomac, and inquiring into the nature of the prevailing fever, especially endeavoring to ascertain—to use the language of the order appointing the Board—"whether it is to be considered an intermittent or bilious remittent fever in its inception, assuming in its course a typhoidal type, or a typhoid fever primarily." This Board consisted of Surgeon A. N. McLaren, U. S. A.; Brigade Surgeon G. H.

[1] 44,558 deaths from diarrhœa and dysentery were reported during the war, out of 156,885 deaths from disease, the causes of which were specified in the reports. If we suppose these diseases to have caused the same proportion of those deaths from diseases the causes of which are not specified in the reports, we shall obtain the estimate of the text. The cases and deaths of the several forms of camp fevers reported to the Surgeon General's Office were as follows:—

	White Troops.		Colored Troops.	
	Cases.	Deaths.	Cases.	Deaths.
Typhus fever	2,501	850	123	108
Typhoid fever	75,368	27,056	4,094	2,280
Common continued fever	11,898	147	......	......
Typho-malarial fever	49,871	4,059	7,529	1,301
Remittent fever	286,490	3,853	30,645	1,002
Total	426,128	35,965	42,391	4,691

This gives a total of 40,656 deaths from these fevers out of 156,885 deaths from disease, the causes of which are specified in the reports. On the supposition just mentioned we obtain the estimate in the text.

Lyman, U. S. Volunteers, and Assistant Surgeon M. J. Asch, U. S. Army. It convened December 16, at the quarters of Brigade Surgeon Lyman, who was chief medical officer of the division commanded by General Fitz John Porter; and during its subsequent labors examined the hospitals of this and other divisions, and collected a great deal of valuable information, in writing, by means of written questions addressed to the brigade and regimental medical officers of parts of the army which its members were unable conveniently to visit. As might have been expected, some diversity of opinion was expressed in the replies received. But in their general tenor the great majority of these replies confirmed the opinion formed by the members of the Board on the basis of their own personal observations. This opinion was that, while a certain number of cases of ordinary typhoid fever existed in the army, the large majority of the febrile cases were "bilious-remittent fevers, which, not having been controlled in their primary stage, have assumed that adynamic type which is present in enteric fever."

In the following spring, after the Army of the Potomac commenced its Peninsular campaign, this mixed form of fever increased in frequency. It assumed formidable proportions during the siege of Yorktown, and reached its greatest intensity while the army lay encamped on the swampy borders of the Chickahominy. The hospitals of Washington and Alexandria, of Baltimore, Philadelphia, New York, and other Northern cities, were crowded to overflowing with the sick. Among the attending physicians were some of the best instructed medical men of the Northern States. They have shown that they recognized that they had to deal with an unusual pathological complex, by perpetuating the name of Chickahominy fever, which appears so often in their reports.

It was under these circumstances that I was ordered, early in the summer of 1862, to serve on a Board of Medical Officers, who were directed to revise the form of sick report in use in the army. I was fresh from the Army of the Potomac, which I had accompanied from its camps before Washington, where I had spent the winter with it. I had lain in camp at Yorktown, and followed the army up the Peninsula. I had formed the opinion that the prevailing fevers of the Army of the Potomac were hybrid forms, resulting from the combined influence of malarial poisoning and of the causes of typhoid fever. I believed that individual cases received their characters in accordance as the one or the other of these influences preponderated in the individual, and that very often the picture was still further complicated by the coexistence of a scorbutic taint. Full of these opinions, I proposed to the Board, as I had previously suggested to the Surgeon General, to designate the complex condition in question by the name typho-malarial fever, and I induced the Board to add this term to the list of diseases printed on the blank form for the monthly sick report. I often regret that I did not also urge upon the Board the preparation of a circular letter to accompany the new sick report, explaining why this term had been adopted, and calling for special reports with regard to the cases which it was intended to designate. As it was, the term went upon the sick report without any explanation or a word of comment. But even under these circumstances, 23,346 cases were reported as typho-malarial fever during the following year, showing how widely the opinions I had formed were shared by the medical officers of the army.

In September, 1863, I published in Circular, No. 15, of the Surgeon

General's Office,[1] a short statement with regard to the sickness and mortality of the army during the first year of the war, in which I stated the meaning I designed to attach to the term typho-malarial fever, and in November of the same year completed the publication of my Outlines of the Chief Camp Diseases of the United States Armies,[2] in which I presented my views at some length. I also gave a short account of the subject in Circular No. 6, Surgeon General's Office, 1865.[3] And now, after a careful study of all the pathological and documentary material accumulated during the war, I still hold substantially to the same views, and hope in the second volume of the Medical History of the War to present the facts upon which my opinion is based in such a manner as to command universal assent. In so doing, I shall, of course, present many additional matters of detail not contained in any of my former publications; and also, I may frankly say, I shall correct some errors of detail (particularly with regard to the pathological anatomy of the intestines in these fevers), into which I fell during my earlier studies, and which were incorporated in the account presented in my book on Camp Diseases.

In the same connection I hope to present historical proof of a proposition, which I do not hesitate even now to announce to you, that such hybrids between malarial fever and typhus, of one form or another, are no new thing in the history of armies. They were not first begotten on American soil. In fact, in every great army that ever yet campaigned for any length of time in a malarial region the prevalent form of fever has been a hybrid between malarial fever and some form of typhus. By a malarial region I mean simply a region in which ague and remittent fevers are the common endemic diseases. The causes of these fevers act always with peculiar intensity upon strangers; and an invading army is, therefore, peculiarly prone to suffer from them. At the same time the uncleanliness and overcrowding of camps favor the spread of some form of typhus, whether originating *de novo* or introduced from without by contagion. In some cases it has been spotted typhus which has made its appearance; in others it has been abdominal typhus (typhoid fever), and as the typhus poison has been of one or the other variety, and as the malarial influence has been more or less intense, the complex result has varied—at times the malarial phenomena, at times the typhous phenomena being the more prominent. The morbid conditions resulting from these twofold causes have usually also been still further modified by the existence of actual scurvy, or at least of a scorbutic taint, the result of an improper and often scanty supply of food.

It would be impossible, in the time allotted to me, to re-examine to

[1] Circular No. 15, Surgeon General's Office, Washington, D. C., September 8, 1863. Sickness and Mortality of the Army during the First Year of the War. The language used in this preliminary report was as follows: "Moreover, while a certain amount of uncomplicated enteric and remittent fever certainly did occur, especially at the commencement of the war, the vast majority of the camp fevers of the army were of a mixed character, exhibiting undoubted enteric phenomena, variously combined with the periodicity and other peculiarities of malarial disease, and still further modified by the tendency to incipient scurvy, which is the ordinary concomitant of camp diet. To indicate this mixed nature, the term typho-malarial fever, which I had the honor to suggest to the Department in June, 1862, appears appropriate, and, at the present time, is coming into very general use."

[2] J. J. Woodward. Outlines of the Chief Camp Diseases of the United States Armies, as observed during the present war. Philadelphia: J. B. Lippincott & Co., 1863. 8vo. pp. 364.

[3] Circular No. 6, Surgeon General's Office, Washington, November, 1, 1865, p. 109 et seq.

any great extent the history of armies from this point of view. I can only bring to your notice a few striking illustrations. First, let me recall the pestilential fever which destroyed the French General Lautrec and the army of thirty thousand men with which he besieged Naples, in 1528. Hecker[1] has analyzed the chronicles of the time with his usual eloquence, and has arrived at the conclusion that it was simply petechial typhus which raged in the French camp. After an examination of the authorities he cites, I cannot agree with this conclusion. The siege began May 1. Almost the first act of the French General was to cut the aqueducts which supplied the city with water. By this rash act the plains on which he was encamped, intensely malarial at the best, were converted into a vast series of bogs and stagnant pools, from which the heat of the sun raised clouds of vapor, which hung around his camp like a pall. I must believe that Jovius[2] was right in attributing the destruction of the French army chiefly to this act of madness. No contemporaneous physician has described the pestilence that followed, but the chroniclers of the siege have recorded symptoms which are incompatible with the theory of Hecker. Jovius tells us that many of the patients were affected with swellings of the belly and legs. They became so pale and death-like in their complexions that their friends were hardly able to recognize them. Scarcely able to crawl, and without attendants to nurse them, they often died in their tents of hunger and thirst. Sandoval[3] describes the same death-like faces, and applies to them the epithet *amarillos* (yellow). These are not the symptoms of spotted typhus, but of intense malarial poisoning. We saw just such cadaverous yellow faces, with swelled legs and bloated bellies, crawling about in the camps at Young's Point on the Mississippi, and at other swamp-beset localities, during our own war.

I agree, however, with Hecker, to whose work I must refer you for particulars, that spotted typhus was no doubt also present in the camp; and I suppose it complicated the acute fevers very much, as it did later in the same century in the Austrian armies in Hungary. Moreover, Guicciardini[4] relates that after Lautrec first fell sick, during the month of July, things fell into confusion, the enemy cut off the convoys of provisions, and some of the soldiers actually died of starvation in consequence. These circumstances, I doubt not, also played their part in destroying the French Army. By the second of August there were but four thousand men fit for duty. Lautrec died August 15, and, the mortality still continuing, his successor thought only of flight. This he attempted August 29, but was pursued, and capitulated at Averse a few days later. Crowded into the stables of la Madeleine, the pestilence continued its ravages among the prisoners, and when, a little later, they were released by the conqueror, but a handful remained to find their way back to France.

Next let me ask your attention to the story of the celebrated Hunga-

[1] J. F. C. Hecker. The Epidemics of the Middle Ages. Transl. of Sydenham Soc., London, 1844, p. 212 et seq.

[2] Jovii (Pauli, Novocomensis, Episcopi Nucerini) Historiarum sui Temporis, Tomi II., Florentiæ, 1550–2, Lib. XXVI. I have not obtained access to the original, but find in the Congressional Library a German translation, printed at Frankfort, 1570.

[3] Sandoval. Historia de la vida y hechos del Emperador Carlos V. Pamplona, 1614–18. Part. II., Lib. XVII., § XI., p. 12 et seq.

[4] Guicciardini. Della Istoria d'Italia. Lib. XX., Venice, 1738. I have used the English translation of A. P. Goddard, 3d ed., London, 1763.

rian fever, which for at least three centuries played so formidable a part in the campaigns undertaken by the House of Austria against the Turks. The valleys of the Danube and its tributaries are still the home of agues, remittent and continued malarial fevers, big spleens, and malarial cachexias. Lying further to the north than our own Mississippi valley, the climate is nevertheless such that the marshy borders of the streams and pools give rise to a malaria scarcely less intense than that with which we are familiar in the lower Mississippi. I may refer you to the papers of Müller, Wenmaring, and Lántz[1] for graphic descriptions of the characteristics of the country and of its prevailing diseases in our own day.

Now, in every considerable campaign against the Turks, in which the armies of the German Emperors invaded the Hungarian plains, from the beginning of the sixteenth century to the end of the eighteenth, a form of fever prevailed among the troops so unlike the ordinary European fevers that it has always been known as the Hungarian fever, and so fatal as to occasion the proverb that "Hungary is the grave of the Germans." This Hungarian fever has exercised some of the best medical minds of the last three centuries. It is now generally admitted to have been a hybrid between the endemic remittent fevers of the Hungarian soil and spotted typhus. The attention of civilized Europe was first directed to it when, after the luckless Hungarian campaign of Maximilian II., in 1566, his pest-stricken soldiers returned to their homes. They scattered the contagion of spotted typhus throughout Germany on their way. The disease bred by that contagion was also called the Hungarian fever, but it was simply spotted typhus, and the student of the voluminous older literature of the Hungarian fever finds no little confusion growing out of this circumstance, and no little difference between the descriptions of such eye-witnesses as Thomas Jordan and Tobias Cober, who described the Hungarian fever as they saw it on Hungarian soil, and the descriptions of those who merely observed the spread of spotted typhus in the German towns, and gave it the name of the Hungarian fever because the contagion had been spread by soldiers returning from Hungary.

The army of Maximilian, however, was not the first German army which had suffered from the Hungarian fever. In 1542 a pestilential fever broke out at the camp before Ofen, in the Imperial German army which Margraf Joachim von Brandenburg had led against the Turks. It became still more fatal during the disastrous retreat, and according to Hæser[2] destroyed 30,000 men. Johannes Langius,[3] who accompanied the Count Palatine, Frederick II., in this campaign, as well as in the

[1] Jos. Müller. Die k. k. Militaergraenze, mit besonderer Berucksichtigung der vereinigten Carlstadter, Banal und Warasdiner Graenze. Med. Jahrb. des k. k. oest. Staates. Bd. 35 (1843), S. 89, 231, 361. Bd. 36 (1843), S. 110, 235, 338. F. Lántz. Phys. Med. Beschreibung der Barangaer Gespannschaft in Ungarn. (Same Journal.) Bd. 55 (1846), S. 98, 231, 361. Bd. 56 (1846), S. 99, 221, 349. B. Wenmaring. Ueber die Sumpfwechselfieber. (Same Journal.) Bd. 57 (1846), S. 11, 129. Consult, also, J. M. Minderer. Das Halbdreitagige Fieber (Hemitritæus), in den Sudlichen Provinzen des Russischen Reichs. Hufeland's Journal, Bd. 28, st. 2 (1809), S. 1. Müller, cited above, speaks of this fever as the Hemitritæus Daciæ. Op. cit. Bd. 36, S. 343.

[2] Hæser. Geschichte der epidemischen Krankheiten. 2te Aufl. Jena, 1865. Bd. II, S. 340.

[3] D. Johannes Langius. Med. Epist. Basel, 1554. Epist. 4 (p. 17). In Cura Causonis Chirurgicorum temeritas.

campaign of 1526,[1] has left an epistle, in which he describes this fever in such a manner as to leave no reasonable doubt of its identity. He tells us, also, that the camp surgeons, whose ignorance he pictures with bitter sarcasm, gave the disease the name of Bhreüne (Bräune) on account of the condition of the tongue, which they supposed to be the most essential symptom. According to Hæser,[2] the Chronicler Wintzenberger gave the same epidemic the name, "pestartige Bräune." These names long continued in use among the designations by which the Hungarian fever was known to the vulgar.

The epidemic of 1566 is famous both on account of the manner in which typhus was spread through Europe by the disbanded soldiers, and on account of the classical description of the Hungarian fever by Thomas Jordan,[3] who accompanied the army of Maximilian as his chief medical officer. The spring had been exceedingly wet, and the great Hungarian streams had overflowed their banks; the summer was unusually hot and dry; the German army was scantily supplied with food. When the fever first appeared the army was encamped at Komorn, at the point of junction of the Waag and the Danube—a marshy, intensely malarial plain. It became still more destructive in the camp at Raab, where the Raab and Rabnitz empty into the Danube. Such was the devastation caused by it that Maximilian, though his force when he went into camp at Raab was about 80,000 men, did not venture to take the offensive, and saw the gallant little garrison at Zigeth captured by the Turks after more than twenty vain assaults, without daring to strike a blow in their behalf. In the early autumn he ignominiously retreated with the remains of his army. After the retreat the pestilence was especially destructive at Vienna. The hospitals could not accommodate all the sick, and the dead and dying were scattered through the streets. Throughout all this devastation it was the German troops that suffered; the native Hungarians almost entirely escaped.

The Hungarian fever broke out anew during the siege of Papa, in the year 1597. The Italian allies suffered most. According to Hæser, of more than 8000 of these troops, less than 1500 found their way back to Italy. This is the epidemic described in the admirable work of Tobias Cober.[4]

Again it made its appearance, as we learn from Esslinger and Hæser, among the 12,000 German troops who occupied Hungary under the celebrated General Montecucculi,[5] in the year 1661. The disease broke out in the swampy camp between Komorn and Neuhäusel, and soon brought the fruitless campaign to an end.

Once more it appeared in the army with which Prince Eugene besieged Belgrade in 1717. The Grand Vizier advanced to the relief of the besieged with a great army. The Turks took the offensive, and shut up Eugene, with his army of 60,000 men, in the marshy plain between the Danube and the Save. It was here that the Hungarian fever broke out,

[1] See Hecker. Art. Hungarica febris. In the Encycl. Woerterbuch der Med. Wiss. Berlin, 1838, Bd. XVII, S. 164; with which compare the Dissertation by G. Agats. De Morb. Hungarico. Berlin, 1840.

[2] Op. cit., S. 341.

[3] Thomæ Jordani, Pestis Phenomena. Franckfort, 1576. Cap. XIX. p. 219. De Lue Pannonia.

[4] Tobiæ Coberi. Obs. Med. Castrensium Hungaricarum. Decades tres. (Edition of H. Meibomius.) Helmstad, 1683.

[5] See Mémoires de Montecucculi. Nouv. ed. Amsterdam et Leipsic, 1756. Livr. III., ch. i., p. 321.

accompanied by a fatal dysentery. A large number of men had perished, when the gallant Prince, venturing all on the fortunes of a day, attacked the Turkish army and routed it after a desperate battle. The surrender of Belgrade followed, and the peace of Passarovitz was the result.

Still more terrible were the ravages of the Hungarian fever during the disastrous campaign which the Emperor Joseph II. undertook against the Turks in 1788. Disappointed with regard to the help he had anticipated from Russia, and his troops decimated by pestilence, the feeble campaign terminated in a humiliating retreat. The imperial army at the commencement of the season numbered about 200,000 men; its losses from disease have been estimated at between 30,000 and 40,000.

The symptoms of the Hungarian fever have been described at great length by numerous writers. I can only refer to a few leading points. The patients were seized, usually during the afternoon or evening, with a slight, short chill, followed by a burning fever. This was accompanied by a headache so intense that the vulgar called the disease sometimes the head disease (Haupt-krankheit); the raging brain disease (Hirntobende-krankheit); or the head misery (Kopfwehe). So prominent was this symptom that in the earlier autopsies medical men sought for the cause of the affection chiefly in the encephalon, and, deceived either by the congested appearance of the choroid plexus, or by the presence of vermiform clots of blood or fibrin in the ventricles or in the great sinuses, actually supposed they had discovered worms in the brain, and gave to the disease the designation "Cerebri vermis" or "Hirnwurm." With the headache came on a pain in the epigastrium so intense that the appellations "Herzbräune" and "Herzbreun," were widely employed. Soon the fever was accompanied by a raging delirium; the tongue became dry and covered with a brown coat, from whence the vulgar names "Bräune" and "pestartige Bräune," already mentioned. Hemorrhages from the cracked tongue or from the gums were common. Then, there was also the characteristic typhus eruption of the skin, accompanied by petechiæ, whence the disease was called "Febris Lenticularis," especially by the Italian physicians. In cases which recovered, the favorable crisis usually occurred on the fourteenth or twenty-first day, but fatal cases often terminated much earlier. In some cases gangrene of the extremities occurred.

All these symptoms, together with the unmistakable contagion, certainly point to spotted typhus; but from a very early period, cultivated physicians, like Sennertus, for example, have pointed out that this fever differed in several marked particulars from ordinary spotted typhus. The most striking differences were its tendency to present a decidedly remittent type in its earlier stages, and the accompanying gastric symptoms, bilious vomiting and the like. Even Rulandus,[1] who was disposed to identify the disease with the febris lenticularis of the Italians, was obliged to admit that during its early days it frequently presented, in a decided manner, the type of a simple or double tertian fever. A com-

[1] Martin Rulandus. De Morbo Ungarico. Leipsic, 1610. Consult Cap. I. p. 9, where, after intimating that these remissions are deceptive in their character (falsa specie), the author admits that in the case of Baron Reinhard, who died of the Hugarian fever, he supposed, during the earlier history of the case, that he was dealing merely with a double tertian. For his argument in favor of the identity of the Hungarian fever with febris lenticularis, see Cap. VIII., § XX. p. 376. Consult further, Cap. VIII., quæstio 39, p. 510, for argument against the supposition that the Hungarian fever has truly a tertian or quartan type.

mon form was the semi-tertian, to which the old Greek physicians gave the designation Hemitritæus; and hence the Hungarian fever has sometimes been described as the Hemitritæus Daciæ.

So marked were these symptoms that Sauvages, the great Nosologist,[1] classes the "Amphimerina Hungarica," as he calls it, among the remittent fevers. The learned Naumann[2] goes so far as to declare that the Hungarian fever is at the bottom merely the common summer fever which occurs every year in Hungary, the valley of the Danube, and Southern Russia, and attains malignity only in epidemic years. Among modern epidemiologists, while Ozanam[3] still clings to the notion that Hungarian fever was merely typhus, both Hecker and Hæser[4] express the opinion that it was typhus, modified by the intense malaria of the Hungarian plains.

Side by side with the Hungarian fever a chronic affection occurred among the German soldiers in Hungary, which, like the Hungarian fever, was often spoken of as the Hungarian disease. (Morbus Hungaricus, or Lues Pannonica.) This was the celebrated Languor Pannonicus, or Asthenia Pannonica.[5] It was a profound debility, accompanied with a disgust for food, unwillingness for exertion, diarrhœa, and pains in the back. It attacked large numbers of men, and not unfrequently proved fatal. I suppose it to have been the joint effect of chronic malarial poisoning and a scorbutic taint. Much in the old descriptions reminds me of a certain group of cases of general debility which were common enough in our own war. These, too, were rebellious to treatment so long as the patient remained in the malarial region, but recovered promptly, like some of the cases of Languor Pannonicus described by Tobias Cober, so soon as the patients escaped to a healthier atmosphere and better diet.

Dysentery was another camp disorder which proved fatal in the Hungarian campaigns. It is mentioned by various writers, as for example, by Cober, but does not appear to have attracted as much attention as it probably deserved.

I cannot dwell further in this discourse on the story of the Hungarian camp diseases. I have said enough to indicate that the chief difference between these diseases and those of our own camps during the war of 1861–5 consisted in the prevalence of spotted typhus in Hungary instead of abdominal typhus, which was the form from which our armies suffered. The degree of malarial complication must have been very similar. The extent to which any scorbutic complication existed is difficult now to ascertain. Almost all the writers complain of the food and cooking of the Hungarian camps; but the soldiers would appear to have had fresh meat in some abundance, for one of the constant complaints is that they persisted in cooking it too soon after killing. Scurvy, of a marked kind, however, does not appear to have occurred, and the most significant evidence of the frequent existence of a scorbutic taint is, after all, the constant development in every army, after a few months' campaigning, of numerous cases of the Languor Pannonicus.

[1] F. Boissier De Sauvages. Nosologia Methodica. Amsterdam, 1768, tom. i. p. 327.

[2] Moritz Ernst Adolph Naumann. Handbuch der Med. Klinik. Bd. III., Abth. I. (Berlin, 1831), S. 233.

[3] Ozanam. Hist. Méd. des Maladies Epidémiques. 2me. Edit., Paris, 1835, tome iii. p. 127.

[4] Cited above.

[5] For a description of the Languor Pannonicus, see particularly the work of Cober, cited above. Consult, also, Sauvages, op cit., tom. i. p. 802.

Of late years we have heard but little of this once dreaded scourge, which only now and then attracts the attention of some medical writer whose tastes incline to historical studies. I cannot but believe, however, that whenever a German army goes again into the valley of the Danube, as perhaps may happen before long unless the present disturbances in the Danubian provinces are fortunately brought to a peaceful termination, the old Hungarian plague will once more appear in its ranks—a more formidable foe to be encountered than the sword of the Turk.

I have thus presented to you a couple of striking illustrations of the hybrid disease resulting from the complication of malarial influences with the causes of spotted typhus. Let me next refer to some examples of the similar complication with typhoid fever.

The first instance to which I shall refer is the epidemic of the so-called morbus mucosus, which occurred at Göttingen during the years 1760–61, and which has become famous, less on account of the extent of the mischief done than because of the admirable description which we owe to two teachers in the Medical Department of the Göttingen University, Röderer and Wagler.[1] The summer of 1760 was warm and rainy; the winter which followed was wet, with notable vicissitudes of cold and mild weather. Besides the inhabitants, a garrison of French troops, numbering, with the camp followers, about 8000, was shut up in the town. Röderer and Wagler have drawn a striking picture of the want of food, the uncleanliness, the general misery that prevailed. Already by the middle of July, 1760, intermittent fevers, sometimes of mild type, sometimes, however, of marked malignity, occurred to a degree unusual in that region. During August the intermittents continued to be prevalent, but malignant forms became more numerous, and many of the cases assumed a continued type. At the same time a malignant dysentery made its appearance, and raged with fatal results till the month of November. The intermittents meanwhile had continued, but during the month of November both these and the dysenteries were gradually replaced by the morbus mucosus, which became the prevailing disease, and continued as an epidemic all winter. During April and May, 1761, it was again, to a great extent, replaced by intermittents, but still scattered cases continued to occur during the summer. In the autumn, intermittents again became the common disease, occurring often in a malignant form; and smallpox, which had first appeared during the summer of 1761, assumed epidemic proportions during the winter of 1761–2.

The mucous fever was a continued fever, which in severe cases was often prolonged beyond the twenty-first day, sometimes till after the thirtieth, though fatal cases often perished as early as the ninth. In its earlier stages it usually presented a decidedly remittent type, tertian, double tertian, or semi-tertian being the most common varieties. Sometimes, as the patients convalesced, the continued fever passed into an ordinary intermittent before recovery took place. After the fever was fairly under way it presented many of the symptoms of ordinary typhoid fever. Delirium, frequent, feeble pulse, diarrhœa, meteorism, in the worst cases petechiæ, made their appearance. At the beginning the tongue was furred, and its swollen, red papillæ projected through the fur; it became brown and dry as the disease progressed. Hemorrhages

[1] J. D. Röderer et C. G. Wagler. De Morbo Mucoso. Göttingen, 1762.

from the nostrils sometimes took place, especially about the sixth day; still more frequent were hemorrhages from the bowels. It is a significant fact that the extract of Peruvian bark often proved highly efficacious in those cases in which the remissions were most marked.

The disease derived its designation from the belief that an excessive secretion of mucus from the alimentary canal was its most characteristic phenomenon. This was manifested by the frequency with which mucous vomiting, with or without bile, occurred at the inception of the cases, just as we know it does in ordinary remittent fever. The diarrhœa was interpreted as but another expression of the excessive mucous secretion, and the dead lumbricoid worms which were often noticed in the stools, or during the progress of autopsies, were supposed to have been bred in consequence of the morbid excess of mucus in the alimentary canal. In the autopsies, the closed glands of the stomach and small intestines were usually found tumefied to a marked degree. This was erroneously supposed to be the consequence of an accumulation of the mucous secretion in their interior. Röderer and Wagler have published three remarkable copper etchings of the appearances they observed. The lesions they have figured are not those characteristic of typhoid fever, but of ordinary intestinal catarrh. In one of the autopsies (No. V.) they describe the agminated glands, near the ileo-cæcal valve, as marked with black pigment, quite like the condition often observed in our own war, and described as "the shaven-beard appearance." The mesenteric glands were enlarged. The anatomical evidences of peritonitis were often present. Dysenteric sloughs frequently existed in the colon. Nowhere, however, do I find any description of the bulky tumefaction, ulceration, and sloughing of the glands of Peyer, which is characteristic of typhoid fever. Nevertheless, I am by no means sure that this essential lesion did not exist in some of the cases at least. It must be remembered that the typhoid lesion was not then known as we know it now. Perhaps some of the gangrenous spots, of smaller or greater size, which our authors describe as having been observed in the small intestines of some of their cases, were really of this nature.[1] This appears to me the more probable, because in the case of the very similar epidemic which Sarcone[2] observed in Naples in 1764, and which, in the circumstances under which it occurred, its course, symptoms, and the anatomical lesions observed after death, appears to have been identical with the morbus mucosus of Röderer and Wagler, I read that these gangrenous spots were again observed in the alimentary canal of some cases, and that they occurred chiefly in the small intestines. But even if this interpretation is correct —and I believe it is—I cannot think that the characteristic typhoid lesion was present in all the cases in which Röderer and Wagler made autopsies, or it surely would have received greater attention from those acute observers, and I must think that in part, at least, of their cases, there was no other lesion of the small intestine than a smart intestinal catarrh.

The doctrine of the mucous fever, as taught by Röderer and Wagler, took firm root in the medical mind of Europe.[3] It figures largely in

[1] Op. cit., p. 162.

[2] Michael Sarcone. Istoria ragionata dei mali osservati in Napoli, nel corso dell' anno 1764. Naples, 1764. There is a German translation by Fuesslin, Zurich, 1772, and a French translation by Bellay, Lyons, 1804. Our library contains the German translation only.

[3] Ozanam, op. cit., tome i., p. 257 et seq., gives an excellent abstract of the works of Röderer and Wagler, and of Sarcone.

many of the text-books of the first half of the present century, and you will even find the descriptions reproduced without criticism, under the head of "Mucous or pituitous fever," in the admirable Dictionary of Copeland.[1] Some writers have expressed the opinion that this mucous disease was after all neither more nor less than ordinary typhoid fever.[2] I cannot agree with them. I see in the story, as told by the original observers, unquestionable marks of malarial complications; indeed, also of scorbutic complications. I do not marvel that the Göttingen observers should have devoted sections of their work to the discussion of the relationship of the mucous disease with intermittent fever, and of its relationship with scurvy, and should have arrived at the conclusion, to use their own striking language, that the epidemic which they observed was the corrupted and degenerate progeny of intermittent fever. They thought they saw also a causal relationship between intermittent fever and dysentery, an opinion which I cannot discuss here, but which I must confess I share to a certain extent. No doubt, since their time, the term mucous fever has often enough been applied to simple typhoid fever, and although of late it has been banished from the books, it still survives among certain practitioners who were educated thirty or forty years ago, and I myself have heard it applied by old practitioners to unmistakable cases of typhoid fever within the last five years. The misuse of terms in medicine is, however, a common consequence of imperfect knowledge, and we must not allow ourselves to be led astray by it.

I cannot dwell longer now upon this interesting epidemic. Let me turn next to another illustration, in which the malarial element was still more potent, and with regard to which I need only present a few salient points, because it is so often cited in connection with the diseases of armies that most of the leading facts must be well known to you all. I refer to the Walcheren fever, which decimated an English army in the year 1809.

The previous experience of English armies in the Netherlands had shown the malignant character of the malarial influences which prevail in that region. It had been described in striking language in the excellent work of Sir John Pringle.[3] He had recorded that all the flat region between the rivers Lys and Scheldt and the sea, was marshy and unhealthful, the home of periodical fevers; that a great part of Holland, including Dutch Brabant, was subject to the same disorders, and that the air was worst of all in Zealand. He has left graphic descriptions of the fevers which prevailed among the English troops operating in these low countries between 1742 and 1748. He had observed especially that when the troops were encamped near stagnant waters the "marsh fevers are not only apt to begin with little remission, but, after intermitting for some days, to change into continued fevers of a dangerous nature."[4] Especially was this the case during the summer and autumn of 1748, when the troops were encamped near the inundations of Dutch Brabant. An epidemic of such fevers occurred among them. Pringle writes: "At the height of the epidemic it appeared that both intermittents and remittents, by extending or doubling their paroxysms, frequently changed

[1] James Copeland. Dict. of Pract. Med., London, 1858, vol. i., p. 988.

[2] As for example, W. Griesinger. Infections-Krankheiten. Virchow's Handbuch der Spec. Path. u. Therapie. Bd. II., Abth. 2. Erlangen, 1857, S. 118.

[3] Sir John Pringle. Observations on the Diseases of the Army. 7th Edit., London, 1774, p. 1 et seq.

[4] Op. cit., p. 173.

into a continued and dangerous form, and that most of those we lost died in this way. These men, as we remarked, had a corrupted smell for a day or two before their death, and soon after, their bodies putrefied. Some had petechial spots, though the place where they lay was neither crowded with sick nor too close; and to these spots were added some other symptoms, the same with those of the hospital fevers."[1]

The expedition of 1809 renewed the experiences of Pringle on a more formidable scale. The English army, of between 42,000 and 43,000 men, was quite healthy when it set sail from the Downs, July 28, but as early as the middle of August the number of the sick was so great as to excite alarm. By the 26th the number was 5000. By the 7th of September it amounted to 10,948. According to Sir Gilbert Blane,[2] 26,846 men were sent to hospital in Zealand between the 21st of August and the 18th of November. The expedition was paralyzed by these misfortunes, and after the surrender of Flushing, August 15, was unable to continue offensive operations. In spite of the fact that a large part of the sick who were sent home to England began to improve so soon as they escaped from the pestilential regions in which their diseases had originated, and ultimately recovered, the total mortality was large. It has been estimated at about 8000 men. I must refer you to Hansard's Parliamentary Debates, the Annual Register, the Edinburgh Review, and the Essays of Marshall, Sir Gilbert Blane, Dawson, Davis, and Wright,[3] for the particulars of this disastrous expedition. I can only pause to emphasize a few facts.

The diseases of the Walcheren expedition were diarrhœa, dysentery, intermittents, and a form of fever which began as a remittent, and subsequently assumed a continued form, and which at that time was designated the Walcheren fever, or the Walcheren remittent. Sir Gilbert Blane, who visited the island of Walcheren during September and October, reported to the Government "that he found so great a proportion of the sick to consist of those affected with the intermitting and remitting fevers peculiar to marshy countries, that there could be no doubt that the sickness of the army was owing to that cause." He admits, however, that he found a certain number of cases of a fever which he called "typhus," and of dysentery, particularly at Flushing, where "the prevalence of these two diseases was very remarkable, particularly in one regiment, of which all the medical officers were either absent or dead, and of which the sick, originally affected with the endemic disease, were suffering also from typhus and dysentery in consequence of the want of cleanliness, as well as of proper medicines, diet, and attendance."[4] Borland and Lempriere,[5] two other medical officers serving with the

[1] Op. cit., p. 181.

[2] Sir Gilbert Blane. Facts and Obs. respecting Intermittent Fevers, and the exhalations which occasion them. Med. Chir. Trans., vol. iii. (1812), p. 11.

[3] Hansard's Parliamentary Debates. Series I., vol. 15, Appendix 22 and 23, and vol. 16, Appendix "Papers relating to the Expedition to the Scheldt." The Edinburgh Annual Register for 1809. Edinburgh, 1811, vol. ii., part 1, p. 660. The New Annual Register for 1809. London, 1810, p. 316. Observations on the documents, including the evidence heard at the bar, laid before Parliament on the subject of the late Expedition to the Scheldt. The Edinburgh Review, vol. 17 (1810–11), p. 331. Henry Marshall, Contribution to Statistics of the Sickness and Mortality which occurred among the troops employed in the Expedition to the Scheldt in the year 1809. Edinburgh Med. and Surg. Journ., vol. 48 (1837), p. 305. Sir Gilbert Blane, op. cit. Davis, Wright, and Dawson—works cited below.

[4] Op. cit., pp. 2 and 3.

[5] J. Borland and W. Lempriere. Report on the prevailing Malady among his Majesty's Forces serving in the Island of Walcheren. The Med. and Physical Journal, vol. 23 (1810), p. 183.

troops on the island, reported, in the same spirit, that the malady was "the endemic fever of marshy countries; the effect of miasmata from a soil the most productive in deleterious exhalations of any perhaps in Europe;" stated that it prevailed also among the natives of the island as an intermittent or remittent fever, and that among the British troops it assumed "a character of greater malignancy."

But the most elaborate studies of the Walcheren fever were made by the medical men whose duty it became to treat the sick who were sent home to England. Dawson[1] tells us that the Walcheren soldiers were affected with intermittents in a number surpassing those who were attacked by the continued fevers; that many of the soldiers who were subjects of the continued fever had already labored under the intermittent, and that on the other hand intermittents were common among those who had recently recovered from the continued fever. Davis,[2] whose account of the fever is more elaborate, tells us that at the beginning it assumed the quotidian, tertian, double tertian, or quartan type, but that the most common of all was the double tertian. It assumed, however, the characters of a continued fever of typhoid type as it progressed, with muttering delirium, small rapid pulse, dry, black tongue, sordes-covered teeth, fetid odor, and black discharges from the bowels.

On account of the frequency with which this fever was associated with dysentery, the characteristic lesions of dysentery were frequently found in the colon during the autopsies which were made. But Davis[3] has also recorded the significant fact that "the ileum and jejunum were frequently interspersed with tubercles, inflamed and ulcerated in different parts." This description would seem to indicate beyond doubt that the lesions, which we have now learned to recognize as characteristic of typhoid fever, were frequently present in the cadavers of those who had perished from the Walcheren disease. After a thoughtful study of the evidence, no doubt is left in my own mind that this fever was not, as has often been asserted, simply a malignant remittent, but that it was a genuine hybrid between malarial and enteric fever.[4]

Did time permit, I might bring forward other interesting illustrations from the history of the British Army in the East and West Indies and elsewhere, from the Algerine experience of the French, and from other sources; but I fear that I have already occupied too much of your time with facts of this class. I cannot, however, leave the subject of the experiences of other armies without bringing to your notice an unexpected corroboration of the views I am urging upon your attention, which I find in an essay published by Virchow in 1871 on the fever and dysentery of the German army[5] during the recent war with France, which he contrasts with the similar diseases of our own civil war, as described by me in my book on Camp Diseases, and in Circular No. 6. Virchow hesitates to acknowledge "typho-malarial fever" as a special

[1] G. P. Dawson. Observations on the Walcheren Diseases. London, 1810, p. 70.

[2] J. B. Davis. A Scientific and Popular View of the Fever of Walcheren. London, 1810. See also Thomas Wright. History of the Walcheren Remittent. London, 1811.

[3] Op. cit., p. 192.

[4] Essentially the same opinion with regard to the Walcheren fever has been expressed by J. J. Levick. Miasmatic Typhoid Fever. American Journ. of the Med. Sciences, April, 1864, p. 409.

[5] R. Virchow. Kriegstyphus und Ruhr. Virchow's Archiv., Bd. LII. (1871), S. 1. Note pp. 5 and 30.

group of diseases, and exclaims, with cautious conservatism: "It seems to me we ought to be very careful in this direction." Yet, in the same essay, while contending that the prevalent fever of the German army was simply abdominal typhus, as proved by numerous *post-mortem* examinations made at Berlin on soldiers brought back sick from the front, this cautious but acute observer finds himself compelled by the facts under his eyes to use the following emphatic language:—

"Nevertheless, abdominal typhus affords such numerous diversities in its course that it is in the highest degree imperative to preserve every precaution in the interpretation of individual cases. Especially does the admixture (Zumischung) of the malarial element, which also in this war has shown itself active, by numerous cases of intermittent fever, confuse the picture of the so-called normal course of typhus in a sometimes very deceptive manner."

Now, I must protest that these words represent the very essence of the doctrine I am here to defend, and if the comparatively mild malaria of the plains around Metz was capable of complicating the course of the abdominal typhus which occurred in the German army to a sufficient extent to justify this eloquent language, what think you would the great pathologist have written could he have observed for himself the fevers of our own army in the valley of the Mississippi or by the banks of the Chickahominy?

I have occupied the greater part of my hour with these preliminary matters; but not, let me hope, in vain. I come now to a rapid sketch of the principal facts with regard to the typho-malarial fever of the civil war of 1861–5.

The characters and distribution of the malarial fevers of the region in which that colossal struggle took place have been described in a most original and interesting work by our countryman, Dr. Daniel Drake,[1] who has embraced them under the general designation of autumnal fevers. This term serves very well to express the greater prevalence of intermittents and remittents during the autumn months; but it must be distinctly understood that their occurrence is not limited to these months. In the regions in which they are endemic, they may occur at any season of the year, and their course can by represented by a curve in which the abscissas begin to lengthen early in the spring, and grow longer and longer, till they attain their maximum most generally at some time during September or October, after which the curve rapidly drops to a minimum during the winter. Sometimes the curve presents, also, a slightly irregular elevation during the early spring, justifying the term vernal intermittents, which has often been used; but most generally the increasing frequency of these fevers in the spring simply represents the commencement of the annual rise in the curve which culminates in the autumn. This is well illustrated by curves which I have had constructed to represent the monthly number of new cases of agues and remittents reported in our armies in the Atlantic and Central regions during the civil war. These I hand you for examination, but I cannot pause to discuss them at this time. Doubtless the winter and vernal cases are to be regarded in part as relapses, in part as illustrations of the postponed development of the consequences of previous

[1] Daniel Drake. The Principal Diseases of the Interior Valley of North America. Cincinnati, 1850. The same, second series, edited, after the death of the author, by S. H. Smith and Francis G. Smith. Philadelphia, 1854.

exposure; but, however you may choose to interpret them, I wish to insist upon the point that they occur with much greater frequency than some of the systematic writers would have us believe.

Dr. Drake has shown that in a general way these fevers are most intense in the States that border on the Gulf of Mexico, and gradually diminish in frequency and severity as we go to the north, so that they no longer prevail in epidemic form beyond the 44th parallel, and cease to occur even sporadically at about the 47th. To the southwest, the Cordilleras of Mexico and the Southern Rocky Mountains constitute their boundaries, while in the higher latitudes they cease on the great plains, long before we reach the mountains. On the Atlantic slope they prevail with constantly increasing severity as we go southward from New York, and though they do not occur on the table-lands and higher ground of the Appalachian chain, yet they ascend high up the valleys of the streams which flow out of the flanks of those mountains.

In the volume of the Ninth Census of the United States, which is devoted to Vital Statistics,[1] there is an interesting map which exhibits the distribution of the mortality from intermittent and remittent fevers during the year 1870. Doubtless this mortality, which is but at best incompletely represented in the Census Report, can only be regarded as a very imperfect measure of the frequency and severity of these fevers; but, imperfect as it is, its indications are valuable. It shows in a general way the almost complete exemption of New York, the New England States, and the mountainous parts of Pennsylvania, Maryland, West Virginia, and Virginia, and a gradual increase in severity indicated by an increasing mortality in the river valleys as we go southward from the fortieth parallel. It illustrates, also, in a striking manner, a fact which arrested the attention of Drake,[2] that in various scattered districts, from the influence evidently of strictly local causes, the malarial fevers display greater prevalence and malignancy than they exhibit further south and on a lower level.

It would be altogether foreign to my purpose to enter in the present discourse into any discussion as to the causes of these fevers. I simply urge upon you to-day the great fact of their endemic existence, in some localities more frequently and with greater severity than in others, formidable even to the natives of the soil, but still more formidable to strangers, throughout the whole region in which our great armies operated during the civil war. The characters of the fevers thus distributed are too well known to you all for any description to be necessary here. I need only remind you of the frequent occurrence throughout the Southern States, side by side with ordinary ague, of malignant forms, the so-called congestive chills or pernicious fever; of the severity of the remittent fevers which prevail; of the frequency of big spleens, disordered livers, and malarial anæmias, and of the great frequency with which, in these regions, an intermittent type is impressed on the ordinary acute phlegmasiæ, and even on chronic disorders.

Drake has also presented an interesting account of the distribution throughout the United States of typhoid or, as he calls it, typhous

[1] Ninth Census, vol. ii., Vital Statistics of the United States. Washington, 1872. See, also, Statistical Atlas of the United States—based on the results of the Ninth Census, 1870. By F. A. Walker, 1874; in which the map in question is given on a larger scale, and better engraved, as Plate 42.

[2] Op. cit., First series, p. 704.

fever.[1] This fever is the usual form in which typhus manifests itself in the United States. The spotted typhus of the Old World never appears on our soil except as isolated cases, imported, as "ship fever," into our seaports. Drake has correctly pointed out that while in a general way the typhoid fever of the United States is more prevalent in the Northern than in the Southern States, it nevertheless does occur both sporadically and in local epidemics even in the southernmost portion of our territory. This fact is strikingly illustrated by a map in the volume of the Ninth Census already referred to, which[2] represents the distribution throughout the United States, during the year 1870, of some 24,000 deaths from typhoid fever. In constructing the map a few hundred cases of cerebro-spinal fever were included, but their number is too small to vitiate its value[3] in illustrating the distribution of typhoid fever during the year in question. This map, while exhibiting in a general way a gradual decline in the mortality from typhoid fever as we go to the South, displays also a number of limited areas of high mortality, representing the endemic prevalence of the fever during that particular year in certain localities. Several of these areas of local prevalence are situated in the Southern States. In one of them, in Georgia, which embraces the region drained by the Altamaha and Satilla rivers, typhoid fever caused about one-fifth of all the deaths—a greater mortality than it produced in any part even of the New England States. In another almost equally remarkable area, embracing parts of the States of Mississippi and Alabama, having near its centre the town of Columbus, Mississippi, the proportionate mortality from typhoid fever was nearly as great.

Drake has urged, as one of the distinguishing marks between typhoid fever and the autumnal fevers, that the former is not limited like the latter to a particular portion of the year, "between the summer and winter solstice, but occurs, though unequally, at every season." But he believes that "on the whole, however, they are most prevalent in autumn and winter,"[4] an opinion which has been shared by several careful writers.[5]

Liebermeister[6] has recently compared the statistics of the monthly prevalence of typhoid fever in some of the great European cities, and shown that in London, Berlin, and Basle, the curve which represents the course of the disease is distinctly autumnal in character. "The minimum is in February and April (in the Berlin curve a little later); the maximum is in September and October. (In Berlin, the maximum is in October.)" Munich alone presented an exception, the maximum falling in February. Now, I must say, my study of the sick reports of our civil war inclines me to believe that the autumnal curve observed by Liebermeister represents also the usual annual distribution in this country. I have constructed curves representing the monthly ratio of new cases to strength in each of the three regions. These curves present certain irregularities in the Atlantic and Central regions corresponding to the

[1] Op. cit., Second Series, p. 358 et seq.

[2] See also Walker's Statistical Atlas, cited above, Plate 46.

[3] Ninth Census, vol. ii., cited above. Special tables of Mortality, p. xxiii. The number of cases is, enteric fever, 22,187; typhus fever, 1770; cerebro-spinal fever, 650. The cases reported as typhus were simply misnamed, and are to be regarded as enteric.

[4] Op. cit., 2d series, p. 358.

[5] See, for example, G. B. Wood. Treatise on the Practice of Medicine. 6th ed. Philadelphia, 1866, vol. i. p. 383.

[6] Liebermeister. Typhoid Fever, in Ziemssen's Cyclopædia of the Practice of Medicine, Amer. transl., vol. i. New York, 1874, p. 65.

varying circumstances in the fortunes of the great armies, but on the whole they represent the disease as most prevalent during the latter part of the summer and autumn, rather than during the autumn and winter. In the Pacific region the curve is strictly autumnal. The maxima are as follows: October in 1861, September in 1862, November in 1863, September in 1864, October in 1865. The minima were in April and May for 1862 and 1863, April, 1864, and March, 1865. In the Atlantic region the maximum for 1861 was in November. In 1862 there were two maxima, one during the Peninsular Campaign in July, followed by a great reduction on the withdrawal of that army, and a second maximum in October and November. In 1863 and 1864, the maxima were during July and August. In 1865, during October. The minima fell in March, 1862; June, 1863; February, 1864; and April, 1865. In the Central region the maximum for 1861 fell in November; in 1862, in May. During 1862 the curve was quite irregular; it rose to a maximum in May, then diminished in frequency during the summer, and steadily increased after September through the winter, attaining its maximum in February, 1863, after which it diminished until June, and then again increased to a second maximum in August. The maximum for 1864 fell in September; in 1865, during September and October. The minima were in March and September, 1862; June, 1863; February in 1864 and 1865.

I find from the Annual Reports of the Board of Health of the city of Boston, for 1874 and 1875, that the mortality from typhoid fever during those years pursued a markedly autumnal course, the maximum being during September in 1874, and during October in 1875.[1] In the District of Columbia the registration returns show that the greatest number of deaths since 1872 have always occurred during the months of August and September.

I incline, therefore, to the belief that typhoid fever presents in fact in this country an annual autumnal curve very similar to that of the so-called autumnal fevers. I base upon this circumstance no argument as to the relationship or nature of either disease, but press it upon your attention as a fact which must not be overlooked.

Next, let me remind you of the important fact that intermittent and remittent fevers often disappear more or less completely from neighborhoods in which they have long prevailed, and are replaced by typhoid fever. This circumstance could not escape such a faithful observer as Drake. He has described it in the following language:—

"Far in the North, remittent fever often presents, almost from the beginning, a tendency to the continued type, displaying the characteristics of the synochus of Cullen's Nosology. It is properly called autumnal fever, because it prevails most in that season, and is an equivalent for the true remittent fever of the warmer climates. Nearly the same remark is applicable to this fever when, in the middle latitudes, it appears in the long-cultivated and drier portions of Tennessee, Kentucky, Western Pennsylvania, and Ohio. Formerly it often abated into an intermittent; latterly, it is apt to degenerate into a continued type."[2]

Whatever criticism you may choose to bestow on the wording of some

[1] See Second Annual Report of the Board of Health of the City of Boston, 1874, and Third ditto, 1875. Both volumes give charts representing the weekly mortality from typhoid fever. Also, Annual Reports of the Board of Health of the District of Columbia for 1872, 1873, 1874, and 1875.

[2] Drake. Op. cit. First Series, p. 781.

parts of this remarkable passage, it is an honest attempt by a keen observer to describe a class of facts which he had had the opportunity of observing on a great scale. The substantial truth, that in numerous districts throughout this land intermittents and remittents were the prevailing form of fever when the first settlements were made; that as time passed by and cultivation progressed, the intermittents diminished in frequency, the remittents exhibited more and more a disposition to pass into continued forms, and finally were replaced by ordinary typhoid fever, which became the prevailing epidemic fever—all this, I suppose, has occurred under the personal observation of many of the American physicians who listen to me to-day, and I need not weary you by multiplying authorities in proof of a fact with which you must all be familiar.

But next let me observe that the change thus effected is not always a permanent one. Often in individual years the intermittents and remittents reappear in epidemic-wise in regions such as I have just described, and then the typhoids vanish for a time, to return once more when the temporary prevalence of the periodical fevers comes to an end.

Existing thus side by side, replacing each other in this intimate—if you will, in this intricate manner—it is evident that the unknown causes of the periodical fevers, and of typhoid fever, whatever they may be, must frequently coexist. Ought we not, then, to anticipate that individuals exposed to both would often suffer with fevers in which phenomena belonging to both affections would also coexist?

I suppose the once popular belief that diseases are entities, and that a man can suffer from but one at time, is now so completely dead that it is quite unnecessary for me to bring forward facts and arguments to disprove it. I suppose it to be now well established that individual cases of disease are always more or less complex, representing in every instance the total effect of all the morbific causes acting upon the individual, and modified always by his individual resisting power, the result of his own individual organization and his own previous history. Even Sauvages, the greatest of all the systematic nosologists of the last century, already recognized this fundamental fact when he exclaimed, in the introduction to his work: "Genera and species of diseases are abstract notions. Throughout the universe neither genera nor species exist, but only individuals."[1] Is it wonderful, then, that hybrid forms of disease, exhibiting the ordinary symptoms of malarial and of typhoid fever, variously combined, should long have been observed in this country? In fact, such hybrid forms have long been observed in Europe also. In the first volume of his Institutes, published in 1781, Burserius[2] recognized them as a group; "the Proportionata," which he defines as a compound species composed of the synochus and intermittent fever. This union, he says, occurs especially "when intermitting fevers prevail epidemically, or at least constitute the prevailing and stationary disease; for then almost all diseases bear some resemblance to intermittents, or sporadic, or intercurrent fevers, of whatever other kind, are combined with the intermitting fevers."

Hermann Schmidt,[3] in his account of the so-called Summer fever, which was epidemic throughout Europe during the year 1827, has still more

[1] Sauvages. Nosolog. Method., Amsterdam, 1768, vol. i. p. 26.

[2] J. Bapt. Burserius. Institut. Med., vol. i., Milan, 1781. I quote ftom Hecker's edition, Leipsic, 1826, vol. i. p. 512.

[3] Hermann Schmidt. Uber des Europæische Sommerfieber, Paderborn und Arnsberg, 1830.

elaborately described as the form of fever then most generally prevailing, a combination of intermittent fever with the endemic typhus of Europe (our typhoid fever). He has subdivided the resulting hybrid forms into two chief classes: 1. *Typhus intermittens subintrans*, which he defined as a combination of typhus and intermittent fever, with a predominance of the typhus element. 2. *Febris intermittens typhosa*, which he defined as a similar combination, with a predominance of intermittent fever. I would refer you to his elaborate treatise for many suggestive details.

Naumann[1] has quoted, with approval, the views of Burserius and Schmidt, and mentions corroborative observations by several other writers, to which I might add many more did the scope of this discourse permit.

Recurring to the American experiences, I would recall to your memory the fact that the existence of the hybrids under consideration did not escape the practical eye of Drake: "When remittent fever terminates fatally in one or two weeks," he observes, "a certain amount of subsultus, a dryness of the tongue, and intestinal hemorrhage, are sometimes present, although no typhus fever may be prevailing in that locality, and this brings us to inquire, not into the distinctive peculiarities of these two forms of fever, but into their combination, into the hybrid or mongrel diathesis which results from the joint impress, in ever-varying proportions, of the causes which produce true typhous and true remittent fevers."[2] For these hybrid forms he proposes the designation, "Remitto-typhous or secondary typhous fever." "I do not recollect," he says, "to have seen a case of fever well-marked as typhous in the early stages terminate as an intermittent, nor a decided intermittent degenerate into a typhous. The union is between remittent and typhous." And this certainly is the most common combination; but the combination with intermittent does also occur, though less frequently, and both the possibilities which Drake tells us he never observed were seen often enough during the civil war.

The same combination of remittent and typhoid fever which was observed by Drake has also been described by the late Professor Dickson of this city. Dickson continually emphasized the doctrine of the frequent blending of those febrile types which are "connate in cause and analogous in symptoms." He tells us that "it is a matter of familiar remark, that in long-protracted cases of the ordinary remittent of malarious regions, there is a domination of the palpable contrast or alternation of the period of febrile exacerbation and unison—a tendency in the former to continuousness, the latter being less an alleviation of the symptoms—and the several symptoms themselves approaching more and more in appearance those which belong to simple continued fever, nervous fever, or typhoid fever. In common professional language, such cases 'take on the typhoid character.'" "On examination, typhoid lesions will sometimes be found in the body dead of bilious remittents. The mucous membrane of the stomach and intestines is highly injected in severe and short attacks. In more protracted cases, follicular ulceration may be found throughout the whole extent of the bowel."[3]

[1] Op. cit. Bd. III., Abth. ii., S. 235.
[2] Drake. Op. cit., Second Series, p. 556.
[3] Samuel Henry Dickson. Elements of Medicine. Philadelphia, 1855, p. 196. See also his Essay On the Blending and Conversion of Types in Fever. Trans. of the Amer. Med. Association, vol. v. (1852), p. 127.

Similar opinions were long entertained by my revered preceptor, Dr. George B. Wood. He has told you that remittent fever "is sometimes of a low, adynamic or typhous character from the commencement. This may be the result of a previous exposure to causes calculated to depress the vital powers and to deprave the blood; but it probably most frequently arises from the co-operation of a typhoid epidemic influence with miasmata." He did not believe that the characteristic lesions of typhoid fever ever occur in pure remittents, as some have reported, and explained their observations by the remark that "there is reason to believe that enteric fever has sometimes been mistaken for bilious or remittent fever, and lesions belonging to the former been placed to the account of the latter; and not unfrequently, in all probability, the two diseases are in greater or less degree mingled together."[1]

Now, under the conditions which existed in the camps of our armies during the late civil war, these hybrid combinations, which had already attracted the attention of such men as Drake, Dickson, and Wood, in civil life, made their appearance, as might have been predicted, on a great scale, and produced, as I have already shown, a formidable mortality. It was for these hybrid forms that I proposed the term typho-malarial fever. I never meant this term to represent a specific type of fever, but intended it to designate all the many-faced brood of hybrid forms resulting from the combined influence of the causes of malarial fevers and of enteric fever. The term corresponds essentially to the "Proportionata" of Burserius. It includes both the "Typhus intermittens subintrans" and the "Febris intermittens typhosa," of Herman Schmidt, and the "Remitto-typhous" of Drake. These are merely varieties of the group of hybrids, all of which I intended to embrace. I pointed out, in my book on Camp Diseases, that this whole group might be conveniently divided, for the purpose of study, into—1. Fevers in which the malarial element, without being the only pathological condition present, is the predominant one; and, 2. Fevers in which the typhoid element is evidently predominant, although the others are also present in a more or less distinct manner. To these I felt compelled to add a third group, namely, "Fevers of either of the first two varieties, in which from the first, or at some time during the progress of the affection, the scorbutic element becomes predominant." To this general grouping of the cases, with all the light of subsequent experience, I must still substantially adhere.

In the group of cases in which the malarial phenomena predominated, the disease began as a simple intermittent or remittent fever, of quotidian, tertian, or quartan type, the most frequent form being a simple or double tertian; but after a week or ten days the fever assumed a more or less completely continued type, with many of the phenomena characteristic of typhoid fever, such as diarrhœa, abdominal tenderness, meteorism, muttering delirium, subsultus tendinum, dry, brown tongue, and the like. But even when the typhoid phenomena were most pronounced, some of the most characteristic of them were often wanting. Thus, sometimes there was no diarrhœa at all, but constipation instead. The characteristic tache rouge, or rose-colored eruption, was generally entirely absent; gastric disturbance, hepatic tenderness, and an icteroid hue of the countenance were much more generally present than in simple typhoid fever.

[1] Wood. Op. cit., vol. i., pp. 307 and 309.

Now, a large proportion of these cases terminated favorably, especially, as I think, because quinine was so freely used in their treatment; the occurrence of ordinary paroxysms of ague was a frequent accident during the convalescence. And, just because of the frequency with which they recovered, I suppose, the number of autopsies in cases of this kind which I have been able to collect is much less than in cases of the second group, of which I shall presently speak. Still, I have collected a number of autopsies of cases of this kind, in which diarrhœa had been present during the fever, and in which, after it had assumed a continuous type, it had strikingly resembled typhoid fever, but in which dissection showed no other lesion in the alimentary canal than a smart intestinal catarrh. Patches of inflammation, scattered irregularly throughout both small and large intestines, and enlargement of the closed glands, often associated with pigment deposits, were the characteristic lesions. The solitary glands of the small intestine appeared as little projecting tumors the size of pinheads, which often had constricted necks, so that they resembled tiny polypi. The agminated glands of Peyer, slightly prominent, were often the seat of pigment deposit, which gave them the so-called shaven-beard appearance. Sometimes the villi of the small intestines were hypertrophied; sometimes they had pigment deposits at their apices. In the large intestine the slightly swollen solitary glands were often the seat of pigment deposits, seated either in the glands alone, or sometimes also in the surrounding mucosa, in which case the central dot of pigment was surrounded by a little pigmentary ring. When the fever had supervened, as often happened, upon a chronic flux, or where dysentery had been developed during the course of the fever, or of the convalescence, and had been the immediate cause of death, the characteristic follicular ulcerations of the colon or the phenomena of the diphtheritic process complicated the picture. Great enlargement of the spleen and congestion of the liver, with or without fatty degeneration, were frequent concomitants. The condition of the intestinal canal in these cases closely resembled that which has been emphasized by Röderer and Wagler, and by Dickson. Between the simple inflammatory enlargement of the closed glands, which I have pictured, and the more luxuriant process which occurs in ordinary typhoid fever, and which most pathologists believe to be specific, every possible transition existed. I, for one, confess myself unable to draw a line between the two conditions. Anatomically they appear to pass into each other by insensible gradations. The essential element of both is the accumulation of a swarm of migrated white corpuscles in the closed glands, in the surrounding lymph passages, and the adjacent connective tissue, associated, doubtless, as we must infer from the study of other inflamed tissues, with multiplication of the lymph cells of the parenchyma of the closed glands by division, though it is difficult, if not quite impossible, to demonstrate this latter phenomenon in the present case. The sloughing and ulceration of the so-called typhous process is, I think, sufficiently well explained by the intensity of the process and the nutritive disturbances which thence result, without conjuring up in our imaginations an undemonstrated specific something to account for it.

The group of cases in which the typhoid phenomena predominated more closely resembled ordinary typhoid fever. They began more like it; they ran their course like it; like it, they refused to be cut short by quinine; after death they presented the characteristic lesions of the patches of Peyer. But even these cases presented, also, many phenomena

which did not belong to ordinary typhoid fever. First of all, I must emphasize the manifestation of an unwonted tendency to periodicity. This was not merely an exaggeration of the daily exacerbation and remission, which we all know as a part of the history of the early stages of typhoid fever. The exacerbations assumed with great frequency a tertian or double tertian type, which has no parallel in the ordinary typhoid history. With this tendency to periodicity, the gastric and hepatic disturbances common in remittent fever were often associated in the early stages, and ordinary ague paroxysms often occurred in the convalescence. The autopsies in these cases disclosed the ordinary lesions of typhoid fever. During my earlier studies I believed that I had observed certain peculiarities in the character of the ulcers in these cases, by which they might be distinguished from the lesions of simple typhoid.[1] A larger experience, especially the examination of a large number of specimens received by the Medical Section of the Army Medical Museum, has convinced me that this opinion was premature. I renounce it as erroneous. There is really nothing in the lesions of Peyer's glands, in these cases, to distinguish them from ordinary cases of typhoid fever; and it was just these lesions, so well known to you all that I need not pause to describe them, which were observed in the vast majority of those fatal cases of fever occurring during the late war in which autopsies have been recorded or specimens preserved. As for the other lesions observed in these cases, tumefaction of the spleen, far beyond the degree ordinarily observed in typhoid fever, was common enough, and the pigment deposits in various tissues and organs, which are so frequent in malarial diseases, were very often encountered. Moreover, the colon lesions, characteristic of chronic fluxes or of acute diphtheritic dysentery, were frequently associated, as is well shown by numerous specimens in our Museum.

I will not for a moment, however, countenance the sophism that, because the lesions of ordinary typhoid fever were those most frequently encountered during the war in fatal cases of fever, uncomplicated typhoid fever was the prevailing febrile form. On the contrary, as I understand it, though this was the lesion in the majority of fatal cases, the slighter lesions described in connection with the first group were those which most probably existed in the majority of the cases which recovered. Nor will I admit the fallacy that, even in those fatal cases in which the typhoid lesion was most marked, the patients are thereby proved to have died of simple typhoid fever. I will not emphasize the big spleens, pigmentary deposits, or other anatomical evidences of malarial complication. I will even admit the uncertain diagnostic value of all these phenomena in the present state of our knowledge. But I cannot ignore the facts of clinical observation. I cannot but see in the periodicity and other clinical evidences of malarial complication to which I have briefly alluded, proof of the action of an additional morbid agency, to which I doubt not we must look for one reason of the great mortality of the fever cases in our armies.

It often happened that, in fevers belonging to either of the classes I have just described, scorbutic phenomena complicated the picture, and sometimes even took a commanding place in determining the course and issue of the disease. I have affirmed, and shall elsewhere bring forward satisfactory evidence in proof of the assertion, that a mild but distinctly recognizable scorbutic taint was wide-spread among our

[1] Camp Diseases, p. 102.

soldiers. It manifested itself as a peculiar anæmia, accompanied by muddy complexion, large, smooth, flabby tongue, and by neuralgic or pseudo-rheumatic pains in various parts of the body, especially in the back. As a rule, it was only after this scorbutic anæmia had existed for some time, either alone or variously complicated with symptoms due to malarial poisoning, that the characteristic scorbutic conditions of the gums, the scorbutic indurations about the joints of the lower extremities, and the well-known scorbutic purpura made their appearance. These easily-recognizable symptoms of fully-developed scurvy were but moderately frequent. The preliminary anæmia, however, was common enough, though often overlooked or misunderstood.

Now, when either of the forms of typho-malarial fever, which I have described, occurred in individuals suffering under the scorbutic taint, the symptoms were modified to a degree corresponding to the intensity of the scorbutic condition. The effect of the complication was to increase the tendency to mental and bodily prostration during the disease, to tardy convalescence subsequently, and to increase the frequency of petechial and purpuric eruptions, and of hemorrhages from the nose and bowels. Sometimes the characteristic scorbutic condition of the mouth was developed during the progress of the fever, when it had not previously made its appearance. When the characteristic typhoid process was developed in individuals laboring under a marked scorbutic taint, the symptoms closely resembled those of spotted typhus. Fatal hemorrhages from the bowels were common in such cases, and on the dissection, the lower patches of Peyer were found converted into dark-red or black pultaceous sloughs of considerable size and thickness. I suppose the scorbutic condition to have modified the typhoid ulceration in such cases, just as we often see it modify the condition of superficial ulcers or of gunshot wounds.

The outlines of the chief phenomena of typho-malarial fever which I have thus endeavored to present to you to-day are necessarily incomplete, for it is, of course, impossible to go into the details of so large a subject in a paper like this. I hope to be able to fill up these outlines in a satisfactory manner in the second volume of the Medical History of the War.

And this brings me, at length, to answer the question—Is typho-malarial fever a special type of fever?—and I reply, unhesitatingly, that it is not. I, at least, am free from the blame of that error, if any one has fallen into it. In my first published account of typho-malarial fever, I expressly denied that it could be regarded as a new disease. "Much rather," I said, "should it be considered simply as a new hybrid of old and well-known pathological conditions, in which the exact train of symptoms is as variable as the degree of preponderance attained by each of the several concurring elements."[1] And this is the view which I advocate to-day. The essential point which I desire most to impress upon you is the recognition of the group of hybrids between typhoid fever and the malarial fevers. The scorbutic complication was a mere accident of the war; its existence is by no means essential to the idea of typho-malarial fever, but in dealing with the typho-malarial fever of the war I could not omit it from the picture.

It was, and still is, my belief that the mixed forms of fever which I have thus sketched constituted the great majority of the continued

[1] Op. cit., p. 111.

fevers of our army during the civil war. I still, however, adhere to the opinion which I expressed in Circular No. 15 of 1863, and in Circular No. 6 of 1865, that simple typhoid fever and simple remmittent fever did also occur, though the statistics fail to show to what extent; and I still adhere, also, to the view then expressed, that a large portion of the cases actually reported during the war "as typhoid and remittent fevers are, to a great extent, to be regarded simply as those in which the typhoid or the paroxysmal phenomena predominated."

It would follow, from the views I have advanced, that typho-malarial fever ought to be encountered also in civil life, particularly in our Southern States; not, indeed, to the same extent that it existed during the war, when hundreds of thousands of soldiers, born and bred in the Northern States, campaigned in the malarial valleys of the South, but to an extent which deserves thoughtful recognition. And this, I must believe, from my own observations, and from facts communicated by professional friends in various parts of the South, is actually the case.

Since the close of the civil war my doctrine of typho-malarial fever has been accepted with approval in many quarters, and the term has been extensively used. Dr. Meredith Clymer[1] has adopted it as a synonym of "American Camp Fever" in his edition of Aitken's Science and Practice of Medicine. Dr. George B. Wood,[2] though unwilling to adopt the name, has fully recognized the great prevalence during the civil war of "this mixture of the two fevers." In the sixth edition of his work on Practice of Medicine, published in 1866, he remarks: "Since the last edition was published this complex affection has been much more prevalent than before, probably because great numbers of young men engaged in the armies at an age when the predisposition to enteric fever is strongest, have been in an unusual degree exposed to the joint action of the causes of the two fevers; to that of enteric fever, in the almost unavoidable filth attendant upon great encampments, and to that of bilious remittent or intermittent in the low grounds from which miasmatic effluvia are so abundantly extracted in our Middle and Southern States in the latter part of summer and in autumn."

Dr. Austin Flint,[3] whose former experience in the South makes me regard his opinion in this matter as particularly valuable, in his Treatise on the Practice of Medicine, has adopted the term typho-malarial fever as a convenient designation to represent the hybrids which had been indicated by Drake and Dickson, and which Flint himself tells us he has recognized in his own lectures to medical classes for twenty-five years. His article on simple remittent and typho-malarial fever is an admirable one, to which I refer you with pleasure.

With this intelligent corroboration and support of my views on typho-malarial fever, there has been, I must admit, some indiscriminate use of the term, which is well calculated to bring it into discredit. I have myself known it to be erroneously applied to simple typhoid fevers, in the clinical history of which I at least could see nothing to indicate a malarial complication, and to simple remittents in which I could perceive no typhoid symptoms. I think I have observed, also, a tendency in certain quarters to bestow the term upon almost any obscure febrile affec-

[1] Wm. Aitken. The Science and Practice of Medicine. With additions by Meredith Clymer. Philadelphia, 1872, vol. i. p. 607.

[2] Op. cit., vol. i. p. 377.

[3] Austin Flint. A Treatise on the Principles and Practice of Medicine. Philadelphia, 1866, p. 749; also Fourth Edition. Philadelphia, 1873, p. 934.

tion which offered diagnostic difficulty. May I not hope that the publication of this paper may serve to diminish abuses of this kind hereafter?

But although widely accepted, my views with regard to typho-malarial fever have not escaped criticism. Dr. Roberts Bartholow,[1] formerly an Assistant Surgeon of the Army, and now a successful practitioner in Cincinnati, has attacked them with a good deal of acrimony in an article on the Camp Fevers of the Civil War, which he contributed to the Medical Volume of the Memoirs of the United States Sanitary Commission. Led away by the energy of his attack, he goes so far as to affirm boldly "a *typh* element did not in my experience exhibit itself as a modifying condition in remittent fever." He declares the camp fevers of the army to have been remittent, simple continued, typhoid, and typhus, and affirms that "these several forms of fever preserved as distinct clinical features in the army as the same forms of disease in civil life." To give force to his criticism, he even goes to the extent of misrepresenting my views, and says: "There were, therefore, according to Woodward, really no cases wholly typhoid, or wholly remittent, in the army"—although I had distinctly affirmed the occurrence of such cases in all my publications on the subject. I will not pause in this place to answer these criticisms of Dr. Bartholow in detail. I would merely remark that, although he makes typhus one of the forms of camp fever, he himself admits that he has no knowledge of it from personal observation. And though he is so ready to deny that the *typh* element, as he calls it, can complicate malarial fevers, he is compelled to concede that the malarial influence can complicate typhoid fever. This he has explicitly affirmed to be a common occurrence in civil life, in another article contributed to the same volume of the Memoirs of the Sanitary Commission. His language is: "I have already adverted to the fact that, as populations increase in malarious districts, typhoid supplants the intermittent and remittent fevers. During the transition period a mixed fever prevails; it is a typhoid fever with a malarial complication."[2] He admits, therefore, the combination of malarial and typhoid fevers, but holds that the typhoid element is always dominant. Against this, I maintain that sometimes the one and sometimes the other of the two elements predominates, and I must believe that the facts are on my side. I may add that the distinguished editor of the volume of Memoirs in question, Dr. Austin Flint, felt himself called upon to append to Dr. Bartholow's essay the following remark: "The general favor with which the term typho-malarial has been received, and the readiness with which it has come into vogue, show that it expresses a pathological doctrine consistent with clinical experience."[3]

In his hasty criticism, Bartholow falls into another error—so mischievous, that I cannot permit it to go unchallenged. He denies the frequency of a scorbutic taint among our soldiers during the civil war, and thinks that even the cases reported as actual scurvy include many of "ordinary stomatitis." He says: "In a pretty extended course of observation, I did not meet during the war a single well-marked case of scorbutus."[4] A general belief that Bartholow's opinions as to this matter are correct would be a serious obstacle to any attempt to improve the alimentation of our soldiers in any future war. In fact, however, this opinion is so far wide of the truth, that I can only understand it as an

[1] Roberts Bartholow. Camp Fevers, being chap. 2 of the Medical Volume of the Memoirs of the U. S. Sanitary Commission. New York, 1867, p. 193 et seq.

[2] Op. cit., p. 126. [3] Op. cit., p. 214. [4] Op. cit., p. 196.

illustration of the extent to which one may be led astray by the eagerness of controversy. It is not necessary to leave the volume in which Bartholow's paper is printed to find evidence to contradict him. You will find such evidence in the paper by Dr. Sandford B. Hunt,[1] which constitutes the third chapter of the volume; and in the first chapter of the volume, by Dr. Bartholow himself,[2] he makes a series of statements which fully justify my views. Thus, he affirms that the amount of aliment furnished to the various armies was too often "determined by the transportation available for bringing it forward, and by the so-called military necessity;" and that "the armies operating in the interior region were more affected by deficiencies in commissariat supplies than those on the Atlantic coast," of which fact he gives several striking examples. He explicitly states that "the diet was not sufficiently varied." "Antiscorbutics came to be furnished after the first year of the war, but the chief antiscorbutic—the potato—was practically unavailable, from the difficulty of transporting and preserving it."

Is it wonderful that, in this connection, Dr. Bartholow, adopting the very term he criticizes when used by me, should have felt compelled to write: "A scorbutic taint was to be observed in troops who had served in the South for a considerable period;" and, further on, that "an impoverished state of the blood, due to defective alimentation, undoubtedly existed to a large extent. It exhibited itself in various forms, but especially in increasing the fatality of diseases and in impressing an adynamic character on those of mild and tractable forms." Still further: "The camp fever of the army was essentially typhoid. It was modified by various accidental and specific causes. We have just alluded to one of the specific modifying causes—malaria. The most important of the others was scorbutus."

It would occupy too much time for me to attempt to bring forward even a small part of the evidence on which my belief of the wide-spread existence of a scorbutic taint among our troops during the late war rests. My own observations and opinions have been corroborated by those of numerous capable medical officers. As for actual pronounced scurvy, so fully developed that it could be recognized by the average regimental surgeon, 30,714 such cases, not of the scorbutic taint, but of "SCURVY," were officially reported among the white troops during the war, and 16,217 among the colored troops.[3]

The other critic to whom I desire to refer is Dr. Jerome Cochran, of

[1] Op. cit., p. 64. [2] Op. cit., pp. 16–19.

[3] The ratio of these cases to strength greatly increased during the latter part of the war, and attained formidable proportions among the troops sent to Texas during the year following the war. The following ratios are computed from the tables published in the first volume of the Medical History of the War:—

Ratio of Cases of Scurvy per 1000 *of mean strength.*

WHITE TROOPS.	
Year ending June 30, 1862	4.7
" " " " 1863	12.6
" " " " 1864	9.6
" " " " 1865	22.4
" " " " 1866	28.4
COLORED TROOPS.	
Year ending June 30, 1864	66.5
" " " " 1865	65.0
" " " " 1866	141.6

Mobile, whose objections I find in the Transactions of the Medical Association of the State of Alabama for 1875.[1] This gentleman's service as a surgeon in the Confederate Army inclines me to regard his somewhat grotesquely presented criticism with a respect I could not have felt had it proceeded from a less reputable source. Singularly enough his objections precisely contradict the objections of Bartholow; so that I might readily call upon either gentleman as a useful witness to contradict the asseverations of the other. Bartholow will see nothing in the continued fevers of the war but the typhoid element; Cochran will see nothing in the fevers around Mobile but the malarial element.

"During the last six months," he writes, "several cases of protracted fever, primarily of malarial origin, but assuming in their course certain typhoid symptoms, have been related to the Society under the name of typho-malarial fever." He objects to this term and the theory which it implies. He thinks the fevers in question are purely malarial. "The malarial mother is easily found, the fruitful mother of many children, but for the typhoid father I have looked in vain through all the streets and alleys, in all the wards and suburbs of the city;" and again: "I can only continue to say that if we have typhoid fever here, I have never seen it."

Now, the very same volume in which Dr. Cochran publishes this, contains a tabular statement of the deaths in Mobile during the year 1874. Among these I find three from intermittent fever, twelve from remittent fever, seven from congestive fever, one from typho-malarial fever, six from yellow fever, one from hemorrhagic-malarial fever, and five from typhoid fever.[2] Dr. Cochran himself tells us in the course of his remarks that his antagonists among the Mobile physicians declare the existence in that town of "a continued fever with diarrhœa, dry tongue, frequent delirium, a rose rash, running a persistent course of three weeks or more, and defying all treatment to arrest its progress;" but he will not admit that this fever is typhoid. He does not bring forward any dissections in proof of his views; indeed, he does not appear to have made a single autopsy in any of the cases in dispute. He bases his opinion entirely on *a priori* considerations deduced from his belief in the incompatibility of the idiopathic fevers. He proclaims that "there can be no doubt whatever of the truth of John Hunter's general doctrine, that no two of them can exist in the same part of the body at the same time." I can only express my belief that if Dr. Cochran had spent a part of the time which he has employed in criticizing my views, in making, with his fellow-practitioners, some autopsies on the cases of fever, which he tells us they persist in calling typho-malarial and typhoid, he would have arrived at results quite at variance with the opinions he has expressed.

And now, one word before I close, as to the question of nomenclature. Is it convenient to bestow a single term, as I have done, upon the whole brood of hybrid forms resulting from the simultaneous action of malaria and of the causes of typhoid fever; or, is it best to use several designations according as the one or the other of the two elements predominates? We might give the cases in which the typhoid element predominates the name miasmatic-typhoid fever, suggested by Levick,[3]

[1] Jerome Cochran. Note B. Typho-malarial Fever. Trans. of the Med. Association of the State of Alabama, 28th Session, 1875. Montgomery, Ala., 1875, p. 337.

[2] Op. cit., p. 314.

[3] Op. cit. supra; also a lecture by the same author, Miasmatic Typhoid Fever. Med. and Surg. Reporter, June 21, 1862, p. 283.

and call those in which the malarial element predominates typhoid-miasmatic fever. Such a double nomenclature we have already seen was suggested by Hermann Schmidt. We might go still further, and apply special names, in accordance as the malarial element showed itself, by giving to the early stages of the case an intermittent or remittent type. I do not think this is at all necessary. I think a single term which shall include all the hybrids quite sufficient; but I shall not quarrel with any one who wishes to make further subdivisions.

If we agree to represent all these hybrid forms by a single appellation, is the designation Typho-malarial the best for the purpose? Would it be an improvement to adopt the term "entero-miasmatic," suggested as a substitute by Dr. George B. Wood?[1] I confess to a preference for the term I have suggested. It is easy enough to pronounce, and seems to me to express its meaning very well; but I attach far less importance to the employment of the name which I have suggested than I do to the recognition of the compound forms of fever which I intended to represent by it. The name is, after all, a mere matter of choice. But if I have rightly presented the subject, a just appreciation of the hybrid forms which I have urged on your attention to-day is a matter of grave practical importance. Not merely as a question of Military Medicine, though most important in that connection, for I take it that whenever again hereafter an army recruited in a comparatively non-malarial region shall campaign on a malarial soil, these hybrid forms will appear once more in epidemic proportions; but meanwhile, I suppose, in sporadic or endemic-wise, we shall continue to have these cases to deal with in civil practice in all the miasmatic regions of our Middle and Southern States, and their right comprehension is, therefore, a question of serious moment to every American physician engaged in practice in such localities.

Discussion on Dr. Woodward's Paper.

After the reading of the preceding paper, Dr. Roberts Bartholow, of Cincinnati, said:—I have listened very attentively to the paper just read, and as it refers to some criticisms of mine, I feel called upon to reply. I have studied Dr. Woodward's writings, and, with many other physicians of the United States, have labored under the impression that he had attempted to erect the Typho-malarial fever into a distinct type of disease. Now he recalls, denies, and repudiates what he has formerly published. In the paper which I had the honor to contribute to the volume issued by the Sanitary Commission, and to which allusion has been made, I simply stated that there were distinct cases of simple typhoid and of remittent fever in the army of the United States. As a practitioner in the Ohio Valley, I have seen cases of fever in some of which the malarial, and in others of which the typhoid element predominated. The paper just read throws no new light on the subject. The form of disease referred to has been long since known. I must repudiate the idea that the statements in my paper were made in an acrimonious spirit. When eminent medical men publish their views, the most obscure member of the profession has a right to express a difference of opinion, and should not be stigmatized as acrimonious simply because he utters his opinions in a determined and fearless manner.

Dr. Woodward said:—I wish simply to say that Dr. Bartholow undertook to write a history of the fevers of the Civil War for the Sanitary Com-

[1] Op. cit., vol. i. p. 377.

mission. He criticized my views, as I thought, in an acrimonious spirit, and I resolved at the time to reply, but have not till the present occasion had a convenient opportunity. If after reading my paper, Dr. Bartholow still thinks that I have misrepresented his criticism, I will offer him an apology for having done so.

Dr. N. S. Davis, of Chicago, said:—I had not supposed that the term "typho-malarial" was intended to indicate any specific form of fever, and am surprised that the author of the paper seems to combat the idea that it should be so received. I supposed that it meant simply the intermingling of the causes of two distinct forms of fever, in such a way that they influenced the human system at one and the same time. This phenomenon of intermingling has been familiar to me ever since I took up my residence in Chicago, in 1849. Then it was almost universally believed that our fevers were of a malarial character; very few recognized typhoid fever as such. The locality was highly malarious. The population was about 30,000, and there was not a foot of sewerage. The supply of water was drawn from superficial wells, and the prevalence of intermittent and remittent fevers was very distinct. Ten or twelve years later, when sewerage had extended, and a supply of lake water had been supplied, the intermingling was observed. Cases of fever at their onset were regarded as malarial, but, not yielding to the influence of quinia, were ultimately recognized as of a typhoid character. As time passed on, the typhoid element predominated, and we now find no difficulty in recognizing the intermingling of types.

Dr. W. Scott, of Cleveland, Ohio, said:—I rise to bear testimony to some of the facts set forth in Dr. Woodward's paper. Physicians of thirty years' standing in the Ohio Valley, and especially in the southern portion of the State, remember the time when this complex condition was unknown. Our experience is that the typhoid element is not affected by the use of quinia. When I have recognized this mixed form of disease, I have given quinia in sufficient doses to destroy the intermittent character of the affection, when the fever persists in the continued form. In 1864 I was in Nashville, and observed many cases of scurvy among the soldiers in the Nashville hospitals.

Dr. Wm. Pepper, of Philadelphia, said:—At the time I first read Dr. Woodward's publications on the subject of typhoid and malarial fevers, they made a clear impression on my mind; and I think his views as now expressed are the same with slight modifications as to the anatomical relations of these two affections. I confess, however, to some hesitation as to the propriety of employing a special term to distinguish a disease which is not itself specific. A great deal of confusion has resulted from the use of such terms; and I have found many who regard typho-malarial fever as wanting the specific characters of typhoid fever, and as, therefore, not requiring the same care as the ordinary uncomplicated form of the disease. We meet with cases of typho-malarial fever in this city, and I have myself become very familiar with it. It is found itself, however, to present so many forms and varieties, partly depending on the relative proportion in which the typhoid and the malarial elements are combined, as to render it very difficult to give a clinical definition of it. Moreover, I have not found that the symptoms which are usually given as indicating the coexistence of the two poisons, are as constant and reliable as appears to be thought by many, and as they certainly must have been in the army cases upon which Dr. Woodward founded his classical memoir. Thus, for instance, it is true that in the malarial form of typhoid fever an initial chill and an early rise in temperature are more common than in the uncomplicated form. But there are so many irregularities in the mode of onset and course of typhoid fever, apart from any complication, as to render this of uncertain value. And in this connection I would add that careful study of the irregular and abortive forms of typhoid fever has convinced me that the fixed rules for the temperature curve in this disease adopted by many authors, must be very considerably modified. What I have said of the initial chill and the fever-curve, I might say

of the kind of tongue thought to be characteristic; of the relative frequency of eruption, diarrhœa, epistaxis, etc., in the two forms of the disease. We are not in the habit of erecting into special types of disease the cases of other affections which are complicated with malaria—as, for instance, pneumonia, dysentery, etc., and, although in the case of typhoid fever this complication is certainly more frequent than in any other affection, it appears doubtful to me whether it would not be better to include typho-malarial fever under the general head of typhoid fever, and to merely designate it as the "miasmatic form" of that disease. Still, there is a feeling among physicians that the use of the term "typho-malarial" is convenient, and it is perhaps desirable that for the present at least it should be recognized.

The Secretary read the following remarks contributed by Dr. J. A. Estlander, of Finland:—At certain periods, malarial fever is very common along the southern coast of Finland; at other periods it disappears altogether. During the last years of the decade 1850–60, it reigned over the whole coast, but from 1860–68 it disappeared so completely that not one single case was to be met with at the hospital. From the last-named year until the present time, it has again plagued this part of the country very severely, but seems now to be fast decreasing. This periodicity seems to depend on the atmospheric pressure, for when during our short summer the average range of the barometer is high, the water-level is low, and the bottom of the sea, in the numberless small creeks and bays penetrating the shores of the Gulf of Finland (where, by the way, no tide is perceptible), lies uncovered and exposed to the sun. When, on the contrary, the mean barometer is low, all is covered with water, and the summer is rainy and cold, but free from malaria. The abdominal form of typhoid fever may be called endemic in my country, for cases of that fever occur every year and at every season, although sometimes with more, sometimes with less, intensity and frequency. At such periods, when the malarial fever is very intense, we often see cases exactly like those so well described by Dr. Woodward. These cases most frequently appear in spring and autumn, and particularly in the early part of the last-named season, to wit, the month of August, for such, in this land of short summers, this month is designated. For this reason, this fever is in my country commonly called "Augusti-feber." Dr. Woodward has named this form of fever "Typho-malarial," to define it as a hybrid of the typhoid and the malarial fevers, and I gladly accept this name and its signification. It is a well-known fact that during a period when the malarial constitution is predominant, many diseases combine in a curious manner with this fever, as for instance diarrhœa and bronchial catarrh, particularly in children, and therefore such a concurrent action of separate intoxications, as the name "Typho-malarial" implies, has many analogies in pathology.

On motion of Dr. Woodward, seconded by Dr. Pepper, the following was adopted as expressing the opinion of the Section:—"Typho-malarial fever is not a specific or distinct type of disease, but the term may be conveniently applied to the compound forms of fever which result from the combined influence of the causes of the malarious fevers and of typhoid fever."

ARE DIPHTHERITIC AND PSEUDO-MEMBRANOUS CROUP IDENTICAL OR DISTINCT AFFECTIONS?

BY

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The term Pseudo-membranous Croup, Membranous Croup, or, for brevity, Croup, is applied to a sporadic inflammation of the larynx, which is accompanied by the formation of a pseudo-membrane upon the laryngeal surface. It is attended by no symptoms. except such as are referable to the local disease, and it destroys life by the mechanical effect of the exudation. Croup is universal; Steiner says, "Croup extends over the whole world, but strikingly diminishes in frequency, according to A. Hirsch, as we pass from the higher latitudes to the tropics." Therefore nearly every physician in general practice meets cases from time to time. My present purpose is to consider the relation of this malady to an inflammation of the same parts, which is accompanied by a similar exudation and with similar local symptoms, but which we know to be a form of, or a manifestation of, a contagious and epidemic disease, namely, diphtheria.

There are two theories regarding the relation to each other of croup and diphtheritic laryngitis. The *first* is that they are entirely distinct, that the one is a simple, non-specific inflammation, not capable of reproducing itself, while the other is specific and contagious; that, therefore, they sustain to each other, as one of our writers has well said, a similar relation to that which exists between ordinary metritis and the metritis of puerperal fever, having sufficient clinical differences to justify our regarding them as distinct maladies. The *second* theory is thus enunciated by one of its foremost advocates (Steiner): "The attempt to distinguish croup and diphtheria as two entirely distinct diseases has been unsuccessful, both from an anatomical and a clinical standpoint; indeed there are many good reasons for supposing that these two affections are only varieties and modifications of one and the same process, which, in consequence of special influences and collateral causes, as yet imperfectly understood, makes its appearance at one time as croup, at another as diphtheria, now in a sporadic form, now as a wide-spread epidemic, now as a primary, now again as a secondary affection."

The attempt to elucidate the nature of croup, and determine its relation to diphtheritic laryngitis, is attended by unusual difficulty for one who makes his observations in a locality where diphtheria prevails, as is the case with most of us who reside upon the Atlantic coast. The difficulty alluded to arises from the impossibility of making a clear and certain diagnosis of croup, where diphtheria is endemic. Has not every physician in such a locality, now and then, believed, that he was treating a case of croup, such as he had, perhaps, observed in former years, even adhering to the diagnosis till the death of the patient, when the subsequent history of contagiousness has taught him that, after all, the case was one of diphtheria? There is no symptom or anatomical fact observed at the

bedside which will enable the physician to say that the case is croup, and not laryngeal diphtheria. Absence of a pseudo-membrane upon the fauces does not, as some seem to think, establish the diagnosis of croup, for I have treated undoubted cases of diphtheritic laryngitis, ending fatally, in which there was not only no diphtheritic pellicle upon the surface of the pharynx, but only a mild degree of catarrhal pharyngitis. Nor is the absence of glandular swelling under the ears certain proof that the laryngitis is croupous, instead of diphtheritic, for, although it is true that considerable tumefaction is a sign of diphtheria, yet this swelling is sometimes absent in cases which are clearly diphtheritic.

For correct and reliable facts in regard to croup I have found it necessary to consult medical literature, and to depend upon my own recollection, and that of the older physicians of New York, as to cases which occurred prior to 1857, at which time diphtheria became established as an epidemic and endemic of the city, and upon the observations of intelligent physicians in localities where diphtheria does not prevail. I am under obligation to physicians who have thus aided me. The relation of croup to diphtheritic laryngitis may be conveniently considered under the following headings—

I. *Croup a local malady ; diphtheritic laryngitis the expression or manifestation of a general disease.*—The late Dr. Hillier, of London, a close observer of diseases, inclined to the belief that croup was a constitutional malady, but the following facts strongly indicate its local nature. Its causes, as we shall see, are the same or similar to those of the ordinary inflammations, which are admitted to be strictly local. There is no evidence, from the symptoms, of any systemic infection or blood disease, and recovery immediately follows relief of the local malady. The depression which attends cases of diphtheria is lacking. If croup were a general disease, there should be, it seems to me, in some cases at least, anæmia, poor appetite, weakness, etc., indicative of the blood change, for a longer or shorter time after the removal of the laryngeal obstruction and abatement of the inflammation.

As evidence, also, that croup is not a constitutional disease, is the fact that the inflammation is almost invariably—I have heard of no exceptional case—limited to a single tract of the mucous membrane. It is the uniform recollection of the older physicians of New York with whom I have conversed in reference to this matter, that they never observed a case of croup, before the advent of diphtheria, which was attended by membranous exudation upon other surfaces than that of the pharynx and air passages. Were there systemic infection, or blood poisoning, in croup, we should expect that, in some instances at least, the characteristic inflammation and exudation would occur in other localities also, as in diphtheria, especially since nearly every mucous membrane in the system is susceptible of croupous inflammation. Another fact indicative of the local nature of croup, is its tolerance of depressing remedies, as we shall see hereafter. Constitutional diseases having inflammatory lesions, badly tolerate such agents. From clinical and anatomical facts like the above, the profession has long regarded croup as a local malady, and I can see nothing in the most recent investigations regarding its nature to shake the belief in this opinion.

How is it with diphtheritic laryngitis, as this inflammation ordinarily occurs? Is it local, occurring independently of blood changes and produced by the lodgment of the diphtheritic virus upon the laryngeal sur-

face; or is it the result of the presence of the diphtheritic virus in the blood, so that it sustains the same relation to the general disease, that the pharyngitis of scarlet fever and the bronchitis of measles do to those diseases? The latter theory, namely that the laryngitis and other inflammations of diphtheria are the result and outward manifestation of an infected or poisoned system, was generally accepted by the profession of this country and Europe during the last twenty-five years, until the publication of the bacterian theory. This theory (according to which it is claimed that a microscopic organism, which is the cause or specific principle of diphtheria, has been found making its way from the surface, on which it has lodged or been received from the air, through the tissues into the circulation) has shaken the belief of many in the constitutional nature of diphtheria, and they incline to the other view, which certain experiments seem to support, that diphtheria in ordinary cases begins locally, and that blood poisoning may or may not result, but that if it does, it is secondary.

It is a matter of great importance—an importance transcending that of almost any other subject relating to the pathology of this dreadful scourge—that we should ascertain clearly and certainly which of the two prevailing theories is true, since the theory influences practice. Is the opinion correct, that the primary event is the entrance of the poison into the blood; or the other opinion, that the poison is received upon an exposed surface, where it excites inflammation, and that the blood either escapes contamination, or receives the poison by absorption or penetration from the inflamed surface?

Experiments on the lower animals have thus far failed to demonstrate the primary nature of diphtheria, as it occurs in the human race. Among the most recent experiments which have a bearing upon this subject, are those of Dr. Rajewsky, performed with the approval and at the instance of Prof. Von Recklinghausen. Rajewsky poisoned the blood of rabbits by injecting diphtheritic matter into it, and then produced catarrhal inflammation of the intestinal mucous membrane by injecting a weak solution of ammonia into the intestine, when the interesting result followed, that the catarrhal became a diphtheritic inflammation. Obviously in these experiments diphtheria was primarily constitutional. The diphtheritic inflammation was developed from within—from the poisoned state of the blood. But in another class of experiments, he first produced catarrhal inflammation of the intestines in the same manner as before, and then injected diphtheritic matter into the intestine. The result was the same as regards the production of diphtheritic inflammation upon the intestinal surface, for upon the inflamed portion, to which the poison was applied, the characteristic diphtheritic pellicle appeared. It may be inferred, that in this class of experiments the primary action was local, but may not absorption of the poison have preceded the local disease, as is now believed to be the case in syphilitic inoculations?[1] (Bumstead.)

The fact that diphtheritic inflammations ordinarily occur upon those surfaces which are most exposed to currents of air, and upon which any deleterious substance floating in the air would be most likely to lodge, has been supposed to lend support to the theory that diphtheria is at first localized in these exposed parts. But if these inflammations were ordinarily produced by the direct contact of the poison received from the air,

[1] Lond. Med. Times and Gaz., Dec. 11, 1875.

coryza ought, it seems to me, to be the primary diphtheritic inflammation in a large proportion of cases; whereas it is not ordinarily primary, and in many cases does not occur at all. The fact is that the diphtheritic virus, in whatever way its reception occurs—whether in the ordinary manner, or through a wound of the skin—appears to be especially attracted to the fauces and contiguous parts, or to find in them the seat of its greatest activity, so that even if the disease is received through a distant part of the system, pharyngitis is apt to occur. Thus Hillier relates the case of an eminent surgeon who, performing tracheotomy on a child supposed to have croup, punctured his thumb. The puncture was painful, and on the following day a pustule occupied its site. This was opened and poulticed. "A day or two later, on removing the epidermis, the subjacent cutis was found in the condition of a peculiar dark slough; there was an entire absence of suppuration, and excessive pain. This was followed in six days by a diphtheritic deposit on the tonsils. The wound on the thumb was long in healing." A month later, diphtheritic paralysis occurred.[1] In cases like the above, if they are correctly interpreted, and the poison is received through the skin, it is evident that the inflammations on the exposed mucous surfaces are not produced by the direct deposit of the virus, but that they result from the blood poisoning. And the important and interesting fact appears, that, in cases thus contracted by inoculation, diphtheria is constitutional, before the pharyngeal or laryngeal inflammation occurs.

But if we could obtain no farther light in reference to the primary nature of diphtheria than what is afforded by experiments on animals and accidental inoculations in man, we would probably never know, when called at the commencement of a case contracted in the usual manner, whether we had to treat a merely local affection, or a constitutional disease with local manifestations. We must fall back to the wider field of clinical experience for help to an understanding of this matter. The following facts, taken collectively, afford, in my opinion, strong corroboration of the theory that there is primary systemic infection in diphtheria, as it is commonly contracted:—

(1) *The long incubative stage in certain cases.*—Although the incubation of diphtheria is ordinarily from two to five days, it is sometimes an entire week, as in the following examples: In April, 1876, a little girl died of malignant diphtheria in West 41st Street, New York. Her sister, aged one year, remained with her from April 14 to 17, when she was removed to a distant part of the city, and placed in a family where there was no sickness, and had been no diphtheria. On the night of April 24, seven days after her removal, this infant was observed to be feverish, and on the following day, when I was called to examine her, the characteristic diphtheritic patch had begun to form over the left tonsil. In April, 1875, two sisters, aged seven and five years, resided with their parents, in a boarding-house, in West 22d Street, New York. A playmate in the same house had symptoms which were supposed to be due to a cold, but which were diphtheritic, when one night severe laryngitis occurred, and ended fatally the next day. The physician who had been summoned diagnosticated diphtheria, and the two sisters were immediately removed to a hotel. But seven days subsequently, diphtheria commenced in the older child. The younger was at once removed to a distant part of the same hotel, but on the sixth or

[1] Hillier on Diseases of Children.

seventh day subsequent she also became affected with a fatal form of the disease.

Now according to the doctrine that diphtheria is at first local, and that the inflammations result from the irritating effect of the virus which lodges upon the mucous membrane, this virus must remain inactive a week upon the surface where it is received. Such inactivity in a poison of unusual energy and malignity would be very improbable. But we have sufficient explanation of the long incubation, if, in whatever way the poison is received, it first enters the blood, and the inflammations occur consecutively to the blood-changes. It is a law in pathology that those specific diseases which have an incubation of several days, are constitutional.

(2) Another fact, which indicates primary blood poisoning in diphtheria, is observed in certain cases, namely, the *occurrence of severe constitutional symptoms for a longer or shorter time, perhaps for half a day, before the appearance of the usual inflammation.* Thus a girl of five years, having malignant diphtheria, whom I saw in consultation, was carefully examined on the first day of her sickness by the attending physician, and although he closely inspected the fauces, there was no appearance which indicated the nature of the malady till the subsequent day. In such cases, a sufficient number of which I have observed, there is apt to be complaint of soreness of the throat, or difficulty in swallowing, almost from the beginning of the general symptoms; but the pain and tenderness seem to be in the deeper tissues of the neck, and the fact that redness of the mucous surface does not appear till some hours subsequently, is evidence that the inflammation is developed from within, and not from the irritating effect of the poison upon the surface.

Again, treatment of the inflammations by the most reliable and efficient antiseptics and disinfectants which we possess, commenced at the earliest possible moment and repeated at short intervals, does not prevent the occurrence of indubitable symptoms of blood poisoning in cases of a severe type. Thus I have treated every portion of the inflamed surface, as far as it was accessible, every second or third hour, with carbolic acid and other disinfectants, almost from the very commencement of diphtheria, and so thoroughly that any vegetable or animal poison, with which the remedies had come in contact, would probably have been destroyed, or rendered inert, and yet, except in mild cases, symptoms of diphtheritic blood poisoning have occurred, and as early and uniformly as if less energetic local measures had been employed. While, therefore, I do not fail to recommend local treatment as calculated to prevent septic poisoning, and relieve the inflammations, I have lost confidence in it as a means of preventing the entrance of the diphtheritic poison into the blood. Its powerlessness to prevent contamination of the blood by the diphtheritic virus is an additional evidence that this contamination occurs independently of the local disease, and probably precedes it.

(3) *The quick succumbing of the system in certain malignant cases* is evidently due to diphtheritic toxæmia. We sometimes observe a fatal result on the second, third, or fourth day, without any dyspnœa, or sufficient laryngitis to compromise life. Cases of this kind, terminating fatally even in the first day, have been reported. The system is suddenly overpowered by the poison, struck down, as it were, by the profound blood change, while the inflammations are still in their incipiency.

(4) Important evidence of the constitutional nature of diphtheria is afforded also by the *state of the kidneys*. No internal organs are so often

affected in diphtheria as the kidneys, and on account of their location and anatomical relation, it is evident that the poison first passes through the system before it reaches them. Any clinical or anatomical fact, therefore, which indicates that the diphtheritic virus has reached and affected the kidneys, affords proof that it has penetrated the system, and poisoned the blood. Now the occurrence of albumen, with granular or hyaline casts, in the urine, in cases unattended by dyspnœa, affords proof of nephritis, caused by the action of the poison on the kidneys. Dr. West, from observations made in London, believes that albuminuria is commonly present in diphtheria, but the frequency of its occurrence varies in different epidemics, according to the severity of the type.

During the latter part of 1875, and in 1876, prior to Aug. 1, I endeavored to obtain and examine the urine in every case of idiopathic diphtheria, having a clear diagnosis, which came under my notice, both in family practice and in the institutions with which I have an official connection. Ordinarily, during the first week of a case, I found that the urine deposited urates on cooling, and that the nitric acid test showed a large relative quantity of urea, but I suspect that this was due to a somewhat diminished quantity of urine. But the occurrence of albumen was of chief interest, and the results of the examinations as regards the presence or absence of this, are recorded in the accompanying table. In most of the cases the urine was examined several times in the course of the disease, and, if albumen were present, a microscopic examination was also made. In nearly all the specimens which contained albumen—all but three or four—casts, usually granular, but now and then hyaline, and sometimes both kinds in the same specimens, were observed. In those cases of albuminuria which recovered, there were comparatively few casts, or none. If the albumen were abundant, and casts plentiful, the case was usually fatal, though not perhaps till after the lapse of three or four weeks, when death occurred with symptoms of exhaustion, paralysis, or feeble heart-action, sometimes with œdema of lungs supervening suddenly, and, probably, formation of heart clots. The albuminuria, unlike that of scarlet fever, seldom occurred except in the grave cases; and in the majority of instances it did not appear till near the close of the first week, or in the second, and, in a few instances, not till a later period. Although the albuminuria of diphtheria is much more grave than that of scarlet fever, it has in my practice been attended by much less serous effusion or dropsy, often by none which was appreciable. The urine, although containing a large quantity of albumen, often had nearly the normal appearance, instead of the smoky or hazy color so common in the albuminous urine of scarlet fever.

I. *Cases attended with the usual membranous exudation upon the fauces, with or without coryza, and without laryngitis or with only catarrhal laryngitis; fifty-eight cases.*

	Died.	Recovered.	Result not stated.	Total.
With albuminuria . . .	13	5	1	19
Without albuminuria . . .	4	27	1	32
State of urine not recorded .	3	4	..	7

II. *Cases attended with membranous laryngitis as the predominant inflammation; nineteen cases.*

	Died.	Recovered.		Total.
With albuminuria . . .	4	1	..	5
Without albuminuria . . .	2	4	..	6
State of urine not recorded .	7	1	..	8

The mortality of the cases embraced in the above table was probably larger than the average in New York practice, for several of them were seen in consultation, and their type was severe. Those in which the state of the urine could not be ascertained, were usually in children so young or so near death that it was impossible to obtain sufficient urine for examination. It is not improbable, also, that, occasionally, in cases in which it is stated that there was no albumen in the urine, it may have been present at a more advanced stage of the disease, or on days when the examination was not made; but, as stated above, there were usually several examinations of the urine made during the progress of each case.

Sir John Rose Cormack, of Paris, in a series of interesting and useful papers relating to diphtheria, published in the Edinburgh Medical Journal during the present year, states that albuminuria, and of course the nephritis on which it depends, sometimes begin as early as the first day. My observations confirm this statement, as in the following cases:—

Case I.—L. McD., aged three years, was first visited by me on Feb. 29, 1876. I learnt from the parents that she had been feverish during the preceding forty-eight hours, and her urine very scanty. A moment's examination was sufficient to show that the case was one of malignant diphtheria, for the fauces were already nearly covered by the diphtheritic pellicle, the temperature was 103¼°, and the pulse 140. The skin was hot and dry, and there was moderate swelling under the ears, and a muco-purulent discharge from the nostrils. On account of the scantiness of the urine, the amount not exceeding ℥iv–v daily, it was impossible to obtain sufficient for examination till the following day. It was then found to have a specific gravity of 1032, to contain a deposit of urates and hyaline and granular casts, a diminished amount of urea, and a large quantity of albumen. It can hardly be doubted from the scantiness of the urine, and the large amount of albumen found when the urine was first examined, that albuminuria had been present on the first day.

Case II.—The following was a similar case: K., aged four years, living in West 36th Street, was visited by me in consultation on Jan. 29, 1875. Her sickness had also continued forty-eight hours; her fauces were swollen, and covered with the diphtheritic pellicle, which was dark and offensive; respiration guttural; pulse 120; temp. 101°; she had a free discharge from each nostril; urine scanty, its specific gravity 1030; it contained a small amount of albumen, with casts, and a large amount of urates, with no apparent diminution of the urea. Death occurred on the fourth day.

In such severe cases, in which albumen and casts are found in the urine at the first visit of the physician, there can be little doubt that the nephritis begins nearly or quite as early as the pharyngitis, and therefore, since poisoning of the blood must antedate the renal disease, diphtheria is in these cases very early, probably from the occurrence of the first symptoms, a constitutional malady.

Again there are cases, though rare—only one I can recall to mind during the last two years in my practice—in which the external manifestations of diphtheria are very mild, even insignificant, and quickly cured, but in which the kidneys are severely affected. The occurrence of such cases is best explained on the supposition that the first departure from the state of health is in the blood, and that the blood change gives rise to the inflammation of the mucous membrane externally, and of the kidneys internally, rather than upon the supposition that the transient and insignificant inflammation of the mucous membrane is the first event in the series of morbid changes, and that this inflammation leads to poisoning of the blood, and the establishment of a much more severe

and protracted inflammation in the kidneys. The following is the case alluded to:—

Case III.—The house, 229 West 19th Street, New York, is an old wooden structure, and the family, which has occupied it during the last five years, has been three times visited by diphtheria, the first case, that of the oldest child, proving fatal. In February, 1876, one of the children had diphtheria in a moderately severe form. He recovered, and, after my visits had been discontinued, his sister, aged six years, who had had scarlet fever when eighteen months old, became feverish and complained of her throat. No rash appeared on her skin, and there was apparently no coryza. Inspection of the fauces by the parents revealed a small diphtheritic patch over each tonsil. Although diphtheria was so frightful a malady to this family from their past experience, the case seemed so mild that the parents treated it without medical attendance, by the remedies which had been employed for the boy. A mixture of carbolic acid, subsulphate of iron, and glycerine, was applied to the fauces every third hour, sufficiently often, apparently, to destroy all bacteria or other vegetable or animal organisms with which it might have come in contact, and within two or three days the inflammation of the throat seemed to the parents to be cured. Nevertheless, with this insignificant inflammation of the fauces, so quickly subdued, and with no other apparent inflammation of the mucous surfaces, there was severe internal disease going on as the result of the general infection. The child did not regain her former appetite; she had increasing pallor, although able to play about the house; and, finally, in the third week, when I was called to see her, slight œdema of the face and limbs was observed. Her urine, which was scanty, was found to contain pus and blood corpuscles, albumen, and granular casts, and nearly two months elapsed before, under treatment, it became normal, and her health was restored.

One more clinical fact which I have often observed, may be cited in support of the opinion that diphtheria is primarily constitutional, and that the inflammations are secondary, and dependent on the blood poisoning. It is this: When the inflammations of the mucous surfaces have apparently ceased, or only slight catarrhal inflammation remains, and the apartment in which the patient has been treated has been thoroughly disinfected, and local treatment discontinued, we are perhaps surprised to observe the reappearance of the diphtheritic pellicle, though less in extent and with less severe symptoms than at first. It is highly improbable that diphtheria has been contracted a second time. The rational explanation is that the reappearance of the inflammation is due to the fact that the blood is still infected; that, as when syphilitic eruptions reappear, it is not yet fully purified of its noxious elements. I have seen this return of the diphtheritic pellicle even in mild cases in which there was no other evidence of general disease. If the second occurrence of the inflammation is due to the poisoned state of the blood, is it not probable that the first inflammation is also thus produced?

I have perhaps occupied too much space in attempting to show, what indeed many already believe, that croup is simply a local malady, while diphtheritic laryngitis is one of the inflammations which result from a general malady. But within the last three or four years, the old doctrine that diphtheria is local in its commencement, and is therefore, in at least many instances, amenable to early local treatment, has been revived, and so promoted by the advocates of the bacterian system, that it already influences the treatment in many quarters. There are physicians, as I have had opportunity to observe, who attach little importance to constitutional measures, if they are called early to a case, and who rely almost entirely on local treatment for the relief of the patient.

But with a correct understanding of the nature of diphtheria, I am convinced that we must continue to regard constitutional remedies as of prime importance.

The evidence is therefore strong that, while croup is primarily and constantly local, diphtheria, as it ordinarily occurs, is a general disease from its commencement, and that the laryngitis and other inflammations result from and manifest the blood poisoning.

II. *Anatomical characters identical in kind, as regards the state of the larynx, but differing in degree or intensity.*—Most of the microscopic examinations of the pseudo-membrane, made during the last twenty years in this country and Great Britain, in cases of supposed croup, must be received with hesitation and distrust, on account of the great difficulty to which allusion has already been made, of making a clear diagnosis in localities where diphtheria prevails. For although diphtheritic laryngitis can often be diagnosticated from croup, the reverse, namely, the diagnosis of croup from diphtheritic laryngitis, is attended by such difficulty, and is so uncertain, that we can obtain, as already stated, reliable statistics of croup only from those observers who reside in localities in which diphtheria does not occur.

Fortunately, for our purpose, there are or have been localities where pathologists have been able to observe the clinical and histological characters of croup, free from the embarrassment to which I have alluded. Thirty years ago the great treatise of Carl Rokitansky, on Pathological Anatomy, appeared, a vast repository of pathological facts, derived from thirty thousand dissections. No one has ever had better opportunities than he, for studying the anatomy of diseases, and no opinion has carried, and still carries, greater weight than his, in all subjects relating to morbid anatomy which do not involve the use of the microscope. I am informed that even up to the present time, in Vienna, where Rokitansky's observations were made, diphtheria is almost unknown, and in his treatise he scarcely makes any allusion to it, so little did he know of it. We may therefore confidently believe that what this renowned observer states in relation to croup, relates to the genuine disease.

Says Rokitansky, "The croupy process occurs on all mucous membranes, and sometimes extends over a very wide tract. The mucous membrane of the respiratory organs shows an especial tendency to it, and we meet accordingly with laryngeal croup, tracheal croup, bronchial croup, and croupy pneumonia." He says, also, which I wish to emphasize, "Neither during nor after the croupy process does the mucous membrane suffer any material injury to its texture; the speedy production of mucus and epithelium prevents any further organization of the plastic exudation, ... and it never enters into an organic connection with the mucous membrane."[1] Dr. Heitzmann, now of New York, who was assistant to Rokitansky till 1874, informs me that diphtheria had not appeared in Vienna up to the time when he left that city, and that Rokitansky still held the same views in regard to the anatomical characters of croup.

Prof. Rindfleisch, of Bonn, says of the pseudo-membrane of croup: " . . . it spontaneously separates, since it is loosened by a moderate production of pus, first at the edges, and then raised from its base. . . Rarely is a secondary enlargement of the pseudo-membrane perceived,

[1] Pathological Anatomy, vol. iii., page 56, Sydenham Society's Translation.

never a reproduction at the same spot." "The boundary between the mucous and the false membrane is naturally very much obliterated by this gradual transition of the cells of both sides, although it never becomes quite indiscernible. It, however, is undoubtedly evident, that the pseudo-membrane is produced by a secretion of young elements at the surface of the irritated mucous membrane, and by their gradual induration, sclerosis, vitreous tumefaction, or by whatever other name we wish to call the degeneration." "The pseudo-membrane is of a peculiarly stratified structure, since upon a layer of cells, at tolerably equal distances, there always follows a layer of fibrin, and this sequence is repeated from one to ten times, according to the thickness of the membrane. Whether I am correct, in directly calling those second layers fibrin, must of course remain a question. I do it upon the first impression which its configuration makes, and with the reservation that therewith I always have in mind only a fluid albuminate, but indurated by the transudation into the air."[1]

Mr. Jabez Hogg, President of the Medical Microscopical Society of London, in 1873, said, as reported in the Monthly Microscopical Journal for that year: "A portion of a croupous cast, examined under a power of 350 diameters, is seen to consist of pavement and cylindrical, or columnar epithelium, and a transparent albuminous substance, entangling the scattered contents of epithelial cells, molecular matter, fat and mucous corpuscles, and a few foreign bodies, involved in a homogeneous matrix. When stained by a weak solution of ammoniacal carmine, or aniline dye, and carefully spread out with needles, the columnar epithelium retains its cilia, each cell being filled with clear protoplasmic and nucleated contents. It is therefore highly probable that these cells are not long retained, but are rather thrown off soon after their formation."

It will be observed that these authorities, from whom I have quoted (and most microscopists and pathologists agree with them), state that the pseudo-membrane of croup consists, mainly, of altered epithelial cells, held together by an albuminate which exists either in the form of layers, or a network. In the interstices, or meshes of this network, we find, in addition to the epithelial cells, granular matter, with embryonic and pus cells. The mucous membrane underneath is more or less denuded of its epithelium, is thickened and softened, and of a bright red color from active congestion of its vessels; or the redness is diminished and its color paler, if the inflammation has to a considerable extent abated.

Does croup produce any other anatomical changes, in addition to those which have been described above, and which are located in or along the air passages? Apart from those blood changes which occur in all inflammations, I believe that there are no other uniform lesions, except such as result from the passive congestion of organs which is ordinarily present in advanced cases, and which is proportionate in degree to the amount of laryngeal, or laryngo-tracheal obstruction. When the dyspnœa becomes so great that lividity of the lips and tips of fingers occurs, a correspondingly severe and dangerous congestion of the internal organs results. Hence œdema of the lungs and albuminuria may supervene in the last hours of croup. I am convinced from my own observations, and from the nature of this malady, that those who believe that they have observed other lesions of croup, as enlarged and altered spleen, enlarged

[1] Pathological Histology, translated by Kloman and Miles, §§ 364, 365, 367.

Peyerian and solitary glands, etc., have observed merely accidental lesions, or have mistaken diphtheritic laryngitis for croup.

Let us now consider the lesions of diphtheritic laryngitis. In order to determine their precise nature, I have obtained the help of skilful microscopists in New York. Dr. Satterthwaite kindly consented to make microscopic examinations of the organs in the first and second cases. The specimens were placed in a solution of bichromate of potassium immediately after their removal from the bodies. The following case occurred in my private practice:—

Case IV.—H——, aged four years, and two brothers S., who lived directly opposite in the same street in New York, were daily playmates. On January 27, 1876, H—— became feverish and complained of sore throat, and four days subsequently died of malignant diphtheria. This case was carefully examined by me in consultation, and minute records of it preserved. Before it terminated, the two brothers S. became affected with diphtheritic laryngitis. The younger brother, aged three years, was for a time in a very critical state from the dyspnœa, but recovered in about one week. The older brother, aged six years, died, having the following history: On January 29, two days after the commencement of diphtheria in his playmate, H——, he vomited and became feverish, and his voice became hoarse. These symptoms continuing, I was asked to visit him on Feb. 2. His respiration at this time was harsh, and audible in the adjoining room, and the cough croupy; pulse 96; temperature in axilla 100°; he takes considerable nutriment, and sits quietly, or walks about the room; fauces red, and slightly swollen, but without any diphtheritic exudation upon their surface; has slight glandular swelling underneath the ears; the urine contains no albumen, and the nitric acid test shows no excess of urea. The constant inhalation of the spray of lime-water is recommended, with the use of tonics. Feb. 4. Pulse 96, temperature 99°; breathes with much difficulty at times, but there is still no pseudo-membrane upon the fauces; has expectorated since the last record two thick pieces of pseudo-membrane, each about one inch in length, apparently from the larynx; specific gravity of urine 1022; it contains a deposit of urates, but no albumen; urea apparently somewhat in excess of the normal quantity. Feb. 5. Pulse 92; temperature $101\frac{3}{4}$°; has a small diphtheritic patch, not more than three lines in diameter, over the left tonsil. Feb. 6. The pellicle upon the tonsils has disappeared; the urine for the first time albuminous, thirty-six hours before death; its specific gravity 1024; temperature 103°; dyspnœa great; pulse about 120. Death occurred on Feb. 7.

Sectio Cadaveris, 19 hours after death; body spare, but not emaciated; rigor mortis present; has post-mortem extravasation of blood along the back, and a thin blood-stained fluid escapes from the mouth; two or three drachms of transparent liquid in the pericardial sac; a large yellowish-white clot fills the right ventricle, and is prolonged into the pulmonary artery; the right auricle also contains a large clot, soft and dark in its centre, but firmer and of a whiter color externally; left ventricle contains a few soft dark clots, with a little fluid blood; left auricle partly filled with blood of a tarry appearance; tonsils not enlarged, but soft, and a yellowish diffluent secretion lies in the depressions on their surface; subcutaneous glands of the neck slightly enlarged, one being somewhat larger than a filbert; under surface of epiglottis, and entire surface of larynx, covered by a firmly adherent pseudo-membrane which entirely conceals from view the vocal cords and the sinuses of Morgagni; the pseudo-membrane is continued over the surface of the trachea, being less adherent than in the larynx, and, near the bifurcation, it floats freely; it does not extend into the bronchus or bronchial tubes of the left lung, and this lung is normal. In the right lung the pseudo-membrane extends as far as the bronchial tubes of the third order; the upper lobe of the right lung is in the second stage of pneumonia, its cut surface being rough and granular, and

liquid escaping from it on pressure; the right, middle, and lower lobes are congested, and in the lower lobe is a single hepatized nodule; those portions of the bronchial tubes which are not covered by the false membrane exhibit the appearance of catarrhal bronchitis. The liver is large, and not fatty; spleen small, moderately firm, and contracted (this is noteworthy, as the spleen has been found large and soft in diphtheria); kidneys congested and swollen, and a stellate appearance of the vessels under their capsules; surface of both small and large intestines congested.

Microscopic Examination.—Red corpuscles of the blood well-preserved, some of them round, others crenated, and all granular; large masses of transparent material, containing red corpuscles, floated in the blood. The rod and chain forms of bacteria were observed in the blood, but not in greater number than are often seen in other blood the same number of hours after death. (A few grains of chloral had been added to this specimen of blood immediately after its removal.) Substance of heart apparently normal, showing no fatty degeneration, nor infiltration; no bacteria can be recognized in the substance of the heart. *Kidneys:* Right kidney examined; Malpighian bodies congested, and extravasations of blood throughout this organ; tubal epithelium granular; increase of connective tissue in points near periphery of kidney, showing interstitial nephritis, but no increase observed in this tissue in other parts of the organ; no bacteria that could be certainly recognized as such in the kidney. *Spleen:* Multitudes of granules in scrapings from the cut surface of this organ, many of them so small as to be with difficulty recognized with a magnifying power of over 600 diameters; some of them gave the appearance of the usual forms of bacteria.

Larynx: Thickness of false membrane which covered the entire surface of this organ varied from $\frac{1}{100}$ to $\frac{1}{25}$ of an inch; thickness of mucous membrane about $\frac{1}{50}$ of an inch; epithelial border of mucous membrane could be traced inwards $\frac{1}{200}$ to $\frac{1}{100}$ of an inch, where it became indistinct, merging into the other tissues which were more or less infiltrated with embryonic cells and blood. The false membrane consisted of a network of a homogeneous material, most of the meshes being empty, but those nearest the epithelial layer containing more or fewer epithelial cells. The boundary line between the false membrane and the mucous surface could not be distinguished by the microscope in many of the sections, the network of the pseudo-membrane extending into the mucous membrane. But in other places the line of separation could be distinguished, and here and there the pseudo-membrane and mucous surface were separated by collections of embryonic cells. The lymph follicles and racemose glands were apparently normal; mucous surface infiltrated with granular matter and red blood corpuscles; cylindrical epithelial cells, some of them with cilia, were distinctly visible both along the free border, and in the under surface of the pseudo-membrane. *Trachea:* The false membrane measures from about $\frac{1}{100}$ to $\frac{1}{30}$ of an inch in thickness; the mucous membrane $\frac{1}{50}$ of an inch, and its epithelial layer $\frac{1}{120}$ of an inch; the epithelial cells are much more distinctly visible than in the larynx, and the line of separation of the adventitious layer and the mucous surface is everywhere distinctly seen under the microscope; the false membrane has the same general appearance as in the larynx, but the mucous membrane is in a better preserved state than that of the larynx; it is nevertheless infiltrated with granular matter, plastic matter, and red blood corpuscles; lymph follicles and racemose glands apparently normal; in the trachea, as in the larynx, a large number of embryonic or lymphoid cells—most of them no doubt becoming pus cells—lay between the false membrane and the mucous surface.

Case V.—A second case, having the following history, occurred in the Catholic Foundling Asylum in New York. George, aged two years and seven months, was under treatment for a second attack of measles, the eruption appearing on March 23, 1876. On March 24, the pulse was 136 and temperature $104\frac{1}{2}°$. The fauces presented a deep-red appearance, indicating severe

pharyngitis, but without any membranous exudation. March 25. Pulse 140; temperature $103\frac{1}{4}°$; the rubeolar eruption is very thick over the entire surface. The Sister who has charge of the ward, noticing unusual offensiveness of the breath, has inspected the fauces and found on them the diphtheritic pellicle. March 26. Cough becoming croupy, and voice hoarse; pulse 152, temperature $105\frac{1}{4}°$. From this date the dyspnœa progressively increased, and death occurred on March 30.

Sectio Cadaveris.—A considerable part of the interior of the larynx is coated with the diphtheritic pseudo-membrane, which is firmly attached to the mucous surface; it extends without interruption over the larynx, and perhaps over one-third to one-half of the tracheal surface. It is not attached to this surface, but hangs over it like a curtain, suspended from its attachment in the larynx. Further down in the air passages there is the usual catarrhal inflammation of the mucous surface.

Microscopic Examination.—Larynx: The false membrane is found to consist of a network, apparently fibrinous; in places, in the larynx, it is raised from the mucous membrane by an accumulation of embryonic or lymphoid cells underneath; in other places it is adherent to the mucous membrane, but with a line of attachment which can be distinctly made out with the microscope; while in other places still the network extends down into the mucous membrane, and no distinct line of separation can be seen. In the upper, or exposed portion of the false membrane, no embryonic or lymphoid corpuscles are observed, but they are abundant in the deeper portion, and they infiltrate the whole mucous membrane extensively; upon the mucous surface, wherever the pseudo-membrane is detached, these corpuscles are abundant; in parts of the false membrane they fill so completely the interstices of the network that epithelial cells can scarcely be distinguished within them; in places, in the sections examined, the epithelium seemed to be wholly replaced by granular matter; in general, the border line between the diphtheritic membrane and the mucous surface is marked by a somewhat denser exudation of the albuminate—a fibrinous appearing material—than is seen in the false membrane generally; the bloodvessels in the mucous membrane of the larynx are numerous, and distended with blood. *Trachea:* The epithelium, consisting of from two to three layers, is seen to be intact wherever it is observed; the surface of the epithelium is covered with minute markings, probably the cilia in contraction; the pseudo-membrane is not seen to be reticulated as in the larynx, perhaps from the contractions which had occurred in it; it appeared granular and fibrous, and contained but few corpuscles. *Lungs:* A portion of one lung was found hepatized, and the alveoli of this portion contained pus cells, epithelial cells, blood, and a fibrinous appearing material (croupous pneumonia). *Kidneys:* The changes observed in these organs were those of tubal nephritis; the tubes were highly granular, both in the pyramids and cortex; no increase in the interstitial connective tissue was noticed; in places the tubes were not granular. The muscular tissue of the *heart* seemed normal.

In the following case, in which the larynx was not involved, Dr. Carl Heitzmann, late of Vienna, made a microscopic examination of the kidneys, in order to determine more exactly the histological changes which these organs undergo in diphtheria, and how these changes contrast with the lesions which may result from simple passive congestion, such as is present in the kidneys in fatal cases of croup.

Case VI.—J——, aged four years, an inmate of the Catholic Foundling Asylum, began to have sore-throat on March 4, 1876. The fauces were red and somewhat swollen, but without any membranous exudation, and the diphtheritic nature of the disease was not at first suspected. My attention was first called to this case on March 11, on account of almost total suppression of urine. The fauces were still injected, and somewhat swollen from catarrhal inflammation; there was a copious muco-purulent discharge from the nostrils;

pulse 148. March 13. Pulse 144; temperature $101\frac{1}{4}°$; urine still nearly suppressed, though one drachm of infusion of digitalis is administered every fourth hour, and bromide of potassium, four grains, every second or third hour, for the restlessness. Dr. Reid, in using the catheter, observed a diphtheritic patch on the vulva; there is moderate tumefaction under the ears; the patient vomits often during the last days; she has livid spots, from extravasation, under the skin; and vision is much impaired, if not lost; it is impossible to obtain any urine for examination. Death occurred without convulsions on March 15.

Microscopic Examination of the Kidneys. (By Dr. Heitzmann.)—The tubuli contorti of the first and second order of the cortical substance of the kidney almost all enlarged; their epithelium swollen in many places to such a degree that no calibre of the tubules can be seen; the epithelium richly provided with coarse granules, the enlarged living matter; the original cement substance missing; instead of this, new transparent lines formed within the protoplasm, indicating the earliest stage of catarrhal inflammation, with partition and new formation of epithelial elements; the same changes, though in a less marked degree, observable in the epithelium of the straight ducts of the pyramidal substance, while the flat epithelial bodies of the narrow ducts appear almost unchanged. The connective tissue between the ducts and the enlarged glomeruli is somewhat increased in size, and it contains newly-formed nuclei in moderate number, with enlarged bloodvessels, some of which are much distended with blood-corpuscles; no fatty degeneration in kidneys. In a few places, accumulations of dark granules occur within the ducts and their epithelium. These granules, not being united with each other by threads, nor staining with carmine, are considered to be micrococci, such as occur in any decomposing animal tissue. Whether they were present during the life of the patient, or were due to early cadaveric putrefaction (which is common after death from diphtheria), is uncertain. But since I have seen micrococci and bacteria in the fresh urine of children suffering from diphtheria, I would not deny the possibility of the occurrence of micrococci in the uriniferous tubules during life; nay, even, they may produce the inflammatory process in a way still unknown to us. In the case under consideration no trace of casts was found within the tubules, so that the inflammatory process doubtless was not a croupous one, but a relatively slight process, termed catarrhal or interstitial nephritis.

The above microscopic examinations in cases of diphtheritic laryngitis and nephritis, correspond with those made by other observers. The following facts in reference to the histological characters of croup and diphtheritic laryngitis, we wish especially to call attention to:—

(1) The croupous and diphtheritic pseudo-membranes consist alike of altered or degenerated epithelial cells, mingled with embryonic, or growing pus cells and matured pus cells, all held together by an albuminate, probably fibrin, which exuding in a liquid state from the mucous membrane underneath, surrounds the cellular masses like mortar around the stones in a wall, and hardens on exposure to the air. The consistence and toughness, therefore, of the adventitious layer, are largely due to the albuminate. The exact chemical character of the albuminate, and the chemical changes which the epithelial cells, of which the pseudo-membrane is composed, have undergone, have not been clearly ascertained, but so far neither the microscope nor chemical tests show any qualitative differences which hold good as a means of distinguishing the false membrane of croup from that of diphtheria. Supposed differences, observed by some, have not been noticed by others with other specimens. The tests of organic chemistry are not sufficiently delicate for the detection of any uniform difference in the nature of the two exudations, if any difference exist, which seems improbable.

(2) The laryngitis of croup, as compared with that of diphtheria, is

more superficial. Most inflammations of mucous surfaces, which are due to sudden changes of temperature or exposure to currents of air, as simple coryza or bronchitis, are more superficial than specific inflammations, or inflammations due to poisonous principles, acting locally, or through the blood. Croupous laryngitis is no exception. It produces less infiltration in the mucous membrane and submucous tissues, and consequently less thickening, and less exudation of the albuminate from the deeper vessels, than occurs in diphtheritic laryngitis.

(3) Croup is a transient inflammation. It commonly begins to abate when the exudation which relieves the congested vessels has occurred. Hence the statement of Rindfleisch, that the pseudo-membrane when detached by suppuration underneath, is never reproduced. The cause of croup acts only for a brief period, and therefore the effect soon begins to abate, if there is no fresh exposure. This transitory nature of croupous inflammation contrasts with the greater persistence and depth of the laryngitis of diphtheria. It is probably not absolutely true that the pseudo-membrane of croup is never reproduced. Nevertheless, the distinction holds good, that there is much less tendency to its reproduction, and less tenacity of the inflammation in croup than in diphtheritic laryngitis. There seems to be a marked similarity, not only in the nature and relations of the false membrane, but in the progress of the inflammation, in croupous phlegmasia, as it occurs in the different divisions of the respiratory tract, that occurring in the minute bronchial tubes and in the alveoli of the lungs, in the common disease croupous pneumonia, being best understood on account of its frequency. As in this disease the exudative stage soon ceases, and resolution commences with the decline of the inflammation, so in laryngeal or laryngo-tracheal croup the greatest danger is from the first exudation, and if the first pseudo-membrane can be removed by treatment there is a fair prospect of a favorable termination. In this fact lies one of the chief points of contrast in the anatomy of croup and diphtheritic laryngitis, for from the persistence of the diphtheritic inflammation, and longer continuance of the exudative stage, the diphtheritic pseudo-membrane, if expelled, will probably reform, as in one of the cases related above, and reform more than once.

(4) The pseudo-membrane of croup is more or less closely adherent to the mucous surface underneath, but most microscopists who have examined specimens removed from the bodies of those who have died of undoubted croup, agree that it does not penetrate it. The superficial character of the inflammation, and the fact that the exudation occurs from the superficial vessels, afford explanation of this fact. In diphtheritic tracheitis, we have found, in the specimens examined above, that the pseudo-membrane also lay upon the mucous surface without penetrating it; and in one of the cases, in which pneumonia had occurred, the fibrinous exudation also lay free within the alveoli of the lungs. But in the larynx, the albuminate, which formed the network of the pseudo-membrane, penetrated the mucous membrane in places so that the line of demarcation was extinguished; while in other places a distinct line of separation of the exudation and mucous membrane was observed under the microscope, though the two surfaces were in close contact; while in other places still, the exudation was elevated above the mucous membrane by the secretion of embryonic or pus cells underneath This intimate relation of the false membrane to the mucous surface w long since pointed out by Virchow, and has been confirmed by m microscopists since. But it is to be remembered that exceptional es

have been reported, in which the pseudo-membrane lay entirely upon the mucous surface without penetrating it. This admits of explanation without impairing the accuracy and truthfulness of the general statement in reference to the intimate relation of the diphtheritic pellicle with the surface of the larynx. For the adventitious layer is detached by cell formation or suppuration underneath, so that in most specimens, as in those described above, there are places in which the detachment has already occurred. If then the stage has arrived in which there is more abundant and general suppuration, the detachment of the false membrane will be in a corresponding degree more extensive. To this intimate relation of the exudation with the mucous membrane in diphtheria, is due the fact that points and patches of ulceration are so commonly observed in this membrane when the diphtheritic layer is detached. Perhaps cases occur, which from their commencement are exceptions to the above statement in regard to the relation of the pseudo-membranes to the laryngeal surface in croup and diphtheria. Perhaps there are cases of mild diphtheritic laryngitis in which the exudation lies entirely upon the surface, as it lay on the tracheal surface in the cases narrated above, but there can, I think, be little doubt that in a series of cases of the laryngitis of croup and diphtheria, the anatomical difference, as stated above, would commonly be observed in the two diseases.

(5) While the lesions of croup, apart from those of the respiratory apparatus and pharynx, result from the passive congestion which corresponds in degree with the amount of dyspnœa, there are or may be in diphtheritic laryngitis additional lesions which result from the blood poisoning, as hemorrhages upon the free surface or in the tissues of organs, sometimes fatty degeneration of the cardiac muscular fibres, and frequently desquamative or interstitial nephritis. The last case related above shows that the renal affection may be an interstitial nephritis, with almost total suppression of urine, occurring without desquamative nephritis, and therefore without the formation of casts, while in other cases the inflammation is largely desquamative, as shown by the presence of a large number of casts. This frequent occurrence of severe nephritis, interstitial and desquamative, contrasts strongly with the milder form of renal affection which occurs in croup. In croup there is passive congestion, with perhaps albuminuria, if the dyspnœa be severe, but I can find no evidence that there is any liability to those dangerous forms of nephritis with suppression or scantiness of urine, which are so common in diphtheritic laryngitis as well as in diphtheria when manifested by other forms of external inflammation. The following case may, I think, be appropriately related in this connection, as showing the blood changes and renal disease to which I have alluded, as common in diphtheritic laryngitis, but as absent or very exceptional in croup. It is true that the case was one of diphtheritic catarrhal laryngitis, but there is obviously no essential difference between dipththeritic membranous and catarrhal inflammations, and, wherever the former occurs, we commonly find at the same time a border or zone of the latter coexisting with it. This case is also interesting to the practitioner, as showing the inefficiency of the antiseptic treatment to prevent the blood changes of diphtheria:—

Case VII.—M., aged four years, inmate of the Catholic Foundling Asylum, New York, began to be sick May 6, 1876; was languid and feverish, temperature 104°, had redness of fauces and an exudation over each tonsil, no coryza; vening, temperature 103°. May 7. Pulse 120; temperature 100°. May 8. lse and temperature as yesterday; urine scanty; no albuminuria, and no dis-

charge from nostrils; the membrane extends from the sides of the throat to the roof the mouth; sp. gr. of urine 1021, urine contains no albumen, no excess of urea, and no deposit of urates. May 10. Pulse 140; has considerable œdema of fauces, and breathing guttural in sleep; vomited once since yesterday; the urine contains for the first time a moderate amount of albumen, with hyaline casts; sp. gr. 1018, acid; no urea deposited on adding nitric acid; that alarming symptom in diphtheria, epistaxis, has occurred to-day. The records which were written daily till death, which occurred on the 14th, show a gradual increase of albumen with hyaline casts in the urine, increasing scantiness of urine, so that on the 13th not more than half an ounce was passed in twelve hours; temperature not rising above $100\frac{1}{4}°$, nor pulse above 108; poor appetite, occasional vomiting, and epistaxis. Death occurred from feebleness and blood-poisoning, notwithstanding that, from the first day, three grains of salicylic acid were given the first hour, two grains of quinine the second hour, and tincture of iron and chlorate of potassa the third hour, these doses having been continued night and day in alternation; with the application of carbolic acid and subsulphate of iron to the fauces, three times daily; with nutritious diet, and the moderate use of stimulants. There were no symptoms referable to the larynx, unless a slight cough.

Sectio Cadaveris.—Mucous membrane of larynx, trachea, and bronchial tubes intensely and uniformly injected, but without any membranous exudation; lungs fully inflated, as if from commencing vesicular emphysema, and pale in front; numerous extravasations of blood in the substance of the lungs and other organs; the hemorrhages in and under the mucous membrane of stomach so abundant that the gastric surface presented a mottled appearance like the skin in measles.

Microscopic Examination. (By Dr. SATTERTHWAITE.)—The mucous membrane of the larynx and trachea was hyperæmic, but was otherwise apparently normal; muscular tissue of heart normal; spleen soft, but not appreciably enlarged. The scrapings of the cut surface of this organ contained red blood-corpuscles; bodies from two to five times the size of the blood-corpuscles, holding in their interior oil drops and fine granules, and having a yellowish-red color; granular lymphoid corpuscles, and granular débris. The walls of the stomach were congested, but without any noticeable exudation upon the surface; the extravasations of blood, described above, were found to be chiefly in the submucous tissue. In some places the gastric tubes were bare, but in other places covered with amorphous matter; but whether the covering substance was altered epithelium or diphtheritic exudation was not determined. The epithelium covering the more exposed portions of the tubes was in many places not distinct, while that covering the deeper portions of the tubes was clearly defined; at the pylorus, upon the valve, the mucous membrane was deficient; those portions of the true peptic glands lying below the tubes were normal. The mucous membrane in the lower part of the ileum was congested. Peyer's patches, and the solitary glands, both in the ileum and large intestines, were prominent, and surrounded by halos or rings of inflammation. Both the cortical and pyramidal tubes of the kidneys contained granular epithelium.

In croup the blood certainly undergoes anatomical changes as in other inflammations, and becomes overcharged with carbonic acid when the respiration is embarrassed; but I am not able to find records of any case of undoubted croup, or of croup occurring in a locality where diphtheria does not prevail, in which such profound destructive changes occurred in the blood, as were present in the above case of diphtheritic catarrhal laryngitis, and as are common in diphtheria whether accompanied by laryngeal or other form of inflammation. I must, therefore, conclude that croup and diphtheritic membranous laryngitis possess such local and general anatomical differences as justify our regarding them as essentially distinct.

III. *Clinical Facts which indicate the Duality of these two Diseases.*—The season of the year and the weather influence the occurrence of croup. Exposure to currents of air and sudden changes of temperature are its common causes. In tropical climates, croup is less frequent than in the moist and cold climates of higher latitudes. In localities swept by cold winds, or liable to sudden variations in the atmospheric heat, croup, as well as pseudo-croup and bronchitis, are common, for these diseases have a common cause. The following example, which I recall to mind, may be cited as showing how croup is commonly produced. A little girl, at a time, and in a locality in which no diphtheria was occurring, had an ordinary attack of measles. As she was beginning to convalesce, though still having some cough, she sat for some time in an open window through which the wind was blowing. On the following night membranous croup commenced, ending fatally in about two days.

The cause of diphtheritic laryngitis is different. It is a contagious principle which poisons the blood, and produces certain inflammations quite independently of atmospheric conditions. Exposure to winds and sudden thermal changes play a very subordinate part in causing diphtheritic laryngitis. They may sometimes act as predisposing causes, by irritating or inflaming the mucous membrane, for clinical experience and experiments on animals show that in a system affected by diphtheria, the specific inflammation is most apt to appear on such surfaces as are irritated or inflamed. Only in this respect can diphtheritic laryngitis be said to have the same cause as croup. In cases which I have observed, I have rarely had occasion to suspect that the patient had taken cold. I have frequently seen the laryngitis occur when there had been every precaution to prevent taking cold, and the proportion of cases of diphtheritic laryngitis has been, I think, nearly as large in warm and equable portions of the year, as in seasons of cold and changeable weather.

Again, croup very seldom occurs under the age of eighteen months, nor over the age of fifteen or sixteen years. Physicians in New York, whose practice extended over a number of years before diphtheria commenced, tell me that they never saw croup after the fifteenth or sixteenth year. Now diphtheria and diphtheritic laryngitis are not rare between the ages of six months and two years. I have seen minute white points over the tonsils, in an infant of six weeks, whose brother was dying of diphtheritic laryngitis, and there are a few cases on record of new-born infants affected with diphtheria,[1] though it is the universal experience that very young infants are less liable to diphtheria than those who are older.

Every physician, again, who has witnessed a severe epidemic of diphtheria, or who has practised where diphtheria is endemic, knows how frequently it occurs in youth, and in the first years of manhood. The statistics of New York epidemics furnish a considerable number of cases which were fatal from diphtheritic laryingitis between the ages of twenty and forty years. There is, therefore, a marked difference as regards the limits of the age in which diphtheritic laryngitis and croup occur.

But the strongest argument in favor of the duality of croup and diphtheritic laryngitis, and one which, to my mind, is conclusive, is derived from the fact that the one is non-contagious and the other highly contagious. Physicians of New York, who saw cases of croup prior to

[1] See Bost. Med. and Surg. Journ., March 11, 1875; also Contributions to the Pathology and Therapeutics of Diphtheria, by A. Jacobi, M.D., New York, page 26.

the time when diphtheria occurred, and country physicians, in localities where diphtheria has not yet prevailed, have informed me, without an exception, that they never saw any evidences of the contagiousness of croup. Would not intelligent physicians, whose united practice in localities not yet visited by diphtheria gives more than a century of observation, be able to record a single instance of the contagiousness of croup, if it possessed in any degree a contagious principle? Contrast this fact with the fearful contagiousness of diphtheritic laryngitis. Now I hold that if two maladies are identical in every other respect, but one is contagious and the other non-contagious, it will be necessary to give them different places in nosology. Contagiousness, although it be a new property assumed by an old and well-known malady, so transforms its nature that it becomes to all intents and purposes new and distinct.

The following is also an interesting fact, as showing an important difference between croup and diphtheritic laryngitis. In New York City, statistics of the mortality have been preserved since 1804, and in the long period from 1804 to 1850, inclusive, no case of diphtheria, or none recognized as such, occurred within the city limits. Now, during this time, croup was not infrequent, the yearly number of deaths from it never falling below 60, which was the number in 1817; nor exceeding 356, which was the number in 1850; the average yearly number being 148.2, and the total number for the forty-seven years being 6964. Physicians of New York, whose practice extended over the latter part of this period, state that they occasionally, at rare intervals, saw cases during this time which resembled diphtheritic pharyngitis, but which went by the name of malignant angina, or malignant sore throat. One of the prominent surgeons of New York lost a child from this sore throat about the year 1833.

Rokitansky tells us that all the mucous surfaces are liable to croupous inflammation; and were these pseudo-membranous pharyngeal, or pseudo-membranous laryngeal, inflammations of the first half of the present century, croupous; or were they the inflammations of diphtheria, which still lingered in the city in a modified form since the great epidemic of the eighteenth century, which Colden and Bard described? Certain facts may be stated which indicate the croupous, and not diphtheritic, nature of these inflammations: first, the absence of all history of contagiousness; and, secondly, the fact that the laryngitis was so much more frequent and fatal than the pharyngitis. The statistics of diphtheria, the world over, show, as every one knows, an excess of cases of diphtheritic pharyngitis over the cases of diphtheritic laryngitis; while the number of fatal cases of diphtheritic pharyngitis and diphtheritic laryngitis is either about equal, or as two to one. From the year 1804 to 1850, there were in New York 6964 deaths from membranous croup, while the deaths from all other forms of inflammation of the throat, including the membranous, for they were all grouped together, numbered only 1387, or about one-fifth of the number of deaths from croup. Now does not the wide difference between the frequency and mortality of these inflammations during the half century, when there was no diphtheria to modify diseases, and of diphtheritic pharyngitis and laryngitis of the present time, indicate a difference in the nature of the inflammations in the two periods?

Let us state the argument in different terms. If the croup of New York, in the first half of the present century, were a modified diphtheritic laryngitis, can any one tell us why membranous pharyngitis, during

the same period, was comparatively rare, and fatal membranous pharyngitis so very rare that the majority of physicians did not meet with a case in years of practice ; when in true diphtheria, the world over, membranous pharyngitis is the most frequent diphtheritic inflammation in fatal cases, as well as in those which have a favorable issue?

Writers have also very properly called attention to the fact, as showing an essential difference between croup and diphtheritic laryngitis, that the former is seldom followed by paralysis, whereas paralysis is common in those who recover from diphtheria, whatever the form of inflammation from which they have suffered.

The results of treatment also indicate a difference in the nature of croup and diphtheritic laryngitis. In croup, emetics, even those of a depressing nature, are tolerated. There is rarely any serious depression, except in advanced cases of croup, from the occasional use, in emetic doses, of ipecacuanha or tartrate of antimony and potassium, so as to expel muco-pus from the bronchial tubes, or any portion of the pseudomembrane which may be detached. In diphtheritic laryngitis, the employment of these agents, under any circumstances, is hazardous, and the profession generally, observing their ill effects, have abandoned the use of all emetics in diphtheria, even those which are least depressing. I recollect that, in New York City, soon after diphtheria began to be prevalent, physicians were sometimes astonished at the rapid sinking of patients with diphtheritic laryngitis, whom they had treated as they had previously treated cases of croup. Thus a girl, aged about ten years, with moderate dyspnœa, but whose case did not seem to be immediately dangerous, took an emetic of "hive syrup," by the physician's advice, and sank so rapidly that she died in an hour or two later. More recently I saw a child of three years, whose dyspnœa did not seem to be extreme, suddenly sink into fatal prostration, under doses of tartrate of antimony and potassium which the physician in attendance had ordered.

While in croup death results from the obstructed respiration, and consequent imperfect oxygenation of the blood, there is the same source of danger in diphtheritic laryngitis, but also the additional danger which results from blood poisoning and structural changes in important organs, as the heart and kidneys. Hence, fatal as is croup, diphtheritic laryngitis involves greater danger, and is less amenable to treatment. Moreover, as we have seen above, in ordinary cases, there are probably greater infiltration of the mucous membrane and longer continuance of the exudative process in diphtheritic laryngitis than in croup, and consequently greater liability to reproduction of the pseudo-membrane. It will be recollected that in one of the cases related above, the patient expectorated two pieces of pseudo-membrane, of considerable size, three days before death, but that the denuded places had been so completely refilled that the surface from which the detachment had occurred was not apparent at the autopsy. If the views now expressed in reference to the relative duration of the exudative period in croup and diphtheritic laryngitis be correct, and I see no reason to doubt them, it is evident that the expulsion of any considerable portion of pseudo-membrane affords much stronger hope of a favorable termination in a case of croup, than in one of diphtheritic laryngitis.

Statistics of tracheotomy for membranous laryngitis which was supposed to be chiefly croup, derived from the surgical practice in New York City some years since, before diphtheria became fully established

as an endemic, gave a mortality of about 77 per cent.,[1] whereas the result of this operation at the present time, in cases which are believed to be chiefly diphtheritic, is much less favorable. A prominent surgeon of New York informs me that in about twenty consecutive cases of tracheotomy for diphtheritic laryngitis, he has saved only one; and another surgeon has saved only one in thirteen cases; while another still, who has probably performed tracheotomy oftener than any other practitioner in New York, has saved only two in about forty operations during the last four years, and has reason to doubt the genuineness of the disease in one of the two which recovered. My data are insufficient for positive statement, but I incline to the belief, from cases which have been reported to me and from conversations with the surgeons, that it is necessary, in cases which do well, to retain the canula longer on an average after the operation for diphtheritic laryngitis, than after that for croup. We would expect this, if there is greater infiltration, and a more protracted exudative stage, in the former than in the latter disease; and although I have known the canula to be safely removed, in one case of diphtheritic laryngitis, at the close of the third day, I think it is oftener necessary to retain it the greater part of a month, or even longer. Wishing to be understood as not committing myself to this last statement in regard to the length of time during which it is necessary to maintain the artificial opening in tracheotomy for croup and diphtheritic laryngitis, for my records are too few for a positive assertion, nevertheless the results of medical and surgical treatment indicate, in my opinion, a wide difference in the two forms of inflammation.

Those who believe in the essential oneness of croup and diphtheritic laryngitis, believe, as far as I can learn, that the croup of former times has been greatly modified to become the diphtheritic laryngitis of the present day. But how far can a malady undergo modifications and yet preserve its identity? Invest a local disease with a specific principle, or call into activity a specific principle which has hitherto lain dormant in it, rendering it a constitutional and contagious disease, and its nature is so transformed that it becomes to all intents and purposes a new malady. It is not dressing up an old disease in a new garb, but it is changing its very essence or nature into something new and distinct.

In this connection an interesting fact is suggested. Those who practise in localities where diphtheria is endemic, often observe other forms of inflammation becoming diphtheritic. The pharyngitis of scarlet fever, when the primary disease has continued a few days, is often complicated by, or transformed into, a diphtheritic inflammation; and I have seen diphtheria thus commencing spread as an independent malady. A similar change in the character of the inflammation is also common in measles, especially during severe epidemics of diphtheria. And not infrequently an inflammation of the air-passages, apparently at first catarrhal, and probably so diagnosticated by the physician, presents in a few days the grave characters of the specific inflammation of diphtheria. It is, therefore, in my opinion an open question, whether the exudative inflammation of croup may not afford a favorable nidus for the reception and development of the diphtheritic virus, so that a case commencing as croup may become one of diphtheria. Perhaps this may explain the apparent fact that croup is less frequent in localities where diphtheria is established, than in former times.

[1] See the author's Treatise on the Diseases of Children.

Discussion on Dr. J. Lewis Smith's Paper.

After the reading of the preceding paper, Dr. William Pepper, of Philadelphia, said :—It seems to me that the recognition of a radical difference between diseases which possess strong points of resemblance and analogy, is a very delicate matter, and that the arguments in favor of such separation should be very clear and unanswerable. In the present case we have to deal with forms of disease characterized by the production of a peculiar pseudo-membrane upon the surface affected. Although there are many grades of severity, and therefore many differences in degree, between the lesions in different cases of croup and diphtheria, I am confident that no essential difference exists either in the microscopical or chemical characters of the pseudo-membrane in the two forms of disease, nor in its relation to the subjacent surface. This form of exudation is so peculiar and distinctive that a strong presumption would seem to exist in favor of the relationship of two affections in both of which it is developed. In considering the arguments against this relationship, which have been so fully and ably presented by Dr. Smith, I am unable to feel that they are convincing. We must not think of diphtheria only in its grave epidemic form, but also in the mild, sporadic form which so frequently occurs. I meet very frequently with isolated cases of undoubted pharyngeal diphtheria (for I would carefully exclude all cases of herpetic follicular angina with pseudo-membranous formation of a different kind), in which the constitutional symptoms are not very asthenic, albuminuria is usually absent, glandular swelling is not very great, there is no evidence of profound constitutional infection, and paralysis rarely follows. In such cases there is by no means the very marked difference from the general symptoms of membranous croup that has been assumed to exist.

It has frequently been my experience also, as I have stated elsewhere in connection with Dr. J. Forsyth Meigs, to find that cases which had been regarded as idiopathic croup, had really been preceded by slight faucial diphtheritic deposits, and were consequently undoubtedly diphtheritic in nature. It is true that the pseudo-membrane may first occur in the larynx in some cases, but I should be glad to see more careful and numerous records of such cases, where no pellicular formation in nares or fauces was present at any period. In different epidemics, and at different times, the proportion of cases of diphtheria in which the deposit occurs in the larynx is very variable. At times very few cases of croup occur; at others, the number is large; in a few epidemics, the larynx has been, as a rule, the first and only part affected. In these instances the attendant constitutional symptoms, and the rate of mortality, have been such as are found in grave epidemics of naso-pharyngeal diphtheria. Last winter I attended a case of membranous croup with highly sthenic general symptoms, in the eldest of three children. No faucial deposit was detected, but there had been nasal catarrh for a few days before the laryngeal symptoms appeared. The child died from suffocation, the operation of tracheotomy having been refused. In every respect this case resembled so-called idiopathic membranous croup; yet, before the child's death, the youngest sister sickened with a dangerous naso-pharyngeal diphtheria, from which she recovered without laryngeal implication. I might quote other cases of similar character.

Nor do I think the results of treatment afford a reliable basis of separation. We cannot regard diphtheria, any more than pneumonia, as though it had a constant and uniform type. There are cases of the one, just as of the other, which will bear and justify a treatment which would prove injurious in other instances, and I believe that the general treatment suitable for cases of sporadic pharyngeal diphtheria is also well adapted, with the addition of special means required by the mechanical obstruction of the larynx, to the treatment of sporadic membranous croup.

I shall add but a word on the argument of contagion. There is still need

of careful clinical work here, and the subject is now full of difficulty and obscurity. When epidemic diphtheria assumes the laryngeal form, the disease is clearly infectious or contagious, as is the pharyngeal form. When, on the other hand, pharyngeal diphtheria occurs in its mild sporadic form, any evidence of infectiousness or contagiousness is often wanting. I have known a mother, affected with an undoubted attack of this form of the disease, to nurse her child of thirteen months throughout the entire course of the disease without communicating it to the infant. I would refer, on the other hand, to the case of apparent sporadic membranous croup, above quoted, which was the cause, seemingly, of severe diphtheria in a younger sister. Is there anything in the anatomical conditions of the larynx and pharynx respectively—the much richer lymphatic supply of the latter, and the greater tendency to putrefactive changes in all exudations in that part—which may influence the liability to constitutional poisoning, and also to infectiousness, differently in the two cases?

I have no desire to assume a partisan position on this subject. I would be understood only as saying that the evident strong analogies between the two forms of disease, and the want of conclusiveness, as it seems to me, of the arguments in favor of their essential dissimilarity, make me feel it hazardous in the extreme to assert that they are radically distinct affections; and that I find myself more and more inclining to the view that they are only different forms or types of one pathological condition.

Dr. Henry Gibbons, of California, said:—When you, Mr. Chairman, and myself attended lectures in this University, no such word as diphtheria was known in the English language. Since then we have, by slow degrees, long study, and diligent practice, become convinced that there are two distinct forms of disease—one which we know as croup, and the other as diphtheria. Years ago we invariably bled for an attack of croup, and nearly always cured it.

Dr. J. J. Woodward, U. S. A., said:—I would like to ask Dr. Smith if, in preparing his paper, he consulted the first or second edition of Rokitansky's work? I thought from the reference to this eminent pathologist's views that his later edition, in which he speaks of diphtheria, had not been consulted.

Dr. C. J. Hare, of London, said:—This question of the identity or non-identity of croup and diphtheria is receiving a great deal of attention on the other side of the Atlantic as well as on this. At this time there is a sub-committee of the Royal Medico-Chirurgical Society of London investigating the subject. I admit that we should search after truth, but I think that medical men spend a great deal of time in re-determining questions that have already been determined. I think that after hearing the paper just read, there can be no doubt about there being two diverse and distinct diseases. The microscope has failed in other cases as well as in this. There appear to be distinctions which it is impossible to overlook. The symptoms of the two diseases have been so fully entered upon by the author of the paper that it would be out of place for me even to recapitulate those which are, as I conceive, distinctive between them; but there are two circumstances or conditions which, taken alone, prove to my mind the diversity of the two affections. I allude to the contagiousness of diphtheria, and to the paralytic affections which frequently result from it. Those who knew croup some thirty or forty years ago, and before the prevalence of diphtheria, will remember that the disease they then knew was never contagious, and was never followed by paralytic affections. Nor do the old authors, who described so graphically the symptoms of croup, ever mention these points. Will any one venture to say that if the contagiousness and the affections I have alluded to had been part and parcel of croup they could possibly have been overlooked, or could have failed to be described? No; they were not referred to, for the simple reason that they did not exist; and they did not exist then, because the disease at that time known was true croup and had no such symptoms; whereas, in recent years, we have also had to deal with another and different disease—diphtheria, of which contagiousness and the

sequelæ I have named are characteristic conditions. Our treatment has been modified, not because one disease has run into the other, but because we have two distinct diseases to deal with. The result as regards treatment, leaves no doubt whatever in my mind as to the non-identity of these two affections.

Dr. R. P. HOWARD, of Canada, said:—I cannot believe with the last speaker that this matter is so definitely settled in the minds of the scientific world. The fact that the Medico-Chirurgical Society of London has just appointed a committee to inquire into the matter proves this to be so, and the circumstance that the French school still regards membranous croup as laryngeal diphtheria, proves that the scientific mind is not agreed upon the question. I think that there is a good deal of truth expressed in the paper read on the subject, and that we will have to work honestly and fairly for a long time to come, before we can controvert some of its statements. If a large number of cases of so-called membranous croup were investigated after death, we might have the data by which to arrive at some truth on the subject.

Dr. W. OLDRIGHT, of Canada, said:—In 1865, while I was practising on the shores of Lake Huron, there was an epidemic of sore throat of a peculiar character. During that time I think I met with one case of true diphtheria. On a Thursday night, a young man became intoxicated, and lay out all night; he was attacked by the disease, and on Sunday morning gangrene set in, and by evening the line of demarcation appeared. Shortly afterwards he died of strangulation. The young man's brother was also attacked, and became paralyzed and remained so for about four months. Another remarkable case was that of a boy about eleven years of age, who on being attacked became perfectly black in the face and throat. His face and ears were swollen terribly, but his intellect remained perfectly clear. He died in two or three hours.

Dr. R. BARTHOLOW, of Cincinnati, said:—As a clinician I have held that the differential diagnosis of croup and diphtheria rested on three conditions—the local affection, the constitutional state, and the sequelæ. In croup, the exudation or false membrane lies in contact, merely, with the mucous membrane; in diphtheria, the exudation extends into and involves the epithelial layer of the mucous membrane, and the neighboring vessels, especially the lymphatics, are crowded with bacteria. Swelling of the sublingual and submaxillary glands accompanies various faucial inflammations, but in diphtheria the chain of lymphatics lying under the sterno-cleido-mastoid muscle is involved, while it is not in croup. As respects the constitutional state, the adynamia is much more strongly marked in diphtheria than in croup, and bears no proportion, necessarily, to the extent of the exudation in the fauces. No sequelæ follow croup. When the local process in the air passages ceases, the disease is at an end. On the other hand, in diphtheria, various sequelæ follow—albuminuria, paralysis, etc. Donders, in his Anomalies of Refraction and Accommodation of the Eye, makes the capital observation that the ocular and other paralyses which follow diphtheria are not determined by the amount of the faucial exudation, and often follow in cases which were supposed to be ordinary cases of sore throat, and in which no exudation had been observed. The contagiousness of diphtheria appears to be well established. If this be admitted it constitutes a fourth means of diagnosis.

Dr. T. L. MADDIN, of Tennessee, said:—In a recent number of the British and Foreign Medico-Chirurgical Review, the unity or duality of croup and diphtheria are fully considered: and from this it appears that recent authors of merit, treating upon the subject, leave the question in doubt. Thus it is evident that in the minds of many there are intimate links of connection between the two affections, if indeed, they are not grades of the same disease. With a large clinical experience, my convictions are clear that pathologically they have no kinship. Croup is a local disease, and its fever symptomatic; its etiology is various, even a common cold often causing it; its nature a local inflammation, with a false membrane strictly on the surface, and never undergoing necrosis. Diphtheria has a specific etiology, and is an essential fever.

Its false membrane, occurring mostly in the pharynx and tonsils, often invades the nasal, laryngeal, and bronchial mucous membrane. It penetrates the mucous and often the submucous structure, and undergoes septic decomposition. It may form on any portion of the body where there is a lesion. The fever is adynamic, and the convalescence slow. Albuminuria is present as in scarlet fever. The treatment of the two is totally different. I am, therefore, clearly of the opinion that croup and diphtheria are distinct diseases widely separated from each other.

Dr. H. P. Ayres, of Indiana, said:—I have seen croup prevail largely, and yet no diphtheritic symptoms accompany it: but I have never yet seen diphtheria prevail without croupy symptoms attending it. I do not think the disease is contagious, although it may be classed as such. I have seen cases in which one of a family was attacked and died, while the others escaped. Had the disease been contagious more than one would have been affected.

Dr. C. C. Hamilton, of Nova Scotia, said:—I have had experience with both diphtheria and croup, and think there cannot be any very great similarity between the two. I have had one or two cases which would tend to prove that croup is contagious, but as a general thing it is not. I have observed that since diphtheria has prevailed croup has been a much rarer affection than formerly.

DO THE CONDITIONS OF MODERN LIFE FAVOR SPECIALLY THE DEVELOPMENT OF NERVOUS DISEASES?

BY

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"THE wear and tear of our modern civilization," is a current phrase which indicates the general belief, lay and professional, that the struggle for existence in our time imposes an excessive and consequently injurious labor on the brain and nervous system of man. That the injury thus done manifests itself in an increased production of nervous diseases, seems to be generally conceded. Without having bestowed much reflection on the subject, I had accepted this popular view; but when I came to investigate the facts which are available for the solution of the problem, in preparation for this paper, I felt obliged to change my opinions. To the question "Do the conditions of modern life favor specially the development of nervous diseases?" I reply in the negative.

At the outset, it must be admitted that the evidence is largely presumptive. No statistical data have descended from ancient times, and, indeed, as respects so recent a period as the last and the present century, an exact numerical expression of the relative prevalence of nervous diseases, is not possible. In the absence of statistical evidence, the inquiry must be directed into other channels. An approximation to the degree of current familiarity with mental and nervous maladies, may be made by a study of the references to such diseases in the ancient secular writers. The actual state of knowledge of mental and nervous maladies, as compared with other diseases (of women, for example) will be exhibited by the ancient medical writers.

Insanity is distinctly portrayed in some of the characters of the Greek drama. The tragic story of Orestes furnishes the material for two of the most powerful dramas of the Grecian stage. Both by Æschylus and Euripides, is the hero, Orestes, made to become insane by remorse after the death of Clytemnestra. The form of insanity which they depict is acute mania, and after a certain period reason is restored. Probably the best picture of mania which has descended to us from the ancient world, is the Ajax of Sophocles. This is delineated with a master hand, familiar with the malady described. Indeed the three dramatists exhibit a degree of familiarity with the salient features of an acute maniacal attack, which could only have been obtained by a personal study of the paroxysms. We obtain glimpses from the comedies of Aristophanes, that cases of senile dementia were not unfrequently subjects of judicial investigation. In his comedy of "The Clouds," which was directed against the sophists, and especially against Socrates, it is indicated that the young men of Athens employed such means to be rid of troublesome seniors. It is an historical fact that Sophocles in his old age was the subject of an inquiry into his mental condition, instigated by his sons who wished to obtain possession of his property. Even Socrates was the subject of a delusion—his inward voice, *dæmonium*—which guided all his actions,

however trifling, and to which he invariably deferred. The *dæmonium* of Socrates has been the subject of an immense deal of speculative subtilty. Without presuming to decide a question so surrounded with difficulties, I may at least express an opinion formed after an examination of all the sources of information within my reach: To me, this voice, inward mentor, conscience, or *dæmonium*, appears to be a hallucination. It should be remembered that Socrates was given to introspection, and had a character full of eccentricities.

In Grecian and Roman mythology, madness is frequently inflicted for disregard of the commands of divinities, in accordance with the popular notion that the anger of the gods is thus visited on man. Bacchus was one of the divinities who most used this means of punishment. He thus afflicted Lycurgus, of Thrace, and the three daughters of Prætus, because they declined to worship him. These insane women are represented to have wandered in the fields like cows, and, quite in accordance with what we now know of the contagiousness of imitation, many Argive women went mad and did the same thing. The "heaven-inspired Cassandra" was doubtless insane, as the Trojans believed. It is a very old trick to feign insanity; Ulysses was the first on record to attempt it.

In the ancient secular writers references to the "sacred disease" are very numerous. Herodotus chronicles the fact that Cambyses was afflicted with this malady, and, very remarkably, he attributes the cruelties practised by this ruler to the mental state induced by the epileptic paroxysms. Gallius and Macrobius quote from Hippocrates in regard to the "sacred disease." In the former occurs the curious statement (attributed to Hippocrates) that the sexual congress is a condition of the body not unlike the epileptic paroxysm. Horace, in his epistles, describes melancholy and paralytic tremor.

Erb infers that neuralgia must have been comparatively rare in ancient times, because "Aretæus is the first who gives a recognizable description of it."[1] Yet Socrates, in the Phædo of Plato, is made to utter some remarkable thoughts on the relationship of pleasure and pain. "What an unaccountable thing, my friends, that seems to be, which men call pleasure; and how wonderfully it is related towards that which appears to be its contrary, pain; in that they will not both be present to a man at the same time; yet if any one pursues and attains the one, he is almost always compelled to receive the other, as if they were both united together from one head." These observations do not, it is true, prove the existence of neuralgia in ancient times, but they show that the acute-minded philosopher had reflected profoundly on the nature of pain, anticipating, in his conclusions, modern knowledge of the functions and attributes of the sensory nerves. These examples from the secular writers must suffice. Doubtless most of the members of this learned body will recall other and, it may be, more pertinent instances; but I must pass to consider the state of knowledge of nervous diseases shown by the medical writers of antiquity.

The writings of Hippocrates furnish us with numerous references to the more important nervous maladies. In the Prognostics, is delineated with admirable fidelity and brevity that form of delirium in which the patient picks at the bedclothes and objects in the air. In the Regimen in Acute Diseases, there is a distinct allusion to trembling delirium due to vinous excess. In the book of Epidemics, cases of "phrenitis" are

[1] Ziemssen's Handbook, vol. xi.

given, the main features of which are much like those of modern cerebro-spinal meningitis. The earlier writers made no attempt to differentiate in the forms of intra-cranial inflammations, for reasons which are perfectly obvious; although under the term *sphacelus* of the brain Hippocrates probably meant *ramollissement.* In the Aphorisms, we find a great many very acute observations on the signs and symptoms of many nervous affections. The sleep which approaches coma, and the sleep which puts an end to delirium, are described, and the prognostic value of sopor and insomnolence clearly stated. The insensibility to pain manifested by the insane is recognized. Hippocrates declares that the prognosis of insanity is more serious after forty years of age, a fact which, we must admit, is generally true. "It is better that a fever succeed to a convulsion, than a convulsion to a fever," is an undoubted truth, which indicates that Hippocrates recognized the clinical difference between those convulsions which announced the onset of a fever, and those which came on at the termination of an acute intra-cranial inflammation. He indicates his acquaintance with aphasia in various aphorisms which refer loss of speech to injuries of the head, vinous excess, etc. He frequently alludes to vertigo, apoplexy, and tetanus, and is minute on the subject of epilepsy, the prognosis of which, he informs us, is more favorable when the disease occurs in the young than when in the old. In a description of the effects of venery, Hippocrates draws a picture of nervous symptoms not unlike those belonging to the first stage of spinal sclerosis. It is known to all here present how pronounced are the sexual symptoms in Duchenne's first stage of progressive locomotor ataxia.

Some of the medical writers subsequent to Hippocrates are more minute and systematic in their descriptions of nervous diseases. Aretæus, who was most probably a contemporary of Galen, describes *tic douloureux* (On Cephalæa), vertigo, and the vertiginous sensations which accompany various intra-cranial diseases, epilepsy, melancholy, mania, ecstasy, apoplexy, paraplegia, hemiplegia, and paresis, and mentions disturbances of the functions of the cranial nerves such as are produced by intra-cranial tumors. The most complete account of nervous diseases that has descended to us from the ancient world is found in Galen.[1] The different varieties of headache are very admirably depicted by this writer, and he indicates his acquaintance with the fact that the symptoms are associated with various intra-cranial lesions. He treats of abscess, meningitis, coma-vigil, catalepsy, hysteria, melancholia, mania, imbecility, senile dementia, vertigo, epilepsy, apoplexy, paralysis, local paralyses, spasms, tetanus, paralytic tremor, etc. Writers subsequent to Galen, for many centuries, as is well known, adopted his descriptions. It would occupy too much time to give a detailed account of the opinions of other and less authoritative ancient medical writers. It must suffice to state that Aëtius, Oribasius, Alexander, Cælius Aurelianus, Celsus, Avicenna, and Rhazes, besides other less important ancient physicians, treat of nervous maladies at very much the same relative length. It is quite evident, then, that various diseases of the nervous system were perfectly familiar to the writers of the ancient world.

If we compare the manner in which the ancient medical writers discussed the various diseases with which they were acquainted, we find that affections of the nervous system occupied as much space in their works as the diseases of other parts. Probably the knowledge of the

[1] De Compositione Medicamentorum secundum Locos.

ancients, as compared with modern acquirements, was more full and exact on the subject of the diseases of females than of any other group of maladies. Some instruments of precision which have contributed so largely to the development of gynæcology, were known to and used by the most ancient practitioners of whom we have any knowledge. The speculum, the uterine sound, and the pessary, were, as is well known, only rediscovered in modern times. An attentive examination of the ancient books on female diseases must convince any one that women have always suffered from the physical infirmities to which, in our day, they seem to be specially liable. The ancient physicians had every opportunity, we may suppose, to become practically familiar with female diseases, and the information conveyed to us in their writings is marked by the accuracy of detail only secured by faithful observation. When we compare the amount of space devoted to nervous and female diseases, respectively, it is evident that the former are treated with as full detail as the latter. If we take the works of Paulus Ægineta, who, according to his own statement, "compiled his collection from the works of the ancients," without omitting "any one distemper," we can see that affections of the nervous system occupy quite as much space as diseases of women, and are treated as intelligently.

When the ancient writers allude to the causes of diseases affecting the nervous system, they are said to be heat, cold, injuries, and excesses in eating, drinking, and venery—just such causes as in modern times are held to be influential. The most superficial survey of ancient society must convince any one that the nervous system of man was, in those days, exposed to extraordinary trials. The writings of Aristophanes, of Lucian, of Athenæus, amongst the Greeks, of Martial and of Juvenal amongst the Romans, give us hints of a licentiousness to which the most debased societies of modern times afford no sort of parallel. The political status of the ancient civilized communities was never permanent. Wars and revolutions, characterized by the greatest atrocities, occupied the restless citizens of the Grecian and Roman States. At home, games, public amusements, constantly recurring political excitements, the worship of Venus and Bacchus with their attendant orgies; abroad, wars, conquest, and plunder, were the occupations of Athenian and Roman citizens.

The minute subdivisions into which the medical art was parcelled amongst the ancient Egyptians, and later amongst the Romans, indicated that in those times diseases were very rife. In Cicero's age[1] the only divisions were three—physicians, surgeons, and oculists; but when Galen wrote,[2] the bad example of Thessalus had filled Rome with a great crowd of hungry specialists in every possible field of medical practice. There were specialists in remedies, as well as in diseases, and in the diseases of every known organ of the body. There were specialists for the globes of the eyes, and for the eyelids; for the uvula, and for the tonsils; for the external ears, and for the internal ears; for palsy, dropsy, stone, and skin diseases; for children, and for old people. There were apostles of the movement-cure, or those who treated rheumatic and paralytic troubles by gymnastics, *massage*, and frictions. Galen contemptuously styled these multitudinous charlatans, the asses of Thessalus; for this magnificent, impudent, and successful quack had declared that he could teach any doctor in six months all that he needed to learn. We obtain from Pliny a lively picture of the ignorance and rapacity of these specialists:[3]

[1] De Oratore. [2] Methodus Medendi. [3] Historia Naturalis.

"I will not speak of their avarice, their swindling bargains made with patients in an extremity, the imposition laid on suffering, the earnest money, so to speak, of death, the secret retainer fees," etc. Alluding to the same class, Galen affirms that the doctors in Rome differed from highwaymen only in the fact that the former lived in the city, and the latter camped out in the mountains.

These facts indicate, I think, that the diseases incident to Roman life were very numerous, and hence the demand for physicians; otherwise the supply had not been so abundant. In this strange medley of specialists, there seem to have been as many, comparatively, devoted to maladies of the nervous kind as to other diseases, and certainly during the times of Galen and Pliny there were in the social and political condition of the Roman people powerful causes in operation to produce these affections.

From the thirteenth to the sixteenth centuries, there occurred in Europe epidemics of purely nervous maladies such as the world had never before witnessed. In the graphic pages of Hecker, we learn that the dancing mania, in Germany and Holland, tarantism in Italy, and the malady of the *Convulsionnaires*, in France, involved great numbers in the wildest excesses. Such moral epidemics are possible only in such a state of society as existed in the mediæval times—an age of faith and reverence, but ignorant and superstitious. During the same period great epidemics of cerebro-spinal meningitis devastated various parts of Europe, notably near the close of the fifteenth and at the beginning of the sixteenth centuries. No additions during the middle ages appear to have been made to the knowledge of Galen. In a voluminous work on "The Generall Practice of Phisicke," "compiled and written by the most famous and learned Doctor Christopher Wirtzung," and published near the close of the sixteenth century, the descriptions of Galen are closely followed. In his commentary on "the braines and all that concerneth them," the learned Doctor includes loss of memory, vertigo, phrenitis, mania, melancholia, dementia, palsy, tremor, spasm, incubus, epilepsy, and sciatica. It is perfectly obvious that the state of medical knowledge then existing did not admit more accurate differentiation in nervous maladies.

The present condition of neurological medicine has been reached only since the physiology of the nervous system has been adequately studied within the last fifty years. Until Bichat had laid the foundation of general anatomy, physiology had scarcely existed. The brilliant results attained by the study of the nervous functions by the modern methods of research, and the great development of morbid anatomy, have stimulated investigation of the diseases of the nervous system, and to the operation of these causes must be attributed the remarkable advance in neurological science. Diseases of the nervous system have been accurately differentiated, and have been elaborately studied both from the clinical and pathological standpoint; but it is more than doubtful if any have been actually discovered in modern times. Various attempts have been made in France, England, and the United States, to determine, by the statistical method, whether an actual increase in the number of insane, relatively to the growth of the whole body of the population, has taken place in modern times. It is extremely difficult to make a thoroughly exact census of the insane. Each succeeding registration is probably more exact than those which preceded it; but even in England it is not claimed that the returns are more than an approximation to exactitude. It is undeni-

able that in each decade a small increase in the number of insane has been noted; but it is conceded in every report on the subject that the registration, becoming more and more exact, makes an increase which is merely apparent. Moreover, a more humane and abundant provision for the insane, and the increased facilities for communicating with asylums, induce the friends of these unfortunates to entrust their keeping to the great institutions now provided for the purpose.

If the kind of evidence which I have submitted has any value in deciding a question of this kind, it must be admitted that the various forms of nervous disease, including insanity, are not more numerous, relatively, now than in ancient times. If, as is probable, nervous diseases are not more numerous than they have always been, an inquiry into the conditions of modern life supposed to have increased them, may appear to be unnecessary. A candid examination of those conditions will demonstrate, I think, that their influence has been greatly exaggerated. The abuse of stimulants and narcotics, although not unknown to the ancients, is commonly held to be more extended in modern times. Certain poisons, dye-stuffs, cosmetics, and deleterious metals of various kinds, enter more largely into domestic use than was the case in former ages. Our modern means of intercommunication by railways, subject the nervous system to a degree and kind of concussion unknown to the people of ancient times. Modern habitations, with their appliances for heating and lighting, social customs, diet, and all the methods of an enlightened hygiene, are very different from those used in the most polished periods of Egyptian, Grecian, or Roman life.

It is probably an error to suppose that alcoholic excess is more common in our day. Wine was the beverage chiefly used in ancient times, as it is now in the wine-growing countries bordering the Mediterranean. Drunkenness is an uncommon vice in those countries to-day. The feasts of Bacchus, in ancient times, were scenes of extraordinary drunkenness and debauchery, which must have initiated thousands into habits of intoxication. The god visited madness on those who refused to engage in the bacchanalian rites. The symposia and orgies, in which the upper classes, Grecian and Roman, engaged, consisted in an abandonment to the vinous intoxication. That such indulgences brought upon the victim the usual punishment, is evident from the references in Hippocrates to delirium tremens and aphasia from vinous excess. It cannot be denied that the chief factor in the causation of insanity, and an influential one in the production of other nervous maladies, in our day, is alcoholic excess, but it must have been equally so in ancient times.

Although the properties of opium were certainly known in the first century, it was not used as a means of intoxication until the second, and then, certainly, to a very moderate extent. Its systematic use in civilized communities is a comparatively modern vice, and even now, although indulged in to an extent which must awaken solicitude, is as yet confined to an inconsiderable part of the population. Opium smoking has attained to extraordinary proportions in China, and it is estimated that twenty per cent. of the population of that country is now addicted to it. A recent observer concludes "that disorders of the nervous system in a general sense, are by no means infrequent amongst the Chinese, . . . the probabilities are that the prevalence of diseases of the general nervous system in China bears some proportion to the prevalence of the same disease in Europe, and other regions, . . . but that cases of alienation of mind, idiocy, lunacy, fatuity, and generally

speaking, insanity, are strikingly few, if we leave suicides out of the reckoning."[1]

Tobacco has now been used by civilized peoples for three centuries. The greatest evils were supposed to be produced by it when Sir Walter Raleigh and his companions first made it the fashion. The large experience and observation of its effects which have been acquired, demonstrate that its moderate use produces no ill results, and that the evils which come of immoderate indulgence are quickly recovered from when the narcotic is suspended. As the disturbances in the nervous system induced by tobacco are functional, we have in this fact an explanation of the apparent freedom from injury of those addicted to its habitual use.

Various modes of contamination by poisonous metals—lead, copper, mercury, etc.—are possible by reason of the employment in domestic life of articles of necessity and luxury, containing them, and by the various applications of these metals in the industrial arts. Individual examples of deleterious results—affections of the nervous system—are not uncommon, but no general or diffused injury has occurred, or probably can occur.

The daily railway travel of a considerable portion of the population of civilized countries, has been supposed to exert an injurious influence on the nervous system. The jar and concussion, the deafening noise which assails the ear, and the rapid movement of objects which fatigues the eyes, are supposed to impair the integrity of the nerve centres. One of the diseases alleged to be caused by railway travel is progressive locomotor ataxia. If ill results are thus produced, they ought to be seen in the locomotive engineers. On the contrary, the Brotherhood of Locomotive Engineers is a body of men who are remarkably free from nervous ailments, and who are indeed distinguished for their vigorous health.

As respects the social and domestic conditions of modern life, it can hardly be disputed that modern man is in every respect more favorably situated than was his ancient prototype. If statistics are worth anything, it must be admitted that the improved hygienic conditions of modern society have lessened the sickness and mortality rates, and have increased the longevity of man. The epidemics as described by Hippocrates; the plague as depicted by Thucydides; the black death of the middle ages; the ravages of smallpox in the last century, have been imitated, but not at all equalled by the Asiatic cholera in this century. Those great epidemical waves of cerebro-spinal meningitis which formerly overflowed Europe are no longer possible. A great moral epidemic, like the dancing mania, would be as much a solecism in this nineteenth century as a revival of the crusades.

The Geneva vital statistics, with the observation of four centuries, and the experience of the great life-assurance societies, based originally on the Carlisle tables, seem to have established the fact of the increasing longevity of man. If man is living under more favorable hygienic conditions, and is less vulnerable in his bodily constitution, it must be admitted that he is improving rather than degenerating. Is it not to the last degree improbable that one system of the body should manifest signs of premature decay, and the rest exhibit unwonted vigor? Such a supposition is all the more incredible when we come to reflect on the important position which the central nervous system holds in respect to the nutrition and healthy functional activity of other parts of the body. It

[1] Dr. George Hearn, Journal of Mental Science, April, 1875.

is as little likely that more men, comparatively, are bald in our day than were so in ancient times. These facts seem conclusive against the views of those pessimists who maintain that the nervous system of man is degenerating under the teasing influences of our modern civilization.

Instead of degenerating, it seems highly probable that the brain of man, like his bones and muscles, is capable of more work than formerly. Modern thinkers rival the metaphysical subtlety of Aristotle, and an ordinary man requires to carry an amount of material as the usual furniture of his brain which would have conferred immortality on an Athenian. The brain in its state of functional activity, indeed, illustrates most aptly what has been styled the principle of least action. A large part of our mental processes are carried on without our consciousness—a state of functional activity to which it is the fashion to apply the term "unconscious cerebration." So nicely adapted is the organ to the work it has to perform, that it accomplishes its tasks without any jar, without indeed disturbing its very sensitive tenant, the Ego. The wear and tear of our modern civilization no more affects the integrity of this organ than the discussions in the groves of the academy, nor than the political dissensions of the Grecian and Roman states.

The following conclusions seem to be warranted by what has gone before:—

I. Nervous diseases, in the most common forms, have been distinctly recognized by lay and medical writers from the earliest periods.

II. They appear to have been as prevalent, relatively, in ancient as in modern times.

III. The sickness and mortality rates having lessened and the longevity of man having increased in modern times, it is highly improbable that the nervous system of man has become more vulnerable.

IV. In modern times nervous diseases have been more accurately studied and differentiated.[1]

Discussion on Dr. Bartholow's Paper.

After the reading of the preceding paper, Dr. Traill Green, of Easton, Pennsylvania, said:—No doubt the ancients were well acquainted with nervous diseases. Apoplexy was well known in Scriptural times, and we find that many characters in Biblical history suffered from it. When a man is attacked by apoplexy now, people are apt to say that he has suffered from overstrain. But we find that the ancients suffered from the same affection, and they had not the business strain of the present day. Statistics compiled by men familiar with the subject, and endorsed by mathematicians, show that there is an apparent increase in deaths from nervous diseases among the policy holders of insurance companies; but this is attributed to the fact that more old people insure now than formerly. The percentage of insanity is just $\frac{53}{100}$ of one per cent., and the number of apoplectics is not large. I have closely observed nervous diseases in my own neighborhood, and out of about fifty persons whom I have known to die or suffer from apoplexy, or palsy, not one could be said to have owed his attack to the result of overstrain. The ages of these patients varied

[1] Thirty minutes was the limit fixed by the executive committee for the length of papers read in the sections. The writer regrets that he was not aware that deviation from this limit would be permitted, for he would otherwise have presented his argument more in detail, and would have entered more fully into the statistical data which exist as to the comparative prevalence of nervous diseases in the first and latter halves of the nineteenth century.

from 58 to 92 years, at the time of attack, and they were from all walks of life.

Dr. Henry Gibbons, of San Francisco, said :—There is nothing in the records of antiquity nor in modern statistics to settle this question. The statistics of insurance companies, derived from selected subjects, are of no avail for the purpose. Mortuary statistics refer only to deaths, and not to the prevalence of diseases. Even if it were proved that nervous disorders caused as great a proportionate mortality in former times as now, it would not reach the question; for with the great advance in pathology and treatment, the mortality ought to diminish. If Dr. Bartholow were to reside a year or two in California, he would change his mind as to the increase of nervous disorders in that quarter. The increase is conspicuous, and there are causes operating there which must have that effect. With a large portion of the population, the home and family influences are wanting, and men in trouble and difficulty are adrift in the world—no wife, no parents, no children to anchor their troubled spirits. Passions of all kinds get control. Very recently the wild speculations in mining stocks, by which fortunes are made or lost in a day, have developed a notable number of cerebral cases, ending in suicide or insanity. Whether modern society in old and settled communities exhibits any increase of nervous disorders, I am not prepared to say; but as like causes produce like effects, and as there is every reason to believe that moral and physical causes of a disturbing character are more abundant in modern times than formerly, we might reasonably infer that disorders of the nervous system would be more frequent, though their aggregate mortality may not be greater. It must be conceded, however, without qualification, that in newly-settled countries, and all around the outskirts of the old civilization, in all parts of the world, the circumstances of society are more or less similar to those I have pointed out in California, and that they must lead to similar results.

Dr. Wm. B. Neftel, of New York, said :—I have no doubt that the causes of nervous diseases have increased in modern days. The subjects of these affections transmit them to their descendants in an aggravated or modified form. We must also consider the law of development. The brain's intellectual sphere has been enlarged, and, with increased mental activity, we have greater liability to nervous diseases. The common use of alcoholic stimulants at the present day, is, both directly and indirectly, a cause of nervous affections. Syphilis, another cause of nervous diseases, is not found to have existed in former times, while now it is very common. The records of the Pathological Institute of Berlin prove conclusively that many fatal diseases of the brain are due to syphilis.

Dr. Thomas L. Maddin, of Nashville, Tenn., said :—I agree with the author of the paper that there are no new diseases to which man is subject, for all disease is but an error of the tissue or function of an organ. But because of education and the surroundings of modern life we may have new modifications of disease, and we often have a physical basis on which to rest those modifications. The exercise of any organ of the body will increase its size and structure, and in that way may modify the diseases of an organ. For instance, a man living luxuriously, and constantly using stimulating condiments and alcoholic drinks, will in the course of time have his liver in an abnormal condition, and then there is a physical basis for organic and functional hepatic disorder. He is predisposed to diseases which another man, living on frugal, plain diet, while subject to the same affections, is not so likely to have developed. People are different now from what they were two thousand years ago, or in their primitive state. People in civilized life differ from savages in reference to the development of the nervous system. Take a man on a plantation, with no education, and no elevation of thought, and take another who has been precociously educated from childhood; the brain of the latter is in a state of hyperæmia; his nervous condition has been stimulated from the beginning; and there is an abnormal condition of his entire nervous system. It is educated

out of relationship to the other organs of his body. It has obtained an ascendency over his system which influences the functions of the other organs. Thus the highly educated and civilized man is more subject to nervous disorders than the quiet countryman.

Dr. A. B. Arnold, of Baltimore, said:—The etiology of all diseases is obscure, but particularly so that of diseases of the nervous system. I believe that the idea that nervous diseases are more prevalent now than formerly has originated with specialists. I have practised for thirty years, and am not aware that I have seen more cases of nervous disease during the latter than during the former half of that time. If the increase spoken of exists, it must be attributed to the progress of civilization, to the wear and tear of body and mind, and to the race for wealth. Intellectual men are generally long-lived. I believe that shocks are more detrimental than hard study, and, as pointed out by the reader of the paper, war and turmoil with all their attendant evils were the almost natural conditions of former times, while now they are comparatively rare. In modern civilization, the mind is better trained and disciplined. It is not so demonstrative in joy, nor so depressed in trouble. Discipline prevents the mind from being affected with shocks. I believe, although it is rather Darwinian to think so, that the nervous system accommodates itself to the strain put upon it—an accommodation of the inward condition to external influences, which is in favor of the present time. I am not aware that even special writers on the subject particularly ascribe insanity to a syphilitic condition; in fact, the subject is so extremely obscure that it is hazardous to express an opinion upon it. Again, medical men allow themselves to be too much influenced by the hue and cry of professional temperance orators. It cannot be scientifically proved that alcohol in the form in which it is at present used has a very marked influence in the production of nervous disease. I believe that, instead of drunkenness being a cause of mental disease, it is in nine cases out of ten only the first warning of its approach.

Dr. N. S. Davis, of Chicago, said:—This question is one of very wide scope, and one which it will be extremely difficult to determine with any degree of certainty, with our present knowledge. Even in this discussion, the fact has been brought out that in certain conditions of society there is to be found a marked increase of particular nervous diseases. The gentleman from the Pacific coast has indicated a condition of modern society which has a tendency to develop that form of nervous derangement that leads to insanity. Go back to the days of the Crusades, when all Europe was aroused by a religious mania, and was led forth to the battle field by hundreds of thousands, and you will find conditions which were likely to, and which doubtless did, produce special tendencies towards the development of mental disease. People talk about mental strain being a cause of mental disorder; about the intensity of our work: I have been led to think that very few brains ever wear out by work. The physiological law is growth; strength; compactness. If there is not some corroding influence to interfere with the law, you will seldom wear out your brains with work. Rush tells us of the ten years before the Revolution, when a large part of the male population of Philadelphia was aggregated in social clubs, eating four meals a day, and drinking intoxicating liquors at each; and he lived to see almost every member of those clubs go to his grave from the effects of apoplexy, paralysis, or dropsy: there was a development of nervous disease. The Revolution followed, there was excitement and war; the people were stripped of their property, and were compelled to labor and toil mentally and physically; and that particular class of nervous diseases almost disappeared.

No doubt some special nervous diseases have diminished, while others have increased. The reason for this increase of particular classes of disease is not our high state of civilization, nor our extent of learning; I doubt whether we have a student in America, to-day, who studies harder than some of the students of Greece and Rome, or even of the ancient Jews. I doubt whether

some of them did not think as deeply and persistently. And no doubt some of them suffered from mental disease, but not from thinking or study. I think Saul got his paroxysms of mental derangement, as a great many do to-day, from *wickedness;* not from study. It is not the hours we spend at books, or at business, that wear us out; much is attributable to our anxiety for wealth and show. When we put our whole heart on accumulating wealth, and the bubble bursts, it produces a severe mental anxiety. Large numbers of our students become lean and dyspeptic, and disordered in health, before they get half through college; why? Because they learn at an early period of life to go to the breakfast-table, and sip two cups of coffee and nibble a crust of bread as an apology for breakfast; thus endeavoring to substitute pure nervous excitation for materials to replenish blood and make tissne. It is the same with many lawyers and doctors, whose friends say they are killing themselves, and yet who do not devote to their professional duties as much time, by one-third, as others do, who are perfectly free from nervous disease. What is the reason? Because as soon as they take their meals they are puffing their tobacco, or chewing their *quids*, until they have their entire nervous systems saturated with a narcotic that cannot fail to produce retardation of molecular changes, thus acting upon the false notion that the *retention* of an *old atom* is equivalent to the *evolution* of a *new* one; and in a few years they complain of being worn out. They have retained the old atoms in the form of inert fat, which begins to find lodgment in the brain, or liver, or heart. If an artery gives way, and they fall from apoplexy or palsy, you give your certificate that they have died from one of these diseases, when the truth is that they have died from long use of alcohol and tobacco. Take these two influences out of the community, and you will have less nervons disease than the ancients, because the other influences are of such a nature that we can guard against them. We would live longer, the race would be healthier, and we would have fewer diseases of any kind. These things have stepped in and filled up the places of some things peculiar to the ancients. No doubt causes have been operating all through the world's history, sometimes making one form of mental disease more prevalent than another. Balance them up, and we may find that we are very nearly where we were in former years. Our advances in sanitary science, and in pathology, and in the treatment of disease, have lessened mortality and increased the duration of human life. If the prevalence is the same as in former times, then we must have more influences at work.

Dr. R. P. Howard, of Montreal, said:—I think that previous speakers have established the fact that nervous diseases are more common than formerly, owing to the use of stimulants; alcohol was used by the ancients, but tea and tobacco are modern, and are potential causes of derangement of the nervous system. Syphilis is another modern cause of nervous disease. One of the speakers has tried to ignore the importance of this, but its influence is well established. The undue attention of modern civilization to culture of the brain, over-developing it in relation to the other organs, may be another cause. Hereditary influences will wear themselves out, else every person would inherit some taint. A man may have too much work as well as too little. The men of to-day work faster than men in former times; everything is done in a hurry. The weight of evidence is in favor of the view that there is a tendency to an increase of nervous affections.

Dr. H. I. Bowditch, of Boston, said:—There are one or two points which have not been noticed. I am satisfied that the climate of this country is more stimulating than that of others. I infer this from the general effect it has upon the English race. The Anglo-Saxon race has grown thin and apparently more excitable than it was in the mother country. English people have found that they cannot safely drink to such an extent as they can in England. I have known cases in which Englishmen in America have been attacked by delirium tremens, from using quantities of alcohol which they had used with perfect impunity in England. Yet they were comparatively temperate men. Again,

there is the perpetual political excitement, affecting both our men and women, which has no existence in Europe. When an American goes to Europe, he gets into a state of quiescence which is perfectly delightful, but when he returns, even after spending six months there, the first sight of the pilot boat puts him into an excitement about the latest scraps of political news.

Dr. GREEN said:—No inference can be drawn from the frequency of suicide, as suicides are committed from other causes than the existence of nervous disease. The Welsh are more predisposed to mental and nervous diseases than other people of Great Britain, so that the question of race should be considered in connection with the subject.

Dr. W. SCOTT, of Cleveland, Ohio, said:—Our modes of travel induce diseases unknown to our forefathers. The custom of sending children to school at too early an age also exercises a pernicious influence. Children are taken from school at the time when they ought to be commencing their studies in earnest. The medical profession ought to take the subject of education in the public schools into serious consideration. Vicious systems of education are the cause of many functional disorders, and often lay the foundation for the development of nervous disease. Another feature of our modern civilization is the want of vigorous development, and this is more fruitful of nervous affections than chewing tobacco or drinking coffee. Plants require the light of the sun in order to grow, and our children should not be denied its blessings. Many houses, of even luxurious construction, are kept in a state of dungeon-like darkness, and children reared in such houses are not nearly so vigorous as the children of a country clown, who have at least the benefits of air and sunshine.

Dr. BARTHOLOW, in reply, said:—I think that the statements of specialists should be taken with a great deal of allowance. They seem to base the opinion that nervous diseases are increasing, upon the fact that they see more cases than formerly; but that is only the result of their individual business increasing, as they become settled in practice. Doctors never see as many cases in their first years of practice as subsequently. It has been asserted that syphilis is a modern disease; that is doubtful. Many causes of syphilis existed formerly, and there is no proof that the disease did not exist too. My great answer to all the arguments which have been adduced is that men are larger, stronger, and better in every way than in former times. A modern Englishman cannot put on the old armor. The body is stronger and stouter than it was, and men are able to do more intellectual work. If the Geneva statistics are worth anything, they prove that the longevity of man has doubled within three centuries. As to the effect of the climate of America, it is found that longevity is greater here than in England.

CONTRIBUTION TO THE ETIOLOGY OF EPILEPSY.

BY

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The remarkable progress made in our knowledge of epilepsy commences with the classical researches of Brown-Séquard in 1850.[1] As the results obtained by him are well known to the profession, I shall merely mention here a few of the more important points. After the division of one half of the spinal cord, or of one sciatic nerve, in the guinea-pig, the animal becomes epileptic in the course of several weeks, exhibiting on the injured side the so-called epileptogenic zone. This zone is characterized by a lowered sensibility, and is situated below the eye, especially at the angle of the lower jaw, and extending toward the neck and scapula. If the section be made through the entire cord, or through both sciatic nerves, the epileptogenic zone appears on both sides, and if the pedunculus cerebri be injured, the epileptogenic zone appears on the opposite side. The epileptic attacks sometimes occur spontaneously, but they can be called forth at any moment by irritating, or even touching, the epileptogenic zone. The artificially produced epilepsy is transmitted by inheritance to the descendants of the animal on which the experiment has been performed.

These facts are of immense importance for the study of epilepsy in general, though as yet they do not admit of a direct application to human pathology, with the exception of the hereditary disposition, which, however, had been already previously established. It is easy to presume that epilepsy can be brought on by injury of a sensitive or of a mixed nerve, and therefore also by injury of the sciatic nerve, or of the spinal cord, but it is not in accordance with the daily experience of physicians that affections of the spine or of the sciatic should cause epilepsy, with an epileptogenic zone. Thus amputation of the thigh (section of the sciatic) is not followed by epilepsy, with an epileptogenic zone. Again, W. Müller[2] has published a case in which the injury of the cord in the human subject much resembled the experiments of Brown-Séquard on guinea-pigs. A man received a wound in the back from a knife that penetrated through the cord; the patient lived forty-three days, exhibiting all the symptoms of injury of the cord on one side, but having no convulsions whatever, though at the autopsy it was found that the entire left side of the cord had been perfectly divided.[3]

[1] Brown-Séquard, Researches on Epilepsy, etc., Boston, 1857; also Journal de la Physiologie des Hommes et des Animaux, 1858–1860; Archives de Physiologie normale et pathologique, 1868–1872.

[2] Wilhelm Müller, Beiträge zur pathologischen Anatomie und Physiologie des menschlichen Rückenmarkes. Leipzig, 1871.

[3] I omit giving here examples of epilepsy caused by injuries of sensitive nerves, and will only mention that it remains as yet undecided whether the epilepsy in these cases is caused by neuritis ascendens, or by purely functional centripetal excitations. Thus in the case described by Virchow, the epilepsy was caused by an injury of the median nerve (neuritis interstitialis prolifera), and in Echeverria's case by a lesion of the ulnar nerve (neuritis).

In 1871, Westphal,[1] in repeating the experiments of Brown-Séquard, whose conclusions he confirmed in all particulars, found that guinea-pigs could be made epileptic by producing a commotion of the brain. By knocking the animal's skull against a solid object, or by inflicting with a small hammer (such as that used in percussion) one or several blows upon its head, the animal is seized with general tonic and clonic convulsions, quite identical in character with the epileptic attacks exhibited by the animals treated by Brown-Séquard's method. If the commotion is very intense, the blow being too violent, the convulsions are generally followed by the death of the animal from paralysis of respiration, the action of the heart still continuing for several minutes. If, however, the blows are moderate, the animals entirely recover from the first attack, but after the lapse of several weeks exhibit the epileptic state and the epileptogenic zone on both sides, exactly like the animals artificially made epileptic by a bilateral section of the cord or of both sciatic nerves. In young animals an attack is called forth by a few slight percussions with the hammer, while in grown, vigorous animals a considerable amount of force is required to produce the same effect.

When the blows are very slight, no real epileptic attacks follow, but, instead of them, what may be called abortive attacks, consisting of peculiar reflex movements. If one of the epileptogenic zones be pinched in such an animal, it scratches the place with the corresponding extremity, and, closing the eyes, turns the head toward the same side. Animals exhibiting abortive attacks can easily be made epileptic by repeating the same operation. The epileptic state and the epileptogenic zones appear generally four or five weeks after the commotion, and may last six months or longer, after which they gradually disappear, though a slight blow on the head can easily bring them on again. Sometimes the attacks manifest themselves spontaneously, but they can always be called forth by mechanical irritation of the epileptogenic zone on each side. The epilepsy artificially produced by cerebral commotion is also transmitted by inheritance.

In order to ascertain the anatomical lesion which caused the epilepsy after these experiments, Westphal removed the skin and periosteum, and, inflicting the blows directly on the bones of the skull, obtained the same result, viz., an immediate attack, followed later by the epileptic state and the epileptogenic zones. This shows conclusively that the sensitive nerves of the skin or of the periosteum have nothing to do with these phenomena. Moreover, the autopsy made by Westphal immediately after the first attack following the blow, did not reveal any lesion either of the skull, or of the cerebral hemispheres, or of the cerebellum. He found only capillary hemorrhages in the gray and white substances of the medulla oblongata and of the upper cervical portion of the cord. Though the experiments of Westphal apparently have some similitude with traumatic injuries of the head, in man, yet he rejects any application of the results of his experimental researches to human pathology.

On the other hand, in Billroth's case there was no neuritis, though the epilepsy was induced by a traumatic cause affecting the sciatic nerve. In the same way wounds and cicatrices may induce epilepsy by the irritation of sensitive nerves. Thus in a case described by Schnee, the epilepsy was brought on by a wound of the head, which left a very sensitive cicatrix the slightest touching of which would call forth an epileptic attack. The epilepsy disappeared after the excision of the cicatrix, in which microscopical examination revealed the presence of neuritis.

[1] Westphal, Ueber künstliche Erzeugung von Epilepsie bei Meerschweinchen; Berl. klin. Wochensch., 1871, No. 38, 39.

He says: "I am not acquainted with a single observation of cerebral commotion (in the human subject), in which convulsions are mentioned among its symptoms." [1]

Hitzig[2] has also produced epileptiform convulsions, in dogs, by injuring the cerebral cortex. There is no doubt that epileptiform convulsions can be called forth in various ways, especially by injuring sensitive parts, among others therefore some central nervous structures. But the epileptiform convulsions thus produced are by no means identical with epilepsy, which, besides convulsions, requires the presence of the so-called epileptic state. In speaking therefore of real epilepsy, we must exclude such convulsions as are caused by the sequelæ of injuries of the cortex (hemorrhages, inflammation, softening, etc.), as in the experiments of Hitzig, or in the cases of Hughlings-Jackson.[3] These latter seem also to have depended on lesions of the cerebral cortex, whereas the genuine epilepsy may exist without any palpable textural changes of the brain.

The following case, lately observed by me, presents unusual interest, inasmuch as it resembles in many respects the experiments on animals, and has besides the great advantage of giving in a precise manner the subjective feelings of the patient. It is not only interesting as regards symptomatology and treatment, but especially so as throwing some light on the etiology of epilepsy.

Mr. H. W. K., lawyer, 24 years old, of a healthy family, without any hereditary neuropathic disposition, was formerly quite well, intelligent, industrious, and very regular in his habits. During a riot in Washington, in July, 1869, a negro struck him with a loaded cane on the head. The first terrible blow brought him senseless to the ground, where he received several additional blows on the head. He was carried to his house in an unconscious state, and, according to the opinion of the attending physicians, remained during three days and three nights in a comatose and hopeless condition. The first blow, which rendered him senseless, was struck on the right side of the forehead, in the direction from the tuber frontale toward the right crista frontalis externa and linea semicircularis. Fortunately the patient had long hair at that time, and wore a straw hat, which undoubtedly weakened somewhat the force of the blows. To the same circumstance must be ascribed the absence of any injury to the skull, on which no marks of contusion were noticeable. The greatest danger that threatened the patient's life was the exceedingly feeble, scarcely perceptible respiration, which at times seemed entirely extinguished. The action of the heart was also very weak (pulse 20 in a minute), and immediate death was undoubtedly prevented only by the strenuous efforts of Surgeon-General Barnes, who remained day and night at the patient's bedside, energetically using all possible means of excitation and derivation.

On the fourth day the consciousness gradually returned, though the patient still remained motionless, and therefore was still considered unconscious. He could, however, hear and understand everything, and felt the greatest fear of being buried alive, hearing the physicians express their opinion of his hopeless condition, and of the fatal termination which must necessarily soon take place. At the end of the fourth day, he felt in the extremities a pricking sensation (formication), which gradually extended all over the body, and slowly disappeared on the fifth day. At the same time he began to make feeble movements with the extremities, and soon was able to contract all other voluntary muscles; but he could not swallow or speak until the end of the first week. During the second week after the commotion, the patient experienced the

[1] Loc. cit. p. 462.

[2] Hitzig, Untersuchungen uber das Gehirn. Berlin, 1874, S. 270.

[3] Hughlings-Jackson, West Riding Lunatic Asylum Medical Reports, vol. iii., 1873.

greatest inclination to sleep, and could not awaken spontaneously. According to the advice of his physicians, he was awakened at times, and prevented from sleeping continuously. He was extremely sensitive to noises, easily felt exhausted, and after the slightest exertion would become unconscious, and remain so during ten minutes, exhibiting at the same time convulsive movements. He remained in bed during three months, having the epileptic attacks at first very often, and then once in two or three days. At last his health commenced to improve, he had a better appetite, and could exercise in the open air. But even in the street he was seized several times by an attack, with pallor, loss of consciousness, and epileptic convulsions.

The most distressing symptom the patient suffered from was an intense headache, which commenced with the return of consciousness after the commotion, and has scarcely ever left him during six years. The seat of the pain is the right side of the forehead and the right eye, the spot of the intensest pain corresponding to the right crista frontalis externa, where it passes into the linea semicircularis. The pain at times increases spontaneously, and becomes unbearable, continuing so for days and even weeks, with but short remissions, and finally disappearing almost entirely. At the height of the paroxysm, the right eye becomes absolutely blind (the patient not being able to count fingers), the slightest touch of the painful zone is unbearable, and pressure upon it calls forth loss of consciousness, and convulsions, considered as epileptic by physicians who have witnessed them. Occasionally such fits occur spontaneously, without any pressure upon the painful zone, and the patient sometimes feels the approach of the attack, and has time to lie down carefully, though more frequently it comes on suddenly, and the patient falls without having taken any precaution. All the time the patient suffers from the violent headache, there is a complete analgesia of the painful zone, and the whole forehead appears pale and cold.

When the pain is severest, the patient lies motionless, suffering from nausea and vomiting, and scarcely able to eat anything, as these symptoms are invariably increased by food. The patient passes sleepless nights through the whole duration of the paroxysm, only getting occasional short naps, from which he awakens with increased pain. At last, under the constant use of hot applications to the head, the pain gradually disappears and the patient slowly recovers. He then remains in a comparatively healthy condition for days, weeks, or even months, with good appetite and the power of sleeping, and with the vision of both eyes normal. Nevertheless the painful zone remains sensitive, even during the intervals between the paroxysms, and the slightest touch, and especially pressure, can bring back the paroxysm of pain with all its distressing symptoms, the fainting, and the epileptic convulsions. The patient has always to avoid touching the painful zone, even while washing his face or wearing his hat. He can hardly read, his head and eyes often feeling heavy; he has also to avoid the rays of the sun, and every physical and mental exertion, and as a consequence, has been obliged to give up every occupation. Of late the paroxysms of pain have become more frequent; his memory, that used to be excellent, has begun to fail; he cannot always control his ideas with the former precision, and at times they appear so confused that he fears the development of insanity. In the course of the last three years, though he has been treated by many able physicians, and has used among other remedies a great deal of bromide of potassium and narcotics, nevertheless there has been a progressive aggravation of the disease, especially as regards the intellectual sphere and the frequency of the paroxysms of spontaneous pain.

Present condition: May 29, 1876. The patient is of middle size, with a well-developed skull and chest, and moderately developed muscles and panniculus adiposus; the color of the skin and of the visible mucous membranes is rather pale; the thoracic and abdominal organs are normal; pulse 70; respiration 18; temperature 37° C. He is now suffering from a comparatively mild attack of spontaneous pain, and has scarcely slept or taken any food during several days.

The painful zone extends over the right half of the frontal bone, the anterior lower portion of the right parietal bone, the upper portions of the greater wing of the sphenoid bone, and of the squamous part of the temporal bone, the right eyelids and eye. The slightest touch of this region cannot be borne, and the patient involuntarily shrinks from the approach of the exploring finger. The centre of the intensest pain (spontaneous, as well as produced by pressure) is the upper portion of the crista frontalis externa, and the adjoining lower portion of the linea semicircularis, extending about four centimetres in length, and half a centimetre in width. The right tuber frontale is also more tender than the rest of the painful zone, but there is no particular tenderness over the foramen supraorbitale. The painful zone does not extend below the eye. The patient is unwilling, for the sake of an experiment, to have the slighest pressure made upon the painful zone, but assures me that it has been done many times by mistake or carelessness, and has always been immediately followed by the intensest pain, with pallor of the face, loss of consciousness, and epileptic convulsions. The skin of the forehead is cooler than the rest of the face, it is quite pale, and analgesic; a prick with a needle is not felt. The right eye looks smaller than the left, and is almost blind. (Only quantitative light can be ascertained.) The right pupil is somewhat larger than the left, and reacts sluggishly; the right retinal vessels are narrower than those of the left side. The patient speaks very little, and almost without contracting the facial muscles, or moving the eyes; it seems as if he preferred to turn the whole head, instead of contracting the mimical and eye muscles. His physiognomy therefore lacks expression, or rather expresses stupor, though it is evident that he keeps the facial muscles immovable in order not to increase the pain. The patient has passed a perfectly sleepless night, and complains of nausea, general prostration, and heaviness of the head and eyes.

As every kind of medication that had been resorted to had utterly failed to benefit the patient, I decided to make a careful trial with the galvanic current. I applied the cathode of a weak current (two elements of Siemens) to the nape of the neck, and wanted to apply the anode to the painful zone, but the patient immediately turned away his head, before the electrode had touched the skin. Leaving the cathode at the same place, I then applied the button-shaped anode to the right auriculo-maxillary fossa, and, without interrupting the current, gradually increased its intensity. When six Siemens's elements had been introduced into the circuit, the patient remarked that the pain in the forehead began to increase, whereupon I slowly diminished the intensity of the current, the whole sitting lasting a little over a minute. He felt somewhat better after the treatment, and slept that night until 3 A. M., when he awoke with headache and nausea, the right eye being perfectly blind.

May 30. I tried again the galvanic current, but this time avoiding, as much as possible, fluctuations of the current-intensity by means of a rheostat, intercalated as an accessory current. The current was derived from ten Siemens's elements, the flat cathode being applied to the nape of the neck, and the button-shaped anode to the right auriculo-maxillary fossa; the resistances were gradually augmented in the rheostat, until the full current-intensity had passed through the body during fifteen seconds, after which the current was again slowly diminished by means of the rheostat, and imperceptibly broken. The effect of this treatment was quite surprising. As soon as the current began to flow with full intensity, the patient exclaimed that the pain had entirely left him, the head felt quite free, and the eyes became clear. The sitting had lasted about two minutes. The patient was quite overcome by the sudden disappearance of all the morbid symptoms; his features became movable, and the blindness vanished. He felt quite well the whole day, had a good appetite, could walk a great deal, and slept six hours. Though he awakened without pain, still, the next day, the head and eyes were heavy, and he felt the premonitory symptoms of an approaching paroxysm.

May 31. I repeated the same treatment, and with the same result; the

patient feeling invigorated, as if, according to his own expression, awakening from a long and refreshing sleep. I found on examination (which I had omitted the previous day), after the treatment, the following facts: Slight touching of the painful zone, with the exception of the most sensitive part (crista frontalis, etc.), could be easily borne; there was no trace of anæsthesia or analgesia in this region, but, on the contrary, a prick with a needle was most painfully felt (hyperæsthesia). It was quite surprising to find complete anæsthesia and analgesia of the left (healthy) side of the forehead, extending exactly to the median line. Moreover, on this side differences of temperature could not be distinguished by the patient, and on applying a piece of lint dipped in hot water, and another in iced water, he felt no difference on the left, healthy side, and could easily bear them both; whereas, on the painful zone, he could instantly and with great precision discover the least difference of temperature, the extremes of which were felt most unpleasantly. I ascertained, besides, that the left frontal muscle was paralyzed, the skin of the left (healthy) side remaining smooth when he wrinkled the forehead; also, that the left upper lid could not be perfectly closed.

June 1. I re-examined the patient, and convinced myself again of the correctness of the above-mentioned facts. I repeated daily the above-described treatment, applying in addition the button-shaped anode also to the left fossa auriculo-maxillaris.

June 20. The patient slept ten and a half hours, last night, and the foregoing nights, six or seven hours, which for years he has been unable to do. He feels quite well and strong, and has no pain; the head is clear, the vision of both eyes normal, and he can read very well, has a good appetite, and is able to walk considerable distances. On examination I find both eyes equal in every respect, and the vision normal. The painful zone admits of a moderate pressure, and even the most sensitive spot can be touched, though only very gently. The sensibility and sense of temperature in the painful zone still remain exalted (hyperæsthesia), and there is no trace of analgesia. However, a stronger pressure cannot be borne, especially by the most tender portion of the zone, and the patient still wears his straw hat obliquely toward the left side of the forehead, thus leaving the right side uncovered. The left side of the forehead cannot be corrugated, and is still analgesic, though a slight touch with the finger can be felt; differences of temperature cannot yet be distinguished.

The galvanic treatment was continued daily as before. viz.: flat cathode at the nape of the neck, and button-shaped anode at first to the right, and then to the left, fossa auriculo-maxillaris. Current-intensity seven to ten Siemens's elements, increased and decreased cautiously, though fluctuations of one element could now be easily borne. Current-duration about one minute, or one minute and a half, for each side.

July 3. The painful zone can now bear a considerable pressure, and even the most sensitive portion can be touched. The left side of the forehead can be wrinkled, though less than the right side, and the left eyelid can be perfectly closed. The senses of touch and temperature on this side have considerably improved, but are not yet restored. The analgesic area has been progressively contracting; the patient at first could feel the prick of a needle only at the lower part of the median line, then at the arcus superciliaris, and now the region of the left temple alone remains analgesic. The patient complains of no pain; all his functions are normally discharged; and his general health is quite satisfactory.[1]

I shall not enter into an analysis of the above described symptoms, as I had not the opportunity of observing the patient at the time of the accident, nor immediately after. Although the intelligent patient described with great minuteness all his subjective feelings, and as much as

[1] After thirty-one sittings of galvanic treatment the patient suddenly and unexpectedly discontinued his attendance.

possible gave the opinions of the physicians who then attended him; still we entirely miss the result of an objective examination as regards sensibility and motility; the ophthalmoscopic appearances; the condition of the pupils; the presence of sugar, albumen, or blood in the urine, etc. The patient did not even know during six years that the left (healthy) side of the forehead was anæsthetic and analgesic, with loss of the sense of temperature and with impossibility of wrinkling it (paralysis of the frontal muscle). On the contrary, he considered the painful zone as being analgesic, which may have been the case during the paroxysms of spontaneous pain, though during the intervals I always found this zone hyperæsthetic.

There can be no doubt, however, that the injury produced a cerebral commotion of the gravest kind, which threatened to terminate with paralysis of respiration, exactly as in the experiments of Westphal, when the blow on the head was too violent. This termination was prevented in our case merely by the energetic and successful interference of the attending physician. Whether with the cerebral commotion some transitory affection of the brain had taken place, cannot be decided with certainty. Thus the vaso-motor phenomena might have been ascribed to a lesion on the surface of the cerebral cortex, in which Hitzig[1] and Eulenburg and Landois[2] have quite recently discovered the existence of vaso-motor apparatus. Again, the existence of pachymeningitis might have been assumed with still greater probability in consideration of the somnolence of the patient during the second week after the accident, and the diagnostic value which Griesinger[3] attaches to this symptom, especially when it is found in conjunction with stupor, contracted pupils, and headache. On the other hand, the sudden and almost complete disappearance of all the morbid symptoms after galvanization, and their periodical absence, make very plausible the assumption that no permanent or grave lesion of the brain had been caused by the accident. This supposition is supported by the researches of Fischer,[4] who has shown that a mere traumatic reflex paralysis of the cerebral bloodvessels is sufficient to produce the phenomena of commotion of the brain. From the history of the disease, we have every reason to presume in our case the existence of such a reflex paralysis of the cerebral bloodvessels and not a permanent structural lesion of the brain.

This case therefore elucidates a most important fact, namely, that *epilepsy can be produced in a perfectly healthy person* (having no hereditary neuropathic disposition), *by an injury of the head causing commotion without any structural lesion of the brain.*

Although the analogy of our case with the experiments of Westphal is very striking in many particulars, yet there are also apparently some differences. Thus we find a zone in our patient, the mechanical irritation of which calls forth at any moment loss of consciousness with epileptic convulsions, and though this zone is here hyperæsthetic, while it is anæsthetic in the animal, yet during the paroxysms of spontaneous pain it also appears anæsthetic and analgesic. We must bear in mind that we generally judge of the presence of anæsthesia in animals by the absence of expression of pain (screaming); but this was also the case

[1] Hitzig, Centralbl. für d. med. Wiss., 1876; No. 18.

[2] Eulenburg und Landois, Ueber thermische von den Grosshirnhemisphären ausgehende Einflüsse; Centralbl., 1876; No. 15.

[3] Griesinger, Archiv der Heilkunde, 1862.

[4] H. Fischer, Ueber die Commotio Cerebri; Samml. klin. Vorträge, Leipzig, 1871.

with the patient, who, though suffering intensely when the painful zone was touched, never manifested any sign of pain, but only became pale and lost consciousness. Possibly animals artificially made epileptic, suffer also from paroxysms of pain similar to those of our patient, but which have escaped our observation. Again, the blindness during the paroxysms of pain (probably a vaso-motor phenomenon) has not been noticed in the experiments on animals. Furthermore, the painful zone in our patient was above the eye, whereas in the animal the epileptogenic zone is situated below the eye. These differences, however, do not seem to be of great importance, and may depend on the incompleteness of the observations, on some unknown anatomical variations, or on differences in the localities to which the traumatic irritations are applied.

Of late, cases of epilepsy have been published, in which an epileptogenic zone has been observed, though in by far the great majority of cases this zone is not present. I treated a patient affected with epilepsy, in 1868, who in several respects resembled the one described in this paper, especially as regards the blindness and headache.[1] I regret not to have paid at that time the necessary attention to the existence of an epileptogenic zone. Subsequently I received additional information from this patient, who is now in perfect health, that in his case there was also great tenderness in the region of the temples, which could not be touched, and that the affection originated from an injury of the head. I may add that, in this case too, the disease, which did not yield to the different remedies employed, nor even to a fortnight's galvanization of the head, was successfully treated by a method similar to the one above described. It seems likely that the cases of epilepsy with an epileptogenic zone are of traumatic origin, and it is therefore advisable in every case of epilepsy to look carefully for an epileptogenic zone. Of course, in the human subject the place of this zone may not always correspond to that observed in animals. It is also probable that in the course of time the epileptogenic zone becomes reduced, and finally disappears, thus escaping further observation.

We are now led to inquire whether, in the majority of youthful epileptics, the disease has not been caused by frequent injuries (falling upon the head), in childhood, especially in individuals with a hereditary neuropathic disposition. From this point of view it would seem surprising that comparatively so few are affected with epilepsy and petit mal. The experiments on animals teach us, however, that not every blow upon the head even in the guinea-pig—this neuropathic animal *par excellence*—is liable to produce epilepsy. For the same reason, in the child, the favorable conditions for producing epilepsy are comparatively rare: it would possibly be requisite to have the head fixed to a solid immovable object, striking it repeatedly with a certain force, and perhaps even at a certain region. If we admit the traumatic origin of epilepsy, we are then enabled to explain satisfactorily many phenomena which accompany this disease, as for instance the prevalence of epilepsy in youth, the hereditary transmission of the disease, the negative results obtained from the pathologico-anatomical investigation of the brain (at least the absence of structural lesions of the brain), the vaso-motor and other phenomena which epilepsy has in common with cerebral commotion, etc. Lastly, I should like to direct the attention of the profession to the above-

[1] Neftel, Galvano-Therapeutics. New York, Appleton, 1871, p. 113.

described method of treatment, which may possibly be also found useful in other cases of epilepsy.

To conclude, I shall sum up what has gone before in two propositions:—

I. It is made evident by the case described, that in a perfectly healthy person, free from any hereditary disposition, epilepsy can be brought on by traumatic influences upon the head, causing cerebral commotion without any structural lesion of the brain.

II. Further investigations will be needed to prove that such traumatic influences during childhood may constitute a most frequent etiological factor in the production of epilepsy.

THE INFLUENCE OF HIGH ALTITUDES ON THE PROGRESS OF PHTHISIS.

BY

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It is assumed that this occasion calls for something suggestive, leading an inquiring mind to look at an important subject in a new light, and seeing the errors of former beliefs through a change in the channels of thought. How desirable then, if we are not now perfect (and I believe the hopeful mind of to-day has more to expect from what we are to learn, than from what we now know), that a controversy of opposing views should here be stimulated in friendly discussion. Believing that our profession as a body is proving itself an insufficient barrier to "the progress of Phthisis," and having convictions which persuade me to differ from many of the authors[1] who have written on the subject of climate and consumption (and I have endeavored to consult them all), I may be excused for seeking to introduce this discussion by occasionally emphasizing these differences of opinion.

The subject assigned me is one of the most interesting and important that can claim our consideration, and all the more so because it has not hitherto received any special attention from our profession at large. As to the disease itself—this great and persistent destroyer of mankind—valuable results have of late accrued from the constant inquiry and pathological research of eminent medical men. Thus this disease has come, through the critical analysis of a transitionary stage in professional opinion, to include, under the one name of Phthisis, various and widely differing abnormal conditions.

Climate and Consumption.

We can define Pulmonary Phthisis as *slow death commencing in the lungs*, or, going back of the local affection to the dyscrasia or abnormal blood state which precedes or accompanies its progress, we can enlighten ourselves as to the various pulmonary and other defects which have till of late been considered as one disease. However, the scope of this report, and lack of time, make the minute consideration of this subject an unwarrantable digression, so that we will content ourselves with the generally accepted significance of the term Phthisis in its broadest sense, including those morbid conditions (chiefly inflammatory[2]) which favor

[1] Dr. Hermann Weber and Dr. Archibald Smith, late of Lima and elevated resorts in the Peruvian Andes, are notable exceptions.

[2] Dr. Hermann Weber, in the Medico-Chirurgical Transactions, vol. lii. (Treatment of Phthisis by Prolonged Residence in Elevated Regions), gives the credit of first promulgating these views to the late Dr. Thomas Addison. "whose teaching, that inflammation constitutes the great instrument of destruction in every form of phthisis, was for a long time either ignored, or regarded as erroneous, by most of his contemporaries." (On the Pa-

weakening or a breaking down of pulmonary tissue; and refer to special phases of the disease as the illustration of our thought may seem to demand.

In considering the climatic treatment of Phthisis, it is not essential to dwell upon the first experiences of our medical fathers in this matter. The conclusion of Dr. Walshe, that "climate takes its occasional share in bringing about the nearest approximations to real cure therapeutically obtainable,"[1] has at length come to be very generally held by medical men. Even before the disease was well understood, while the failure of medicine to cure or check it was slowly being realized, the considerable benefit derived from even mild changes of climate must have strongly suggested a climatic cure. So if the remedy of high altitudes had not long been utilized, as we now learn it has been in the Peruvian Andes, the first two cases mentioned by Dr. Hermann Weber, in his excellent paper "On the Treatment of Phthisis by Prolonged Residence in Elevated Regions,"[2] would have suggested this most potent means of cure to every reflective mind. These cases occurred in natives of elevated localities, who had become residents of London. Prostrated by acute phthisis, they returned to spend the remainder of their probably short lives in their early homes, the one to the Black Forest, in Germany, at an elevation of 2800 feet, and the other to Switzerland, 5000 feet above the sea-level, and in both the disease was quickly arrested. The evidences of the therapeutic value of high altitudes are, however, comparatively recent.

Thus it is not strange that, having no better experience to trust to, the medical profession has hitherto placed confidence in the scattered successes furnished by Mediterranean, low continental, maritime and English health resorts. In England, and on the continent, the description and classification of favored health resorts have been carried to an exactness which has hardly been reached elsewhere.[3] But it is a question, in my mind, if these analyses are not sometimes made to the exclusion of important data, notably the moisture of the air, and with undue importance given to less vital points, such as the precise degree of temperature and the daily variation. Then again the author so often feels it incumbent on himself to tell you how to get away from a place, if you are not suited, that often no relief would seem to be expected; while the poor invalid, usually in some doubt, ekes out a six months' health trip at as many separate resorts. This guessing at the answer to the invalid's conundrum, "where can I best find relief," is far from the certainty and confidence in the course to be pursued, which should characterize the climatic treatment of phthisis. There may be some physicians who are so specially gifted in the adaptation of these low climates to the consumptive's needs as to gain very fair apparent success. After studying the descriptions of numerous low, inland, and seaside Mediterranean health resorts, with all the phases and symptoms of phthisis so nicely met by the appropriate

thology of Phthisis, by T. Addison, M.D., Guy's Hospital Reports. 2d Series, No. V., 1845.) The same has recently been emphasized in the works of Dr. Felix Von Niemeyer, and among others by Dr. Ludwig Buhl (Inflammation of the Lungs, Tuberculosis, and Consumption, New York, 1874).

[1] Diseases of the Lungs. W. H. Walshe, M.D. London, 4th edition, page 585.

[2] Loc. cit.

[3] Climate, in Williams's Pulmonary Consumption; Lettsomian Lectures, on the influence of climate in the treatment of pulmonary consumption, by C. T. Williams, M.D.; Consumption, its early and remedial stages, by Edward Smith, M.D.; Diseases of the Lungs, by Dr. Walshe, 4th edition; The effects of climate on Tuberculous disease, by Edwin Lee, M.R.C.S., one of the Fiske Fund Prize Essays.

climatic remedies, I must confess to a slight feeling of hesitation. It is well for us to ask ourselves how certain we are of any permanent relief being obtained by consumptives in low humid climates. Though there is no lack in the number of "watering places," the field is so small (with elevation left out) that one might almost as well think of supplying all the climatic needs of consumptives within the boundaries of the little State of Massachusetts. If in these low and narrow limits favorable qualities are found, then efficiency in the climatic treatment of phthisis demands more decided proportions of these same qualities, which we hope to show are to be found, together with additional advantages, at suitable elevations.[1]

Low Climates.—As I am going to speak positively in favor of the utility of high altitudes, where not specially contra-indicated, and shall ask you to concur with me in this favorable opinion, it is well to make mention of the grounds I have for feeling less confidence in low climates. As will be apparent when we come to discuss the humidity of the air, we are deprived of important data with regard to the undesirableness of low climates, in that we have few positive records of the relative or absolute humidity of these places in Europe. They all, however, represent more or less moist climates. Of climates in England, Dr. C. T. Williams, in his very carefully prepared Lettsomian Lectures, gives valuable statistics of several localities, mainly along the southern shore of the Island, and remarks upon the favorable results of 386 winters spent at those places by 243 consumptives. "Bear in mind," he says, "that these general results assign the largest amount of benefit to the most easterly stations, and follow a course exactly the reverse to that of the warmth of the localities; Hastings, the coldest, being at the top of the list, and Torquay, the warmest, being at the bottom," Ventnor and Bournemouth being between these. This result will be claimed as an argument for high altitudes, when we come to consider that a cooler temperature, as well as decreasing humidity, are found to be the natural attributes of great elevations.

On the continent, there are also warm, moist climates, called *sedative*, and more exhilarating resorts, termed *stimulating*,[2] which, like Hastings in England, begin to partake of the best qualities of high altitudes, and to show, according to Dr. C. T. Williams's excellent tables, a proportionate improvement for the winters there spent by consumptives. Of the former group there is Pau, remarkable for the *stillness* of its climate, where, as we are informed, a plentiful amount of rain "falls perpendicularly."[3]

[1] In Europe, a reason has seemed to exist for sending south to the warm shores of Italy and to the south of France those consumptives who, from feeble stomachs, are unable to digest the quantity of food necessary to supply the waste caused by the greater amount of oxygen inspired in the colder regions of the north. In the warm, moist climate, less carbonic acid is given off, and less heat required, so that the feeble digestion is not so heavily taxed as in the severe climate of England. The plan works well, chiefly as a palliative measure; but as warm weather comes on it is reasonable to believe that the scales must be turned, and that the indigestion and enervation felt at the South may be much relieved by a return to the cool climate of the North. So the consumptives who have shunned a harsh New England spring, by a sojourn in Florida, hasten back, as warm weather approaches, to cool seaside and country resorts, and are kept constantly on the move, with considerable indifferent benefit. This idea of allowing the digestive capacity to decide to what climate a consumptive should go, may often seem to result in good, but it is founded on uncertain premises, and lacks much desired definiteness in results. Like much of the theorizing as to the treatment of phthisis indulged in by authors for many years, it is not radical enough to answer our needs in the war we have to wage against one of the greatest of human destroyers.

[2] Walshe, op. cit.

[3] C. T. Williams, op. cit.

There is Valencia, in Spain, which Dr. Walshe characterizes as "probably one of the warmest, most humid and depressing spots in continental Europe;" Pisa, a once populous city in Italy, of which the same author says that it has "completely lost its *quondam* prestige as a sanitarium," though, as we fail to notice positive climatic qualities to greatly recommend it, we are surprised with the further assertion that this "changed opinion is the result of sheer medical caprice;" and Venice and Rome, the latter of which Dr. Williams[1] represents to have "had formerly a great repute, though for many years its unfitness as a shelter for invalids has been recognized, and it is now mainly resorted to for pleasure, and not for health." Among the latter group ("stimulating"), we find Hyères, Cannes, San Remo, and Nice (representing the Riviera), Barcelona, Gibraltar, Malaga, Lisbon, and the Mediterranean islands (as Sicily, Sardinia, Malta, and Majorca), which, as before remarked, partake of more favorable qualities, and are notably less moist, than the "sedative" group, or which have some peculiar attributes; thus Dr. Edwin Lee says of Naples, "The frequent variations from dryness to moisture, and from warmth to coldness, promote the disengagement of electricity."[2]

There are marine climates which, being wholly surrounded by salt water, I fancy confer some of the advantages of sea voyages, in that the air is constantly in motion, and the insensible activity of the skin thereby promoted, while the air is always pure and bracing, considering the temperature. So that these may be considered better than the moist climates on the continent of Europe; and yet Dr. Walshe says of Madeira, "much of this promising character turns out to be practically delusive. The damp is so extreme that changes of temperature thermometrically small —of three or four degrees—prove sensationally great; a temperature of 58° indoors, without being actually cold, is disagreeably chilly," etc.[3] The Canary Isles, St. Helena, some of the Mediterranean islands, the Bermudas, Bahamas, West Indies, etc., may also be considered in this group. There are *relatively* very dry climates, as Egypt and Syria, whose enervating quality is the high temperature which would hardly be bearable, were the dryness *absolute*.[4]

Of American climates, we have the characteristic low resorts of moist and sedative Florida; the balsamic regions[5] among the pines in southern Georgia; the dry and alkaline regions of southwestern Texas, in the vicinity of San Antonio; and the exceedingly equable and mild Pacific coast, San Diego, Los Angeles, Santa Barbara, San Luis Obispo, San José, and San Rafael, in California. Because of the great humidity of the atmosphere, these last are much like the warm moist climates in the Mediterranean basin. The favored low resorts in the United States might be much better chosen climatically speaking, but then the attractions have thus far been where the comforts of life are to be found.

[1] Op. cit.

[2] Op. cit.

[3] A sea captain of long experience, who now lives in Denver, for the relief of asthma, tells me he has carried many consumptives to the Sandwich Islands, "*but brought none of them away.*"

[4] In America, in Southwest Arizona, and in the inland portion of Southern California, notably in the vicinities of the Death Valley and Fort Yuma, the winter months only are comfortable, and sometimes hardly that, because of the intense radiation from the sandy plains. At Fort Yuma, Dr. Geo. S. Rose, of the U. S. Army, has informed me that the mean annual rainfall has been for nineteen years only 2.95 inches, while the average annual temperature has been remarkably high—about 75°.

[5] The Pine Forests of Georgia as a Resort for Invalids; by Ezra R. Pulling, M.D. Reprint from N. Y. Med. Journ., Sept. 1871.

Medium Altitudes.—America is blessed with advantageous resorts for cases in which high altitude is not well borne, or rather not permissible. Notably is this true of the *medium altitudes*, though the need of them has yet to be sensibly felt by physicians who guide invalids, before desirable comforts and accommodations will be assured. Among these should be mentioned the Cumberland Table Land, in the mountains of eastern Tennessee, more especially "Walden's Ridge" (elevation about 2000 feet), lately described by the facile pen of Dr. E. M. Wight, of Chattanooga;[1] western North Carolina (Ashville, elevation 2250 feet);[2] southwestern Texas (Fredericksburg, Bœrne, Uralde, and Ft. Clarke, elevation 1500 to 2000 feet, valley of Rio Grande); southern New Mexico (the Mesilla Valley) and western Kansas (elevation 3000 to 4000 feet), the former in winter and the latter in summer being comparatively warm enough to be placed in this group. So also northern Minnesota (St. Paul and Brainard, elevation 1200 feet), because of its cooler temperature and northern situation. In Europe, examples of medium altitudes might be mentioned, as Silesia (Görbersdorf, 1700 feet), Le Mont Dore (3400 feet), Königswarth in Bohemia (2200 feet), Black Forest (2800 feet), Lower Engadine (about 4000 feet), Eaux Bonnes (2450 feet), Cauterets (3254 feet), and Bagnères de Luchon (2060 feet). In Brazil, Dr. Walshe desires to call attention to San Paulo (2300 feet), a short distance inland from the port of Santos. In Mexico, we have Jalapa (4000 feet).[3]

High Altitudes.—Coming now to high altitudes proper, a few places will be mentioned, chiefly those which will serve for our present study, lying in the region of an *altitude of approximate immunity from phthisis*, which we will consider further on. In Europe we have to content ourselves with more unfavorable complications than obtain in the chosen high altitudes of America. The mountains of the Alps present, in the upper Engadine, such resorts as St. Moritz (6100 feet), Samaden (5500 feet), and Davos (5200 feet). They are high enough for their northern latitude, but are burdened with an excess of moisture gathered by the mountains, exposed as they are to the moist Atlantic currents. This is evidenced by Dr. Walshe's citation from Dr. Hewlett,[4] that, at St. Moritz, "in fourteen winter months (seven in 1866–7, and seven in 1867–8) there were only fifteen clear days, while 148 rainy days, and others more than half clear, with their admixture of snow, sleet, and rain, made up the rest."[5] This is considerably at variance with the excellent reputation Dr. L. Vacher gives Davos, for the five cold months ending with March, during which time only the stay of consumptives at this resort is recommended.[6] It is here that the plan, personally investigated by Dr. Vacher (so novel to us who live at the base of the Rocky Mountains, though partly exemplified by the invalid's experiences in Minnesota), is presented. It consists in keeping invalids in the region of continuous snow, until the thawing season in the spring, when they are quickly removed to a lower level. Dr. Vacher gives the number of invalids seeking this resort

[1] A People without Consumption, and some Account of their Country, etc. Reprint, Trans. Tenn. Med. Society.

[2] Western North Carolina as a Health Resort; by W. Gleitsmann, M.D. The Climatotherapy of Consumption; by S. E. Chaillé, A.M., M.D.; from N. O. Med. and Surg. Journal, May, 1876.

[3] Influence of Climate in North and South America; by J. Disturnel.

[4] Walshe, op. cit.

[5] St. Moritz; by R. W. Hewlett, M.D. 1871.

[6] Le Mont Dore—Davos. Étude médicale et climatologique sur les cures d'air dans la Phthisie Pulmonaire; par Le Dr. L. Vacher. Paris, 1875.

as eight, in 1865; 55, in 1870; 220, in 1873; and 400, in 1874; which ratio serves to show the increasing favor which the results of the treatment have warranted, as well as the comparative newness to Europeans of the special efficacy of high altitudes. I shall have many occasions to refer to the experiences furnished by Davos. In Asia, the elevated plateaux in Persia and Armenia, and those adjoining the lofty Himalaya Mountains, together with other elevated inland and dry localities, would seem to furnish excellent climatic combinations, but I have not the necessary data for an account of that vast section.

In America, North and South, the great Rocky Mountain and Andes chains furnish, along and between their variously diverging branches, almost innumerable resorts of any desired elevation or latitude. From the north to the south, in the United States, the great plateaux and elevated basins between the Rocky and the Cascade Range, the Sierra Nevada and the Sierra Madre Mountains, present variations in altitude, temperature, and humidity, which would claim our attention could we now spare the time necessary to study so broad a field. Generally having favorable climatic attributes, with unfavorable distances and local peculiarities, we pass over this vast section to mention the western slope of the Sierra Nevada, in California (vicinity of lakes Donner and Tahoe, in summer), and especially the eastern slope of the Rocky Mountains, in Colorado and New Mexico. (Cheyenne, in Wyoming Territory, 6100 feet; Boulder, Colorado, 5300; Denver, 5200; Idaho Springs, 7540; Colorado Springs, 5980; Manitou, 6200; Pueblo, 4630; Canon City, 5260; Trinidad, 5800. In New Mexico, Santa Fé, 6850; and Albuquerque, 5030.)

It is in Colorado that the Rocky Mountains proper reach their greatest continuous height, many peaks being over 14,000 feet, and present a remarkable width of 100 miles. Interspersed with these lofty peaks are elevated plateaux, or valleys, as North, Middle, and South Parks, 8000 to 10,000 feet; Fairplay, 9800, and San Luis Park (Del Norte), 7000 feet. In Mexico, this vast plateau is continued, being about 1000 miles wide at the north, and nearly 2000 in length, and is called the *Anahuac*. It varies in elevation from 8000 feet on the east, to 4000 feet in some portions of the western, or Pacific, slope (the City of Mexico, 7400; Chihuahua, 4640), and includes most of the *Tierras Frias*, or cool regions. These exceedingly dry, elevated plains, including extensions to the north into New Mexico and Arizona, would claim much more of our interest and study were the habits and lives of their greasy original inhabitants more inviting and congenial to the cultivated American. The loose sand and fine lime dust, together with the rapidly increasing growth of the prickly and inhospitable cactus, as you go south and southwest from central Colorado, may reasonably indicate the undesirableness of much of that southern region.[1] As exceptions to this uninviting region, I note the beautiful and favorably-located southern slope of the mountains, toward the San Juan river, in southwestern Colorado, where lie the Pagosa springs (most wonderful of the hot springs in America); a genial climate and interesting volcanic region near Fort Bayard

[1] Where the cactus grows to the size of trees, as in some parts of New Mexico and Arizona, Lieut. Philip Reid, of the United States army, informs me, are the most uninviting places imaginable for the steady abode of man. Yet here, on the advent of railroads, I predict will be found the extreme climatic remedy which will suit many in winter who are now without much hope anywhere. Such is the fight the invalid will have to make with his destroyer.

(elevation, 6000 ft.); the foot-hills and possible favored resorts in sunny uplands of Chihuahua; and the inviting surroundings of the antique City of Mexico, almost encircled by the lofty Sierras, which inclose beautiful lakes and favored resorts, and send, among other peaks, the Popocatepetl 17,735 feet above the sea, far into the regions of perpetual snow.

The *Cordillera de los Andes*, which has divided in Guatemala, to form the eastern and western boundary of the *Anahuac* in Mexico, extends into South America, to rise still higher just north and south of the equator. Here the Cordilleras, lying close to the Pacific coast, diverge in ranges which inclose elevated valleys or plateaux from less than 8000 to over 12,000 feet in height, while snow-clad peaks like Chimborazo in Ecuador (21,500 feet), and the Sorata and Illimani, in Bolivia (22,400 and 21,250 feet), tower above.[1] In the central portion of Peru the *Montana* consists of a lofty plateau, averaging 12,000 feet in height, which contains many cities and villages, possessing, according to Disturnel, "a fertile soil, amid the most magnificent scenery on the earth's surface," and "enjoying a temperate and delightful climate." The considerable moisture in the air, for such great elevations, as contrasting with the excessively dry regions on and to the north of the Anahuac, may yet indicate valuable discriminations for the thousands who will seek a restoration of their consuming strength in high altitudes.

Analysis of the Attributes of High Altitude Climates.

We have now hurriedly, and somewhat superficially, reviewed the characteristics of low climates, and have geographically indicated medium and high altitudes. In claiming our present consideration, high altitudes have *positive* features and effects to contrast with the *uncertainties* of low levels. Therefore it is to be hoped that the considerable space given to low climates may not be thought amiss: so much does our appreciation of good, as in fact the enjoyment of life, come to us by contrast. I have gladly accepted the invitation to consider high altitudes in general, rather than any one locality, though I fear that the dry study of so large a field will tax your patience, and be otherwise unsatisfactory. Let us then consider high altitudes in the abstract, and seek to analyze the attributes of its atmosphere, leaving, for the present, geographical and local considerations mainly in the background.

We may form in our minds an idea of the climates which will best answer the consumptive's needs. Not that we can fathom the Creator's plans, and improve on the world he has given us, but that having satisfied ourselves of the *ideal*, for the majority of phthisical patients, we may then seek the *real*, among the varied climates of the earth. Here it is instructive to remark how various are the visions indulged in by unsatisfied man, of the most beautiful and desirable country in which to live. It is of consequence also to note that it is improbable that we physicians would agree, were we to vote now as to the average tempera-

[1] When it is considered that, under the equator, the growth of wheat is not established till the altitude of 4500 feet is reached, and that sugar cane has been grown at 7500 and the banana at 5400 feet, it is not to be wondered at that the most comfortable existence for man is to be found at a considerable elevation, and that populous cities lie high up in the mountains, as Bogota, the capital of the United States of Colombia, 8650 feet; Quito, of Ecuador, 9540 feet; Cuzco, the ancient capital of Peru, 11,380 feet; and towns bordering on lake Titicaca, between Bolivia and Peru, 13,000 feet.

ture, humidity, amount of sunshine, electric tension, elevation, and characteristics of wind, as to direction and velocity, etc., of our ideal climate, for the majority of consumptives, or even for uncomplicated phases of the disease. The attributes of climate just mentioned are so intimately interwoven and dependent on each other, that their separate consideration is difficult. We will try, however, to avoid repetition by referring to them in the order named.

Temperature.—The temperature of high altitudes makes life pleasant under the equator, and gives marked variety to climates north and south of that line. In general, the decrease in the temperature of the air has been reckoned at 1° Fahr. for each 300 feet of elevation.[1] But the rule for the decrease of temperature cannot be arbitrary for all elevations, because of local peculiarities. For instance, the mean annual temperatures of places along the eastern base of the Rocky Mountains in Colorado, are much higher than their elevations would indicate, judging from the records of stations in the same latitudes on the Atlantic coast of the United States. The cause of this is (1) *The far inland position.* "The sun's rays passing through the air with but trifling loss, fall on land or on water. The specific heat of land being only one quarter that of water, it both absorbs heat and gives it out more rapidly . . . consequently the more land the greater is the heat, and the wider the diurnal and yearly amplitudes of fluctuation."[2] (2) *The dry sandy soil.* The plains are generally but lightly covered with grass (rivers and trees being very limited), and this tends to increase the difference between ordinary clay soil and water, and so furnishes both greater absorption and greater radiation of heat than the ordinary soil of lowlands. (3) *The protection afforded by the mountains*, to the west and northwest. These break or divert strong winds, and gather moisture from western currents, thus promoting an increase of sunshine on the plains. (4) *The dry and warm Equatorial and Pacific winds.* These prevail at all exposed points on the mountains, and bring pleasant weather to the plains below.

In considering the variations of temperature referred to above, we remark that the more rapid absorption and radiation of heat by land than by water, explains the low fall of temperature at night, and the considerable rise at midday, away from the sea-coast, and shows, by inference, the *equalizing* influence on temperature of moisture in the air. These variations are increased in high altitudes by the lessened capacity of the air to hold moisture, due to elevation and the resulting loss in weight. The annual fluctuations, due to the changed angles with which the sun's rays fall on the earth, are for the same reason affected by elevation. Thus the following table is explained, giving the mean daily range, the average monthly range, the range of monthly means, the annual means, and the annual range of temperatures of places chosen as nearly as possible in the same latitudes, on the sea-coast, inland, and at the base of the Rocky Mountains.

[1] Under the equator it has been estimated to be 1° Fahr., near sea level, for each 333 ft.; for each 297 feet at 10,000 feet, and for each 318 feet at 20,000 feet elevation; while recent observations, in temperate climates, have given the rule of 1° Fahr. for every 400 feet. The records of Mr. Glaisher's balloon ascension indicated different strata of air, the fall in temperature being under a clear sky 5° Fahr. for each of the first four inches of barometric fall, then 4° per inch till the thirteenth inch of descent, and 4.5° each for the last three inches.

[2] Parkes, Practical Hygiene, p. 417. 4th ed. Phila., 1873.

Table I.—*Showing Ranges of Temperature from July*, 1874, *to July*, 1875.[1]

	Mean daily range.	Mean monthly range.	Range of monthly means.	Annual mean.	Annual range.
Atlantic Stations.					
New Haven, Ct.	16°	42°	48°	48.6°	91°
Atlantic City, N. J.	15	41+	44.1	49.7	89½
Baltimore, Md.	18	44	48	53.9	98½
Norfolk, Va.	18	44	40.7	57.3	89½
Average	17	43	45.2	52.4	92½
Inland Stations.					
Davenport, Iowa	18	53.5	68.6	46.8	120
Indianapolis, Ind.	22+	57	58.5	51.7	127½
St. Louis, Mo.	20	53	57.4	54.2	117
Nashville, Tenn.	21	49	49.7	59.5	108
Average	20+	53.12+	58.55	53.05	118⅛
Elevated Stations.					
Cheyenne, Wyo.	31	61.5	48.9	43.6	136
Denver, Col.	30	60.5	53.7	49.2	131
Colorado Springs, Col.	30	63.5	47.7	46.8	123
Santa Fe, N. M.	29	49.5	45.3	49.2	88
Average	30	58.75	48.9	47.2	119½
Pacific Coast Stations.					
San Francisco, Cal.	12+	31	11.5	55.5	50
San Diego, Cal.	13	31	5.6	59.9	60
Average	12½	31	8.55	57.7	55

Besides showing the effect of inland position and elevation on the daily, monthly, and annual ranges of temperature, this table makes evident certain facts of interest both in reference to the stations separately, and the groups into which they are divided. (1) The fluctuations in temperature (notably, of course, in the annual range) decrease toward the South. This continues till even greatly elevated localities, near the equator, are quite equable in temperature, as on the elevated plains in the Peruvian Andes. (2) The comparatively low range of monthly means, at the "elevated stations," considering the high mean daily, monthly, and annual ranges (with the low average relative humidity of this section, about .45), indicates that there must have been a large proportion of pleasant, even weather at those stations in each month, to have made up for those other extremes. This preponderance of pleasant, sunny weather is quite uniform at the base of the mountains. (3) The extremely low mean range of temperature for all divisions of time, on the Pacific coast, indicates the existence there of a continuous, warm, moist wind, which is unobstructed, and only gives up its moisture when cooled by the winter temperature of that latitude.[2]

The influence of heat on the activity of several functions of the body leads us to prefer a cool to a warm temperature. (1) Heat is opposed to stimulation, so far as the nervous system is concerned. "In experiments

[1] Report of the Chief Signal Officer, War Dept., 1875.

[2] It is unobstructed, for otherwise there would be greater precipitation of moisture, and accompanying thermal changes. That it only rains in winter, is a fact known to all. It is a continuous wind, for otherwise terrestrial radiation and absorption would have produced a much more marked effect on the temperature than the records show. It may be considered warm, because a cold wind could not carry moisture enough to have produced such thermal effects in these latitudes. It is moist, because if it was dry the daily and seasonal influences of the sun would be greater than the records show. We are not surprised then to learn that the average atmospheric humidity is out of proportion to the amount of rainfall on the Pacific coast.

on frogs, when a temperature much over the natural amount is applied to nerves, the electrical currents through them are lessened, and at last stop.[1] E. H. Weber's observations[2] show that for men the same rule holds good, the most favorable temperature being 99.5° Fahr."[3] Then a contrast between the temperature of the surrounding air and that of the body favors the influence of atmospheric electricity, which experience proves to be an active agent at considerable elevations. (2) Heat in the shade has been shown by Rattray[4] and others to have a sensible effect in increasing the temperature of the body, to counteract which a healthy activity of the skin is required, but is impeded by a coincident high relative humidity of the air. The experiment of entering a heated oven (200° to 300°) becomes more difficult when the air is moist than when it is dry; evaporation is hindered, and the bodily temperature is said to rise even 7° or 8°.[5] (3) Heat lessens the number of respirations per minute. According to Rattray's observations, the variation has been known to be from 16.5 respirations per minute in England, in winter, to 12.74 and 13.74 in the tropics. Other observers note that the breathing is also less deep. Dr. Francis (Bengal Army) and Dr. E. A. Parkes[6] have both observed that the lungs are lighter after death in Europeans in India, than the European standard. On the other hand, a healthful activity of the respiratory function is favored by a cold temperature, and is an important feature of the high altitude cure. (4) Heat, especially if continuous, lessens the digestive powers. The stimulation of the appetite which cold is well known to produce, is lacking, especially for much-needed animal food. So the destruction and formation of tissue is retarded. This is also opposed to that renovation of system which is decidedly promoted by both cold and elevation.

Humidity.—To continue our proof that *cool, dry* climates are better constituted for the needs of phthisical patients than those which are *warm* and *moist*, we will consider the humidity of the air, bearing in mind that coolness and dryness are constantly favored by increasing elevation. This brings us to one of the strongest arguments in favor of high altitudes. It is, however, difficult to thoroughly explain this subject, because of our lack of familiarity with the precise laws which govern the various changes of the moisture in the air.

The Signal Service Bureau of the United States Army determines the *relative humidity* of the air by the difference in temperatures given by the wet and dry bulb thermometers, and thus the percentage of saturation of the air is known. But these humidity records have not been well distributed, and it would seem either that the officers of this bureau have little confidence in the reliability of humidity statistics, or that they do not appreciate their important bearings on scientific and climatological investigation. While the *relative* amount of atmospheric moisture is most important, as determining the healthful activity of the skin, the evaporation and quickening of perspiration from the surface of the

[1] Eckhard, Henle's Zeitschr., Band x., S. 165, 1851.

[2] Weber, Ludwig's Phys., 2d ed., vol. i. p. 126.

[3] Parkes's Practical Hygiene, 4th ed., p. 400.

[4] On the Effects of Change of Climate on the Human Economy, by A. Rattray, M.D., Surgeon R. N. Proceedings of the Royal Society, 1869–1872.

[5] By inference we find here one cause of sunstroke, which in our experience does not occur at great elevations in America, notwithstanding that Dr. Vacher (op. cit.) speaks of sunstroke occurring on the Alps.

[6] On Algide Cholera, by E. A. Parkes, M.D., p. 14 (1847).

body being greatly in proportion to the removal of the dew-point and temperature of the air from that of the body, yet these records are in a measure superficial, simply because they are *relative* and not *real*. This, too, notwithstanding that a little more than half the ratio of saturation is shown by them on the plains east of the Rocky Mountains, to that which exists in some localities on the sea-coast. This is proof enough that our temperature is not much in the suspicious company of the dew-point. I am, however, inclined to claim that the importance of a large amount of atmospheric vapor, as a causative, and of a small amount, as a curative, element of phthisis, is such that the *absolute* rather than the *relative* amount of moisture in the air needs our special study. If to-day we were possessed of the knowledge of the average amount of vapor in a cubic foot of air in all parts of the country, I believe we would have one, if not the most valuable indication possible, of the best localities for phthisical patients. With some reservation as to temperature, the smallest ratios would indicate where consumption originates with the greatest difficulty, where a low ratio of relative humidity, a small annual rain-fall (excepting sudden and great precipitation), a large proportion of sunshine, and a favorable diathermancy of the air would be assured, so much do these favorable conditions accompany real dryness of the air. This absolute humidity would of course be shown to be greatest in the tropics, least toward the poles; increased over large bodies of water, decreased as we go inland from the sea; increased by heat, as in summer and at mid-day, and decreased by low temperature, as in winter and in the morning.

To more fully consider the importance of the real atmospheric humidity, it is to be noted that the air is capable of holding moisture, mainly according to its temperature. The expansion of air under the influence of heat increases its capacity for retaining invisible vapor from about half a grain in a cubic foot, the limit at zero, with a barometrical pressure of thirty inches, to 19.84 grains, the limit at 100° Fahr. (See Table II.) As to a loss due to change in the weight of the air, many complications present themselves, among which a very slight one is the fact that dry air weighs from 2 grains at a temperature of 44° Fahr., to 8 grains at 80° Fahr., more in the cubic foot than saturated air.[1] The elastic force of vapor, or the absolute humidity of the atmosphere, Mr. Buchan says, "diminishes with the height, but the average rate at which it diminishes is not known."[2] The records of balloon ascensions give some information in this direction, but not sufficient to constitute any rule of change. The balloonist Croce-Spinelli, who perished in his aerial flight, recorded before he died that, at an elevation of 16,370 feet, "there was no longer vapor in the air." And the experience of many of us who have ascended great heights, agrees with that of Gay-Lussac, who recorded in his balloon ascension that, at 14,715 feet, "the air was so dry that for want of saliva he could hardly swallow bread." We understand that the air is twice rarefied at a height of 3½ miles, 4 times at 7 miles, 16 times at 14 miles, 64 times at 21 miles elevation, etc. This expansion of the air with increasing elevation is accompanied with a loss of part of its humid contents, through the coincident cooling which takes place, so plainly described by Prof. Tyndall.[3] If, with our "mind's

[1] Smithsonian Meteorological and Physical Tables, by A. Guyot.
[2] Handbook of Meteorology, by Alexander Buchan, 2d ed., p. 162.
[3] The Forms of Water, by John Tyndall, LL.D., F.R.S., 1874.

eye," we follow a cubic foot of saturated air in its ascent through space, we notice the loss of most of its moisture through the chilling due to the expansion; but there is another portion of the moisture, as well as of the air, crowded outside the cube by the same expansion. Mr. Glaisher's tables, before referred to, which give the weight of vapor in a cubic foot of saturated air at different temperatures, are made for a barometric pressure of 30 inches. If these tables are as accurate for places a mile up in the air as for those at sea level, then there is no diminution of moisture due to elevation independent of temperature, and the saturated air of such height may retain more moisture in proportion to its weight for a given temperature than the air at sea level. The air at the School of Mines, in Golden, Colorado, 5900 feet above sea level, was found by Prof. E. J. Mallett to contain at 60° Fahr. 331 grains of nitrogen and 99 grains of oxygen, or 430 grains weight for the cubic foot, which is just about four-fifths the weight of air at sea level.

TABLE II.—*Showing Weight of Vapor, in grains Troy, contained in a cubic foot of saturated air, at temperatures between* 0° *and* 105° *Fahrenheit.*[1]

Temperature of air Fahr.	Weight of vapor in grains.	Temperature of air Fahr.	Weight of vapor in grains.	Temperature of air Fahr.	Weight of vapor in grains.	Temperature of air Fahr	Weight of vapor in grains.
0°	0.545	36°	2.469	63°	6.361	90°	14.810
4	0.649	39	2.759	66	7.021	93	16.176
8	0.772	42	3.076	69	7.739	96	17.648
12	0.916	45	3.426	72	8.521	99	19.235
16	1.090	48	3.811	75	9.372	101	20.357
20	1.298	51	4.234	78	10.292	103	21.535
26	1.674	54	4.696	81	11.291	105	22.771
29	1.892	57	5.202	84	12.376		
33	2.208	60	5.756	87	13.546		

Is there no diminution in the capacity of this rarefied air for moisture, due to this lessened pressure of three pounds to the square inch? The question is of importance to us as affecting the basis of our judgment of high altitudes. Besides, the Signal Service weather reports often come to us with relative humidity noted as "not corrected for elevation," and we are left to learn what such correction may be. Now, seeking to avoid the headache and uncertainty which are induced by the search for suitable explanations in Guyot's and Glaisher's complicated tables, we will simply say that the only correction given is by the former author,[2] and adds from two to three per cent. to the relative humidity of places of the altitude of the Rocky Mountain base. This correction, as affecting the dryness of air in high altitudes, amounts to but little; besides, as before inferred, the comparison of climates by the *relative humidity* of each, is superficial, when temperature is not taken into account. With temperature, however, as indicating the capacity for moisture, the question becomes equivalent to this: Is the comparison of climates by the *absolute humidity* of each just? Our argument in advance has been that possibly it is unjust to high altitudes, as far as desirable dryness is concerned. Let us suppose it just, however, and (adding Guyot's corrections) make one or two comparisons which will suffice for all, and show the superiority of the highlands over the lowlands. We are enabled to compute the weight of vapor from Mr. Glaisher's table already given, or from one of Guyot's tables. Compare Jacksonville, Florida, with Denver, Colorado.

[1] Smithsonian Meteorological and Physical Tables, page 93.
[2] Ibid., page 72.

According to Dr. Baldwin,[1] the mean annual temperature of Jacksonville is 69.82° Fahr., and the mean relative humidity .696. The Signal Service records at Denver give a mean annual temperature of 49.2°; relative humidity .46 (corrected for elevation, .48). Then the average cubic foot of air in Jacksonville contains 5.6 grains of vapor, while in Denver the amount is 1.91 grains, about one-third as much. Or again, to compare the humidity of Jacksonville, Davos, and Denver, for the five cold months ending with March, the term of the thermal cure at Davos, we have:—

	Mean temperature.	Mean relative humidity.	Grains of vapor in a cubic foot.
Jacksonville	60°	68	3.92
Davos	23	84[2]	1.24
Denver	33	51[2]	1.13

The small amount of absolute humidity compared with the high relative humidity of Davos, shows the fallacy of judging solely by the relative humidity, while the whole table shows the remarkable dryness of high altitudes in winter.

The sensational effect of cold with excessive *absolute*, as opposed to a high ratio of *relative*, humidity, and really little moisture, is considerable. A gentleman, now a resident of Denver, who formerly resided in Jacksonville, says that he "stands the winter cold better," and "feels less chilly," in Colorado than in Florida; and the temperature of 58° Fahr., in doors (which is always very comfortable in Denver), is said by Prof. Walshe to be "disagreeably chilly" at Madeira. Excepting certain high altitudes where mountains act as constant condensers of moist currents, it is now incontestably shown that, as to temperature and humidity, coolness and dryness are progressively concomitants of increasing elevations. The proof that cool, dry climates, or high altitudes, are better than warm, moist ones, or tropical lowlands, needs no better conclusion than the evidence furnished by Dr. C. T. Williams's excellent analysis of 593 winters spent by 251 consumptive patients in foreign climates. I quote with pleasure (and with thanks for the unintentional favor) the indisputable evidence which this zealous student of European and lowland resorts gives for high altitudes.[3] I have already adverted to the experience of 243 consumptives who spent 386 winters in English "home" stations, which resulted in the colder resorts carrying off the palm.[4] Now as to the experience in foreign climates, Dr. Williams concludes: "As to what class of patients profit most by dry climates, it has been shown that, taking collectively all forms and degrees of phthisis, the dry climates are the most likely to arrest the disease. . . . As to the desirability of moist climates for consumptive patients, the evidence is decidedly against their use in the treatment of ordinary chronic phthisis. The addition of warmth only makes the damp tell more unfavorably, though a strong saline element and invigorating breezes do something to counteract the humid influence. Still even these do not place a moist climate on the same level as a dry one."

Diathermancy of the Air.—A striking, and to most people a novel feature of high altitudes, is the clearness of the air, as shown by the intensity of the sunshine. I was impressed with this characteristic of Colorado

[1] "An address on the Climatology of Florida." By A. S. Baldwin, M.D., President. Proceedings of the Florida Medical Association, Sessions 1874 and 1875.
[2] Corrected for barometrical heights by Guyot's Meteorological Tables, 3d ed. page 72.
[3] Lettsomian Lectures, Lecture III. [4] Id. op., Lecture II.

air on my first arrival three years ago, when, while riding toward the northeast, on a cold winter afternoon, the exposed portions of my body became chilled, while the back of the carriage seat, on which the sun was shining, was very hot; and I have ever since been curious to understand the phenomenon. After ineffectually endeavoring to get the Signal Service officers to study this matter, I conceived the idea of determining for myself the ratio of difference between the temperature in sunshine and shade, for each decided elevation, believing that, with the same conditions, excluding the influence of the wind, artificial heat, etc., observations at a few stations would serve to establish an approximate rule for almost all elevations. Accordingly, since February last, the desired temperature has been recorded at Washington, D. C.; at Royalton, Vt.; at Denver; and more lately by C. S. Richardson, C.M.E., at the Dolly Varden mine, on Mt. Bross, Colorado. Ordinary thermometers in metallic frames, and of Fahrenheit scale, have been used, observations having been taken at 2 P. M., on clear days only; and western rooms, not artificially heated, and with western windows, having been chosen. The temperature of the room has been taken in the shade and sun, with the window closed, and also with the window open, the direction, etc. of the wind being noted. The results must be taken as only approximate for general conclusions, because of the many conditions by which they may be affected, the fewness of the stations, and the short time the observations have been continued, extending only over parts of March, April, May, and June.[1] I have lately found an average of the same kind as my own, taken at the city of Mexico by Dr. Jourdanet,[2] which I add to the accompanying table:—

TABLES III. AND IV.—*Showing Diathermancy of the Air.*

STATIONS.	Elevation above sea.[3]	Difference between temperature in sun and shade in-doors, window shut; from	Average difference.	Ditto, window open or out-doors, the windy days excluded; from	Average difference.
Washington, D. C. . .	80 feet.	12° to 37°	23°	17° to 37°	24°
Royalton, Vt. . . .	500 "	16 to 42	27	23 to 47	28
Denver, Col.	5200 "	40 to 48	43	46 to 55	50½
Mexico, Mex. . . .	7600 "	....	...	56 to 65	60[4]

STATIONS.	Elevation above sea.	Temperature in shade.	Temperature in closed box with glass cover (sunshine).	Difference.
Mt. Bross, Col.	13,400 feet.	56°	162°	106°
Alma, Col.	8,800 "	68	154	86
Denver, Col.	5,200 "	88	160	72

Besides the increase of diathermancy, the tables show the more uniform clearness of the air at high than at low altitudes. From these

[1] Lately I have been pleased to learn, from Dr. Vacher's brochure on "Davos, Le Mont Dore," that others also have been making observations similar to my own. But as these observations, one with the thermometer in vacuo, and another, that of M. Saussure, which consisted in "exposing to the rays of the sun a thermometer placed in a wooden box lined with black cloth, and covered with glass," were not made under conditions favorable and usual to human existence, I have more confidence in the utility of my own; though the contrasts between sunshine and shade are not as great in my observations as in the others.

[2] Le Mexique et L'Amérique Tropicale, Climat, Hygiène et Maladies, par D. Jourdanet.

[3] Partly from Dictionary of Elevations and Climatic Register. By J. M. Toner, M.D., 1874.

[4] Taken in January.

tables we can approximately arrive at the desired result, the difference between sunshine and shade, or the heating power of the sun's rays. Mr. Richardson has kindly taken many observations for me on Mt. Bross, the difference between the temperature in the sun and in the shade out-doors at 2 P. M. being only from 10° to 20°, and not showing the real diathermancy of the air. This I attribute to the rapid radiation of heat and constantly moving cold air of that exposed place. As there was no suitable building in which to try the other test, I prepared pasteboard boxes, with glass covers, and with black velvet lining, in which we took the temperatures at Denver, Alma, and on Mt. Bross, at 2 P. M. for a few days, at nearly the same season. The differences between these records and the temperatures in the shade, noted at the same time, serve to show the increasing diathermancy of the air at these greater elevations, notwithstanding the peculiar influence of the cold air and wind of those heights. Now the differences between these diathermancy tests tally as follows for the given differences in elevation:—

TABLE V.

Stations.	Difference in elevation.	Average difference in diathermancy.	Average difference in elevation for one degree of diathermancy.
Washington and Denver	5120 feet.	23°	223 feet.
Washington and City of Mexico . . .	7520 "	36	209 "
Royalton and Denver	4700 "	20	235 "
Royalton and City of Mexico	7100 "	32	222 "
Denver and City of Mexico	1400 "	9½	148 "
Denver and Mt. Bross	8200 "	34	241 "
Denver and Alma	3600 "	14	257 "
Alma and Mt. Bross	4700 "	20	235 "
		Average . . .	221 feet.

Thus is evolved a general rule, which may be changed by more accurate and extended observations, of *one degree greater difference between the temperatures in sun and shade, for each rise of about* 220 *feet.*

This diathermancy (the increased facility with which radiant heat is transmitted) explains as clearly as the absolute humidity of the air, the daily fluctuations in temperature so gradually augmented with increasing elevation. But the absolute humidity, or elastic force of vapor in the air, is the main key to its transparency, the elevation only giving an additional increase in diathermancy to that produced by the decreasing humidity. So dry, low places, like Fort Yuma, California, will be exceptions to the rule given. However, this does not disprove, but, on the contrary, helps to establish the increased diathermancy of high altitudes, where, as we have already shown, there exists a considerably lessened absolute humidity. The effect of this diathermancy of high altitudes on solar and terrestrial radiation is wonderful. The solar radiation is rapid, and soon after sunrise the temperature rises, because of the slight resistance which the rarefied and dry air offers to the sun's rays; while, after sunset, the terrestrial radiation is also rapid, because there is no moist envelope shrouding the face of the earth to prevent the natural cooling of the dry ground.[1] The climax of this proof is as follows: Reasoning from the effects inimical to life in mines, cellars, and ill-lighted abodes,

[1] The character of the sunlight of high altitudes is a nearer approach, if possible, to white light, than at sea level, and many find it desirable to wear gray-blue or smoked glasses to neutralize its effect.

to the benefits of sunlight at low levels, where a stratum of moisture intercepts the sun's rays like a thin cloud, and thence to the healthful influence of the unobstructed sunshine of the highlands, we can say, without fear of successful contradiction, that the beneficial effects of sunshine increase with increasing altitude. It is further to be noted that, the sun's rays being less obstructed, there is a proportionately *longer* influence of sunshine in each twenty-four hours, besides the excess due to a usually cloudless sky over such plains as those east of the Rocky Mountains.[1] Thus, too, is explained a certain degree of incorrectness of ordinary temperature records of the Signal Service, which are taken in the shade and on the north side of the building, with the cold nine o'clock observation doubled to make up the general average. We are warmer during the hours in which being out of doors is desirable, on the elevated western plains, than is indicated by the temperature records.

Atmospheric Electricity.—The importance of atmospheric electricity as a curative power in disease is not to be measured by the little we know about the element itself. The experience in high altitudes of persons of susceptible nervous temperament goes to substantiate the universal evidence of science that, as a rule, electric tension in the atmosphere is increased with each decided elevation. Leaving out of consideration the change in electric tension and the negative quality of atmospheric electricity during storms, we learn from extended observations (Kew observatory) that with a clear sky the electricity of the air is always positive, which quality, Alexander Buchan says, becomes manifest "on flat ground" at a height of five feet. It would seem that, with the increase of tension due to elevation, the positive electricity of the air, so abundant in dry elevated regions, would be constantly nearing the negative electricity of the earth. With no other evidence to prove this than the experiences of our nervous systems, I shall claim that this is the true state of electric tension of high altitudes, till evidence is produced to the contrary. Now here is important teaching for the consumptive multitude, in fact for most of the human family. The houses we live in, and the piazzas and trees we sit under, are intercepting this vital fluid, which it is very probable we were intended to utilize more than we do. The conception and realization of this truth make us pine for the nomadic life of the travelling Israelites and Arabians, because we are sick at heart in view of the fact of increasing deaths from consumption with increasing civilization.

High-altitude experience goes towards strengthening the common-sense idea of the consumptive's course everywhere, which is in favor of an *out-door life*. The continued mediumship of the human body between the *negative* ground and the *positive* air, whether a man is on foot or on horseback, is a constant renewal of his vitality. Here is a secret of the great utility of camping out and "roughing it" in dry, elevated countries. You lie down to sleep, as only a tired camper-out can, on the ground, and get up in the morning from your *negative electric* bed to stretch yourself in the *positive electric* air. I have yet to meet with the experience which will dissuade me from always giving the advice to consumptives to live in the open air and sleep on the ground, whenever conditions are favorable for so doing.

[1] According to records in the Signal Service office at Denver, there have been only thirteen days since January 1, 1873 (to August, 1876), in which the sun has been invisible the whole day.

Dr. James Henry Bennet, whose personal experience with phthisical invalids entitles his opinion to great consideration, thus emphasizes the importance of defective vital or nervous power as a cause of pulmonary consumption, which cause even underlies "the most minute anatomical and histological researches." "Clinical observation shows that the manifestation of these forms of lung disease, chronic and acute, must be looked upon as the evidence and result of a serious, perhaps final, diminution of vital or nervous energy. . . . . Unless the vitality of the individual can be roused, the morbid condition will surely progress, and life will be extinguished sooner or later, according to the state of the constitution of the patient, and to the type of the disease."[1] Without delaying to add much corroborative proof at hand of the soundness of this conclusion (more especially the influence of inheritance), we can immediately draw the inference, which is abundantly substantiated by experience, that the increased electrical influence of high-altitude atmosphere is one of our most important aids in the successful battle against consumption.[2]

Ozone.—As to ozone, reliable deductions cannot be drawn from experiments made, because of the many liabilities to error with Schönbein's test, which was used—the starch and iodide of potassium paper—and especially one element of error which is liable to be increased in high altitudes, namely, the decided dryness of the air. But from the tests which I have had made on the western plains and in the mountains, the usual increase of ozone in the mountains is shown, while on the plains there is a good proportion, considering the lack of moisture and distance from the sea. A variation in the amounts observed may, I think, serve as a profitable indication of the special advantage of living in the mountains during the heat of summer. It is this, that the excess noted during the spring months on the plains, came proportionately later in the season the higher up the observations were made. This utility of going into the still higher regions in summer is in consonance with the increased benefit of the cooler temperature which we have already shown elevation insures.

Winds.—While not much can be said of the winds of high altitudes, independent of local peculiarities, yet two general facts may be mentioned, which will not be considered detrimental to elevated climates in view of the generally admitted healthful influence of a moderate amount of wind. The total movement of the air at nearly all exposed high altitudes, is of course greater than that at similar places in the lowlands, yet not nearly so much greater for many points as would generally be expected. As I have elsewhere explained,[3] the total movement of air,[4] and the number of observations of wind direction, as to winds which are usually mild and bring pleasant weather,[5] for some elevated stations as at Denver, Colorado, compare most favorably, for mildness of results, with the records of low altitude stations. The winds of high altitudes being less diverted from their general course, their indication as to the

[1] Pulmonary Consumption. By James Henry Bennet, M.D., 2d ed., p. 16.

[2] Some further remarks by the author on this subject may be found in The Archives of Electrology and Neurology, for November, 1874. "The Influence of the Climate of Colorado on the Nervous System."

[3] "The Winds of Colorado." Report to the Colorado State Medical Society, 1876.

[4] Plate X. "Total movement of air, with resultants." Statistical Atlas of U. S. from ninth census, 1870. By F. A. Walker, M.A., 1874.

[5] Annual Reports of the Chief Signal Officer, U. S. A.

character of weather is more constant than at most low stations, and especially is this the case of inland localities. For instance, according to excellent charts, in the Annual Report of the Signal Service Bureau for 1876, giving wind directions which are most, and those which are least likely to be followed by rain, the indications are very strong for Northern and Eastern Colorado, and Southeastern Wyoming (Denver and Cheyenne), that the south, southwest, and west winds bring pleasant weather, while their opposites, the north, northeast, and east, bring most of the rainy weather.[1] In Eastern New Mexico (Santa Fé), nearly the opposite rule holds good; the northwest, north, and northeast winds bringing pleasant weather, while the southeast, south, and southwest winds bring most of the rain. The damp atmosphere comes in both cases from below—the Missouri valley and the Gulf of Mexico—while the dry, warm air comes from over the mountains, which have acted as condensers. This is proved by the records of the Pike's Peak station, where the rain-bearing winds are the above-mentioned dry winds on the plains, and the pleasant weather winds are the rainy winds on the plains.[2] Another point is that winds of high altitudes, while they have the same influence in purifying the air, have proportionately less force, rapidity being the same, according to the lessened weight of the air due to elevation. It is to be mentioned, however, that in desirable dry regions, as the Eastern Rocky Mountain slope, and especially where the adobe soil of New Mexico, Southern Colorado, and Western Texas, exists, the dust storms which strong winds raise are a disagreeable element, to be avoided for the time being by those having weak or diseased lungs.

Altitude.—The physiological effects of high altitude on man are well understood when we consider the lessened pressure of the air which obtains at a given elevation. Reckoning the superficial area of a man's body as sixteen square feet, and the pressure to the square inch as three pounds less at the base of the Rocky Mountains (6000 feet), than at sea level, we have this surface relieved of the enormous weight of nearly 7000 pounds. With these figures in our minds, we are struck with the wonderful adaptability of the normal human organism to changes in atmospheric pressure, when the limit of danger lies in an atmosphere almost twice rarefied on the one hand, and five times condensed on the other.[3] But when we go further, and learn that Lieberkühn[4] has computed the area of respiratory surface in both lungs of man to be 1400 square feet, which Dalton avers is not exaggerated,[5] we find a subtraction of pressure from those organs which is truly wonderful, even for an elevation of one mile. The respiratory organs, then, are those which are chiefly influenced by the lessened atmospheric pressure, the effect on other organs being mainly secondary to the induced activity of respiration. Let us analyze the *modus operandi* of this phenomenon. The proportions of oxygen, nitrogen and carbonic acid, to each other, do not vary greatly for habitable elevations. There is then a fifth less oxygen in a given

[1] See tables of winds in "The Winds of Colorado," just mentioned.

[2] This evidence seems to be opposed to an explanation of sudden changes in temperature in New Mexico, given by Dr. Gardner as "depending on whether the wind blows from the dry, parched plains of Texas, or from the snow-crowned summits of the Rocky Mountains." "Diseases peculiar to Mountainous Regions." By W. H. Gardner, M.D., U. S. A., Post-Surgeon, Ft. Union, N. M. (Am. Journ. of Med. Sciences, July, 1876).

[3] Parkes's Practical Hygiene, 4th ed., p. 409.

[4] Simon's Chemistry of Man. Phila. ed., 1846, p. 109.

[5] Human Physiology. By J. C. Dalton, M.D., 3d ed., p. 236.

space of air a fifth rarefied, as at 6000 feet, and the lungs must inspire a fifth more air to get the same amount of oxygen. This is favored, first, by quickened respiration, which as well as the coincident increased activity of the heart is best shown in aeronauts, who make no muscular exertion as they rise.[1] Dr. Vacher says that M. Gaston Tessandier, the surviving aeronaut on the Zenith, gave him the record of his respirations as 26, at 5300 metres (17,331 feet), and on earth 19 to 23 per minute. Dr. Vacher himself, when he went to investigate the wondrous effect of the winter air cure (Cures d'air), at Davos, made careful estimates of his respirations, which were 18.2 per minute at Davos, while at Paris they were 16.6, his pulse increasing from 69 to 78 beats per minute.[2] As I came to Colorado three years ago, I noted an increase in my respirations from 20 to 30 per minute, in 24 hours, while rising 4000 feet in Western Kansas, and an increase of the pulsations from 72 to 82.[3]

I thought I noticed another of the immediate effects of elevation, which I have nowhere seen mentioned, but which may yet be of account in studying the therapeutics of the high-altitude cure of phthisis, an early and almost constant symptom of which disease is *prolonged expiration*. I refer to a quicker expiration, which was as though the increased number of respirations per minute was made up from the time given to expiration.[4] As to the effect of altitude on breathing, Dr. Vacher takes issue with Dr. Spangler, of Davos, and also with Dr. Jourdanet, formerly of the city of Mexico, the two latter gentlemen claiming that "the lungs supply a deficiency of oxygen by deeper and slower," while the former emphasizes the "more frequent" respirations. I shall have to differ with these gentlemen on both sides—especially with Jourdanet—saying the respirations are both deeper and more frequent, at first the *frequence*, and afterward the *profundity*, predominating. According to Coindet, who made 1500 observations of the frequence of respirations in French and Mexicans, they were 19.36 per minute for the French, and 20.30 for the Mexicans. The greater depth and breadth of the lungs of inhabitants of high altitudes has been noted by all observers who have had favorable opportunities for testing the question. Our knowledge of the mountain Indians of the western part of the United States, tallies with the fact noted by Jourdanet, "that the Mexican Indians, though of medium height, have unusually large and wide chests, quite out of proportion to their size." Even children, born in the Rocky Mountains, have been noticed while yet young to have chests of unusually large capacity, and the increased circumference of the chests of consumptives after undergoing the high-altitude treatment, is shown in many of Prof. Weber's cases, as well as in my own. M. Jaccound, at Saint Moritz, says there are not only more frequent, but fuller respirations. "This circumstance brings

[1] Balloon Ascents; by Parkes:—

Biot and Gay-Lussac, at	9,000 feet,	increase of	18 to 30	beats of the pulse.
Glaisher, at	17,000 feet,	"	10 to 24	" "
Glaisher, at	24,000 feet,	"	24 to 31	" "

[2] Op. cit.

[3] This record is of use, as compared with that of Dr. Vacher, who was probably in health, to show the increased disturbance elevation favors in those whose lungs are impaired; for at that time, ten months after pulmonary hemorrhages, my right apex and some other peripheral lung tissue showed signs of being clogged with inflammatory or other products, since happily driven away by Colorado's light air.

[4] Colorado as a winter resort for invalids. Chicago Med. Examiner, Jan. 15, 1874.

into use certain portions of the lung I call idle, because in ordinary conditions they take only a small part in respiration."[1]

The exercise of the will makes a great difference in the frequency of respiration, and besides, I think, the respirations in high altitudes are more generally increased in frequency by muscular exertion than in low countries, as any member of an Alpine club can testify. Lungs of habituated capacity to high altitudes have little incentive to frequent respiration when perfectly quiet, as is usual when the respirations are counted. Dr. Jourdanet evidently expresses this opinion in furtherance of his theory (Désoxygénation de l'air),[2] that the deficient oxygen inspired at great elevations is the cause of the immunity from phthisis. Now as to both fact and theory I have to differ with this noted author, in reference to the rationale of high-altitude therapeutics. So I may be excused for quoting and commenting at length on his views as given in his "Le Mexique et l'Amérique Tropicale," page 295. "We have stated that anæmia is the habitual condition of men who people great elevations, anæmia by the imperfection of the respiratory act. If it is natural to admit, *à priori*, that this anomaly may have a necessary influence upon the beginning, nature, and progress of maladies in general, with how much more reason must we acknowledge that the organ which presides over the respiratory functions would feel the first influence of it? The lungs in fact find themselves in the presence of a rarefied air, dry, and of diminished barometrical pressure. The three conditions of rarefaction, dryness and lightness of the atmosphere, evidently render the endosmose of oxygen, destined to feed respiration, less easy. But if we are correct in saying that the most natural stimulant of any organ is the element in which it acts, we may affirm, without fear of contradiction, that the lungs are always stimulated in a less degree upon the heights than at the level of the sea." After clearly showing evidences of considerable immunity from phthisis on the Anahuac, and at other elevated stations, he continues: "Moved by the consoling spectacle of this happy influence of great elevations upon consumptives, I have sought a reason for it with the view of turning this fact to a practical account. Several elements are grouped in my mind, in the resolution of this difficult problem. Upon one hand I am governed by an incontestable fact, already proclaimed as a general fact throughout this work; it is that the chronicity of inflammatory affections is incompatible with the climate of the heights. On the other hand, I consider the rarefaction, lightness, and dryness of the air. The rest which results from these conditions excludes the idea of any permanent excitation of the lungs. I consider that, in these circumstances, the lungs find a guarantee against pulmonary phthisis, while upon the coast, in a heavy and humid atmosphere, which oxidizes everything with which it comes in contact, a general predisposition to a chronic inflammatory condition favors the progress of tuberculization. My mind is struck by the importance of these curious facts, from which it draws the irresistible inference that an inflammatory element is necessary to pulmonary tubercles, to follow out the divers phases of their existence, etc."

Let us briefly dissect these anomalous theories, without objecting to the ultimate facts they are intended to explain. This "respiratory diet," as Jourdanet calls it—the theory which makes a deficient supply and

[1] La Station de Saint Moritz, p. 34.
[2] Dr. Vacher's Davos, Le Mont Dore, page 13.

lessened inspiration of oxygen the cause of the immunity of high altitudes—takes no cognizance whatever of the adaptability of the respiratory function to changes in elevation. The bare mathematics by which we get this deficit for high altitudes, which Dr. Vacher reckons is "a daily deficiency of 139 grammes" of oxygen for an ordinary person at Davos, throws out of consideration the existence of a pneumogastric nerve with its reflex function to supply any lack of air, or oxygen its vital constituent. Even if the deficiency of oxygen is not compensated for by more frequent and deeper respirations, it is not proved that endosmose of oxygen has any constant relation to the amount of oxygen inspired. On the contrary, Dr. Parkes says,[1] "in experiments on animals, as long as the percentage of oxygen did not sink below a certain point (14 per cent.), as much was absorbed into the blood as when the oxygen was in a natural proportion." This, too, is in addition to any augmented respiration due to elevation. It is much more probable, in view of the more perfect combustion (oxygenation) which takes place in consumptives who move to the highlands, that this deficiency is more than compensated for in high altitudes. As to the "*anœmia* by the imperfection of the respiratory act" which "is the habitual condition of men who people great elevations" (and so opposed to inflammation), I wish to ask, in all candor, how long are we to continue the manufacture, in our own minds, if not by drugs, of one disease to cure another? What, we are inclined to exclaim, have the much vaunted theories of allopathy (antipathy?) and homœopathy to do with this question? Is the improved and more active circulation of blood in the lungs (even more in them than in the capillary circulation in other parts of the body), a condition of "*anœmia*" on the heights? By no means; it is simply a condition of increased normal activity.

Let us examine this interesting point further, especially noting the changes in density of the respired air, while it is in the lungs. I have already adverted to the wonderful extent of respiratory surface in the lungs. This large area and the considerable change in atmospheric pressure are two striking factors of the changed respiration due to elevation. The mechanical effect of this lessened atmospheric pressure is what chiefly interests us. With the atmosphere say one-fifth rarefied, the respirations are deeper and more frequent. Then the density of the air in the lungs during inspiration would seem to be lessened in proportion to the greater quantity of air which has to be breathed. This increased approach to a state of vacuum in the lungs is calculated to draw the blood quickly into the pulmonary vessels. But expiration follows, and it is during expiration that the respiratory muscles have the greater power, greater because the muscles of expiration are in their normal condition for activity when the thorax is distended, and the thorax is more distended, because it has to labor with more air. I have already expressed the opinion that expiration rather than inspiration is shortened by the diminished pressure of elevation, and this fact would still further show the increased advantage of the expiratory muscles. Then the density of the air within the lungs is suddenly made greater than the density of that outside the body. The bloodvessels are pressed upon, and the previously augmented flow into the lungs is driven quickly on to the left side of the heart. It is possible that in very great changes of elevation, as in quickly climbing mountains above 12,000 feet, or rising to an immense height in a balloon, the pul-

[1] Practical Hygiene, page 407.

monary vessels may not stand this alternate rarefaction and pressure, and hemorrhage may result. But we are speaking now of reasonably high altitudes, which must be considered as varying for different invalids. The more rapid and perfect renewal of the capillary circulation in the lungs is opposed to the stasis of blood which is an early stage of inflammation. Not only that, but chronic hypertrophied tissue, accumulations of mucus, epithelial débris, and the products of inflammation, are "crowded to the wall" by this pressure, increased from within outward during expiration, and the lung thus purified and cleansed of the results of inflammation and morbid products in the blood, of lingering tendency, has no place for adventitious products or the deposit of tubercle.[1] Is this exaggerated activity of the pulmonary vessels and tissue a condition of "anæmia," and is it here we find the "rest" which "excludes the idea of any permanent excitation of the lungs?" "There is no rest for the weary" lung, and in this lies its salvation. Rest is best for a broken bone, but it offers no special antagonism to progressive degeneration of lung tissue, even if, as is here erroneously assumed, "the lungs are always stimulated in a less degree upon the heights than at the level of the sea."

As to the "endosmose of oxygen" being "less easy" in high altitudes, how, may I ask, if it is so, can the lungs enjoy the "rest" claimed, whose function it is to promote the endosmose of oxygen? Is it natural for an organ to be at rest, whose function is obstructed? I can see no obstacle to the endosmose of oxygen due to lessened atmospheric pressure; on the contrary, almost every effect of rarefaction of the air is to complete and perfect this process. According to Dunglison, "the rapidity with which endosmose is accomplished varies according to the nature of the septum or tissue, and of the penetrating body, and to the penetrability of the tissue." Now, in high altitudes, the improved condition of the capillary circulation, everywhere shown, but chiefly in the lungs, the quickened flow of blood on one side of the intercepting membrane, and the proportionately increased pressure and rapid renewal of the air on the other, all combine to favor the endosmose of oxygen. It is not then necessary for us to burden our minds with doubtful problems to understand the healthful influence of the atmosphere of high altitudes upon the organs of respiration. This more perfect oxygenation and capillary circulation of the blood favors a healthy renewal of tissue, and is constantly washing out of the system and preventing the focusing of impurities or disease germs, of which the consumptive body is full.[2]

The improved respiration and circulation bring demands for increased digestion and alimentation. Not only the lungs and heart, but the stomach, liver, and kidneys all contribute a share of this increased labor

[1] Elsewhere I have given the opinion, which I have never seen mentioned by others, that this *outward pressure* of the air within the lungs is chiefly what so generally cures asthma on the elevated inland plains and mountains of America, by the power it has in stretching the circular, muscular fibres of the smaller bronchial tubes beyond their easy contraction. Report to American Medical Association, June, 1876, on the "Climate of Colorado, and its relation to Pulmonary Diseases."

[2] An exception should be here noted, that this purifying process may be too great a task for the feeble remaining vital-force. Too extensive or far advanced disease in the lungs, the system being saturated with its accumulating effects, may naturally prove too strong an enemy for this or any other purifying process, and in the heated contest which is inaugurated the victim is quickly consumed. Such has been the fate of some who have suddenly risen to great elevations for the sake of restoration to health, when already advanced in the last stage of consumption.

to sustain a quicker but a more perfect life. The loss of flesh in one, and the gain in another (the latter especially for those new-comers for whom *to live* "is gain"), are but the preponderance of waste and repair over each other, which come to all with varying results.

Altitude of Approximate Immunity from Phthisis.

The study of the geography of phthisis gives us an additional and remarkable evidence of the curative influence of high altitudes. For if a mode of life, or a locality of living, should give a large number immunity from this disease, then there is the strongest inference possible that the afflicted who could and would adopt the same manner of life or place of abode might so far enjoy special chances of recovery. Some have thought that an exact line of immunity from consumption could be drawn, depending on altitude and latitude. Among these, Küchenmeister made such close estimates as to give a rule for the altitude of immunity in any latitude. In Switzerland it was 3000 feet,[1] and at the equator 9000, the difference for different latitudes in Germany being 375 feet less for each degree from south to north.[2]

But there are local conditions, variations in the climatic attributes we have thus far considered, which will greatly vitiate any exact rule of this kind. For instance, the Coast Range of the Sierra Nevada Mountains, in California, which condenses much of the moisture from the constant Pacific winds, and renders the high altitudes there uncongenial to most people, would give a very different climate from that of the elevated portions of the next range, and that in turn from the central range running through the continent. It is preferable not to try to defend any positive law, because, if an opponent meets with an instance of the origination of some type of phthisis above the stated elevation, you have attempted too much, and can go no further with him. It is not intended to convey the impression that a given altitude always insures immunity, nor that an approximate immunity may not exist at some low levels, as is claimed for certain far northern lands, and by one M. Maydell for a vagrant and extensive population who inhabit a steppe in Asia, said to be one hundred feet below sea level. We know nothing further of such localities, and are somewhat inclined to consider them like some new gold fields, and many far-off favored localities, where "distance lends enchantment to the view." The argument here intended is that, if twenty to thirty per cent. of all deaths occurring on the coast and borders of rivers are from phthisis, as on the fortieth parallel in the United States, ten to twenty per cent. on higher ground under 1000 feet, five to fifteen per cent. from that height to 2000 feet, two to five per cent. from that to 4000 feet, one to two per cent. from that to 6000, and seldom a case above that point, then the proof is conclusive enough as favoring high altitudes as a resort for consumptives, even though a signal service observer on the top of Pike's Peak should there suffer a lesion of the lung which should eventuate in his death from phthisis (a most improbable supposition). We speak then of an altitude of *approximate* immu-

[1] This is too low by one or two thousand feet, according to the valuable statistics of Dr. Lombard, of Geneva, who, Dr. C. T. Williams states, was one of the first to notice the rarity of phthisis in the high lying habitable districts of Switzerland.

[2] "Die hochgelegenen Plateaus als Sanatorien für Schwindsüchtige," von Dr. Friedrich Küchenmeister, 1868.

nity, which, like the good attributes of high altitudes already discussed, is found higher up as we approach the equator. And herein lies the strongest proof imaginable, outside of the experience of a considerable number of invalids, of the soundness of the combined arguments here presented, in regard to the climatic attributes of high altitudes.

Drs. Weber, C. T. Williams, and Walshe have given, in valuable works already quoted, abundant evidence from mortality records of this gradual decrease of deaths from phthisis originating in high lands. The corroborative evidence of these authors, with others, as Küchenmeister, Brehmer, Lombard, Archibald Smith, Füchs, Nühry, Vacher, Spangler, Jourdanet, Hirsch, and Guilbert, is full and explicit, but would consume more of our time than can now be spared. In general, the altitudes of approximate immunity are at or a little above the elevations given in the first part of this paper for favorable high altitude climates. In America, Dr. E. M. Wight characterizes the inhabitants of the Cumberland table-lands as "a people without consumption," their mode of life probably having much to do with their immunity. Dr. Archibald Smith, who practised medicine a long time in the Peruvian Andes, says that phthisis was unknown above 8000 feet, "except as an exotic," which evidence Dr. C. T. Williams well remarks "is important, because we know that, even up to 13,500 feet, large towns are found in that part of South America, such as La Paz, the capital of Bolivia, and therefore there probably exist some of the causes specially favorable to the development of phthisis." In Mexico, Dr. Jourdanet places the line of immunity at 7000 feet. His personal observation of the rarity of phthisis in the Anahuac, referred to the cities of Puebla and Mexico, where he practised medicine respectively two and eight years. These two cities, comprising a population of 270,000, showed a rarity of the disease, cases occurring only among the poorly fed and scantily clothed class, or "among those natives of the heights whom the interests of business induced to visit lower levels." During his ten years' practice in Mexico, he encountered but twelve cases of phthisis. All occurred among the indigent, a class rendered quite numerous in Mexico by natural indolence, acquired habits, and defect of social organization. In New Mexico, Arizona, and Utah, not a little evidence of an altitude of immunity is shown in communications I have had the pleasure of receïving from army surgeons who have served at elevated stations.[1]

[1] Dr. B. J. D. Irwin reports for Fort Defiance (6500 feet), Northwestern New Mexico:—"Surgeon John F. Hammond, U. S. Army, writing on the diseases of New Mexico, Rio Grande valley, says, 'Phthisis pulmonalis I have never seen in the country, except in two instances, once in an officer of the U. S. army, and once in an American emigrant. It was developed in each before he left the United States, and each very gradually improved. One resided in Lucoro, and the other in El Paso Del Norte.' I can add to the foregoing [Dr. Irwin writes] that during a service of some seven years in New Mexico and Arizona, ranging over a region embraced between the 31st and 36th degrees of north latitude—Fort Union, New Mexico, to Fort Buchanan, Arizona—I never saw or heard of a case of tuberculous disease (consumption) amongst the native inhabitants of those territories. When I went to New Mexico, in 1855, several recruits suffering with well-marked symptoms of pulmonary tuberculosis were sent along with the detachment destined for the troops then serving in that region. I kept track of them after their arrival, and had the satisfaction and pleasure of learning that they all outgrew the tendency to develop phthisis pulmonalis." Surgeon E. P. Vollum, from Camp Douglas, Utah, reports "In an experience of three years and a half in Utah, I have not seen a case of consumption that originated in the territory, and the Mormon physicians declare that no persons born in Utah—excepting Indians—have had the disease, to their knowledge." Among other army surgeons who have written in corroboration of marked immunity at their stations are B. G. McPhail, Camp Apache,

In Colorado, I will place the altitude of *approximate* immunity at 6000 feet. There are good grounds for this decision. I have made quite a thorough canvass of the subject among the physicians of the State, and have found but one case originating above this altitude, and that not very well authenticated—said to have been a case of catarrhal pneumonia of the right apex which afterward improved. Drs. T. G. Horn, of Colorado Springs, and G. S. McMurtrie, of Central, with others who have practised in the mountain districts for a considerable time, have reported that they could furnish no cases of the kind from their experience in Colorado. Between the elevations of 4000 and 6000 feet, however, I have found a few cases. There have been twenty-two in all up to the present date, occurring in the practice of physicians who have most of them resided in Colorado several years. Let us analyze these cases, and learn that it is greatly due to the looseness of the general term phthisis that most of them come under this nomenclature.

Dr. H. O. Dodge, of Boulder, reports four cases from that section; two of the patients have died, and two are not losing ground rapidly; all of these cases commenced with severe colds from exposure; three of the patients probably had pneumonia, and one suppression of menses, which has since continued: time they had lived in Colorado not stated. Dr. Joseph Anderson, of Golden, reports a case of general tuberculosis following miscarriage, rapid decline, and death in four months. Also a doubtful case of abscess following pneumonia in a young man whose father died of phthisis, and which has resulted in apparent recovery. Dr. Jacob Reed, Jr., of Colorado Springs, reports a case in a young man aged 18, three years a resident of Colorado, and with very marked tendency to phthisis by inheritance. Had had slight cough five months when he received a severe injury to one lung, followed by hemorrhage and death in one month. Dr. H. B. Tuttle, of Puebla, has seen two cases "one a sequel to scarlatina, and one following extreme exposure during menstruation, both badly treated, both fatal."

For the city of Denver and immediate vicinity, we have the records of all the deaths occurring since January, 1874, two years and a quarter. These records require information as to the origin of the disease. We find thirteen cases recorded as originating in Colorado. They are of "phthisis," "phthisis scrofulosa," "acute phthisis supervening" as a sequel to other diseases, etc. I have interrogated most of the physicians who reported these cases, and find two acknowledged mistakes, in that the disease existed before the parties came to Colorado; two rapid cases of *Cachexia Africana;* two cases of rapid decline after child-birth; a child one month old, whose mother died of the same disease nine days later; a case of abscess in the base of the right lung following pneumonia; acute phthisis following pneumonia, and hip-joint disease; a case of "whiskey consumption"; a fatal result in a very strong man, after hemorrhage from severe lifting; and a case following scarlatina. Now I cannot see that elevation offers any special antagonism to such cases; and as to accidental lesions of the lung, causing hemorrhage, and also acute double pneumonia, the mechanical influence of rarefied air cannot for the time be considered otherwise than injurious; while phthisis fol-

A. T. ("Arizona as a Health Resort for Consumptives," Virginia Medical Monthly, December, 1874.); W. J. Wilson, Ft. Bayard, N. M.; Oscar A. Woodworth, Mesilla, N. M.; J. V. Lauderdale, formerly stationed at Tucson, Arizona, and C. B. White, Fort Independence, California.

lowing scarlet fever is not to be wondered at, in that the convalescence from special fevers is generally noticed to be tedious in the Rocky Mountain regions.[1]

An additional evidence in favor of an approximate immunity due to elevation may, I think, be gained from the low rate of mortality from pneumonia, as well as from phthisis, as shown by our census statistics, especially as we are coming so generally to consider phthisis as of inflammatory origin; it is that while high altitude increases the unfavorableness of the prognosis of *acute* pneumonia, the ratio of deaths from the disease in the Rocky Mountain states and territories is greatly lessened, showing that there must have been few cases of pneumonia there to have passed into chronic disease.

Experience.

The crowning argument in favor of high altitudes should be the carefully recorded results of the extended sojourn in elevated climates of a good number of consumptives, representing the various types and stages of the disease. In presenting the following table of the records of such of my cases as have been kept under observation more than two or three months during my three years' practice in Denver, it seems to me the shortness of the time is greatly compensated for by only recording the results during the patients' stay in Colorado. It is to be noted that the results, after patients left the high altitude, were generally unfavorable. Those patients who had to be sent away immediately, and those who were not kept under observation two or more months, are excluded from the table, which includes all the other uncomplicated cases of phthisis I have recorded. Three only of the patients had been in Colorado longer than three years, the time of observation specified, while six were in the State the shortest time, namely two months. The period of sickness previous to coming to Colorado, dating back to a persistent cough, pulmonary hemorrhage, hæmoptysis, or evident cause, varied from two months to seven years. Some had resorted to other climates (low altitudes) without benefit. Quite the usual amount of inheritance, in some cases the most decided in my experience, existed in these cases.

Almost all types of phthisis are included, the classification as to the *Nature of Disease* being quite general, and only of account as indicating the usually more favorable prognosis for *inflammatory* than for *chronic tuberculous* cases. As to the *Stage of Disease*, I have endeavored to make close diagnoses, and may have put some cases in the third stage which other physicians would have placed in the second. However, the second stage, in my opinion, does not amount to much in high altitudes, and for practical purposes may as well be considered with the third. If lung tissue has become useless, and "softening" commenced, "excavation" is not far off, with the expansion of the lungs due to elevation. That such a transition is necessarily a part of the high altitude treatment in consumptives so far advanced, will not appear strange to the reflective mind; though its expediency in some acute cases may well be

[1] I have known a case of catarrhal and another of chronic pneumonia, contracted in Denver, in negroes who subsequently recovered, but who would undoubtedly have died from phthisis had they been sick in the East.

doubted. This positive character of the rarefied air prescription leads me to differ as to part of Dr. C. T. Williams's classification. I refer particularly to the "stationary" cases which include a good number of his patients. With the healthful respiratory quickening of high altitudes, the lungs are either not equal to the strain, or they improve under the exercise. I prefer to designate the apparently "stationary" cases of advanced phthisis by what sometimes occurs in them, namely, an extension of excavation, while the resistance of the system to the general effects of the disease is favored by the healthful surrounding influences.

As to whether this table represents the average severity of the phthisical patients who seek relief in Colorado, there is a *pro* and *con* to the question, with a preponderance of evidence in favor of its representing more than the average severity. Invalids whose disease is uncomplicated, and with whom the benefit is immediate and continuous, after their arrival in Colorado, do not, generally, consult physicians there. A proportion of such cases, including many I know to have improved, but of which I have no record of examination, are, of course, not included in the accompanying table. For instance, in the Pullman car in which I left Denver, to come to this Congress, there were four returning health-seekers—three ladies and one gentleman—three of whom went to Colorado in the first stage of phthisis, and the other in the second or third; all of them apparently now enjoy a complete arrest of the disease, and none of them employed a physician in Colorado. On the other hand, a few months after my arrival in Colorado, I wrote an article discouraging the sending there of consumptives with unfavorable complications, after the stage of softening, those affected with heart lesions, etc.,[1] and I am pleased to say that my professional friends in the States have seldom recommended such cases to me. However, the cases are severe enough taken as they are, and I am agreeably surprised with the progress of several having more or less excavation.

[1] Chicago Medical Examiner, January 15, 1874.

Table VI.—*Phthisis in Colorado.*

Analysis of 69 years, 2 months, spent by 66 consumptives in Colorado, with results.

	Number of cases.	Total duration of disease before coming to Colorado.	Average for each case.	Number lost sight of after a time, and probably living in Colorado.	Total duration of disease in years, after coming to Colorado till Aug. 1, 1876, till death, or till departure from the State.	Average for each case.	Much improved or cured.	Slight improvement.	Slight local extension, with general tolerance of disease.	Extension and advance.	Died in Colorado.	Improved, or disease tolerated.	Worse or died.	Died after leaving Colorado.	Fatality evidently hastened by leaving high altitude.	Number now residents of Colorado.
STAGE OF DISEASE ON COMING TO COLORADO—		y. m.	y. m.		y. m.	y. m.										
1st stage (deposit)....	25	34 8	1 4½	3	34 7	1 4½	17	7	....	....	1	24	1	1	1	15
2d stage (softening)..	11	14 4	1 3½	3	10 11	1	2	4	2	2	1	8	3	....	....	4
3d stage (excavation)	30	69 10	2 4	5	23 8	9½	5	5	8	10	2	18	12	5	2	13
Total	66	118 10	......	11	69 2	......	24	16	10	12	4	50	16	6	3	32
AGE WHEN THEY CAME TO COLORADO—																
Under 20............	3	4 9	1 7	1	5 4	1 9	....	1	....	2	....	1	2	....	....	1
20 to 30	40	68 6	1 8½	6	47 3	1 2	17	8	7	5	3	32	8	2	1	22
30 to 40	18	20 3	1 7½	4	11 6	7⅔	7	4	2	4	1	13	5	3	2	7
40 and over	5	16 4	3 3	......	5 1	1 ⅕	....	3	1	1	....	4	1	1	....	2
Total	66	118 10	......	11	69 2	......	24	16	10	12	4	50	16	6	3	32
NATURE OF DISEASE—																
Inflammatory (Pneumonic, etc.)..	25	25	1	3	23 7	11⅓	15	5	1	2	2	21	4	1	1	11
Catarrhal............	11	12 5	1 1½	2	9 2	10	3	2	4	2	....	9	2	2	1	6
Chronic tuberculosis.	30	81 5	2 8½	6	36 5	1 2½	6	9	5	8	2	20	10	3	1	15
Total	66	118 10	......	11	69 2	......	24	16	10	12	4	50	16	6	3	32
HEMORRHAGIC CASES—						No.										
Hemorrhage or hæmoptysis before coming to Colorado, probably without cavity............................						15	10	4	1	....	....	15	....	1	....	10
Ditto, probably with cavity...........................						6	....	1	1	4	....	2	4	1	1	3
Hemorrhage or hæmoptysis before and after coming to Colorado, without cavity.........................						1	....	....	....	....	1	....	1	....	....	....
Ditto, with cavity..................................						5	1	1	1	2	....	3	2	....	....	2
Hemorrhage or hæmoptysis after, and not before, without cavity..........						1	....	1	....	....	....	1	....	....	....	1
Ditto, with cavity..................................						2	....	1	....	1	....	1	1	....	....	1
Total...						30	11	8	3	7	1	22	8	2	1	17
Hemorrhage or hæmoptysis after leaving high altitude.						4	1	1	....	2	...	2	2	2	....	....

Both lungs affected, 38. Males, 54. Females, 12. Sent away to lower altitude, 7: 3 for heart disease, 1 irritable cavity, 3 far advanced. Became worse by going to lower level, 9

Of these analyses, that in reference to the *stage of the disease* is first and most important. The twenty-five first stage cases were 38 per cent. of the whole, and they averaged over a year and a third under observation in Colorado. Of these, 68 per cent. (17) were much improved or cured, 28 per cent. (7) gained slight improvement, and 4 per cent. (1) died. Of the one unfavorable case, an explanation is needed. A beautiful young lady, age twenty, came to Denver in July, 1875, having chronic laryngeal phthisis with complete aphonia, which had been coming on two and a half years; loss of weight, strength, and appetite, increased of late. Dyspnœa increased, and catamenia absent three months. Grandfather, father, brother, and aunt and uncle, died of consumption, and brothers and sisters living, not strong. Dullness with pleuritic adhesions at base of right lung, and deposit, which I believed to be of true tubercle, at left apex. Slight improvement during the fall. Afterwards she went to live on a ranche, by my advice, with instructions not to mind the loss of voice, but to gain in general strength. Gained seven pounds in two or three months. In February, she followed the meddlesome advice of somebody, and submitted to "throat treatment" from a man who claimed to be a physician, in a neighboring town. The local application of a sixty grain solution of nitrate of silver was a sample of the treatment under which this delicate creature lost thirteen pounds in less than a month. In two months more she died of acute phthisis. I regret exceedingly to have my list marred by such malpractice. Sixty per cent. (15) of these cases are now living in Colorado.

The cases of the second and third stages include a variety, the second embracing several acute, and the third some extremely chronic or prolonged third stage cases. Of the 17 per cent. (11 patients) who were in the second stage, averaging one year under observation, 18 per cent. (2) were much improved; 36 per cent. (4) slightly improved; 18 per cent. (2) had slight local extension, with general tolerance of the disease; in 18 per cent. (2) the disease extended and advanced; and 9 per cent. (1) died in Colorado—or 73 per cent. (8) improved or tolerated the disease, and 27 per cent. (3) became worse or died. Of the 45 per cent. (30 patients) who came to Colorado in the third stage, averaging nine and a half months under observation, 17 per cent. (5) were much improved, 17 per cent. (5) slightly improved, 27 per cent. (8) had slight local extension with general tolerance of disease; in 33 per cent. (10) there was extension and advance, and 7 per cent. (2) died in Colorado—or 60 per cent. (18) improved or disease tolerated, and 40 per cent. (12) worse or died. Of the whole sixty-six patients, 17 per cent. (11) were lost sight of after they had been under observation two or more months; 37 per cent. (24) were much improved; (24) per cent. (16) slightly improved; 15 per cent. (10) experienced slight local extension but tolerated the disease. In 18 per cent. (12), there was extension and advance; and 6 per cent. (4) died in Colorado—or 76 per cent. (50) improved and tolerated the disease; 24 per cent. (16) became worse or died; 48.5 per cent. (32) now reside in Colorado. Thus is shown by evidence, which is abundantly conclusive, the salutary influence of the elevated plains in Colorado upon phthisis—a disease eminently progressive in character. Thus too is shown, what those who live in Colorado have all along believed, namely, that the first or primary stage is pre-eminently the remedial period for high altitudes.

As to *age*, we find 4 per cent. (3 patients) under 20 years old when they came to Colorado, of whom one was the most remarkable example

of inheritance I have ever met; the little fellow weighing two and one-half pounds at birth, and seventeen pounds when four years old, at which time he came to Colorado. His father died of phthisis two months before he was born; his mother had been subject to a chronic cough; and five of her family and six of the father's had died of consumption, with evidence of its existence in the preceding generations. This case comes under the head of chronic first-stage, and the duration is estimated as since birth. The child has been in Colorado four years with gradual improvement. The other two cases under 20 were advanced acute cases. Both became worse, and were advised to leave. Sixty-one per cent. (40 patients) were between the ages of 20 and 30, of whom 42.5 per cent. (17) were much improved; 20 per cent. (8) slightly improved; 17.5 per cent. (7) experienced slight local extension with general tolerance of the disease; in 12.5 per cent. (5) the disease was extended and advanced; and 7.5 per cent. (3) died in Colorado—or 80 per cent. (32) were improved or tolerated the disease; and 20 per cent. (8) became worse or died; 55 per cent. (22) now live in Colorado. Between the ages of 30 and 40, 27 per cent. of the whole (18), resulted as follows: 40 per cent. (7) much improved; 22 per cent. (4) slight improvement; 11 per cent. (2) come under the head of slight local extension with general tolerance of disease; 22 per cent. (4) extension and advance; and 5 per cent. (1) died in Colorado; 72 per cent. (13) improved or tolerated the disease; and 28 per cent. (5) became worse or died. Of the 8 per cent. (5) over 40 years old, 60 per cent. (3) were slightly improved, none being much so; 20 per cent. (1) came under the head of slight local extension, with general tolerance of disease; and 20 per cent. (1) under that of extension and advance; or 80 per cent. (4) improved or tolerated disease; and 20 per cent. (1) worse or died. Thus is shown (as would be more clearly indicated if the first-stage cases were excluded) the less favorable influence of elevation upon young persons with acute phthisis, and upon old persons with chronic phthisis, inasmuch as all but one of those over 40 years of age, were of this latter character.

When we come to analyze the *nature of the cases*, we find 38 per cent. (25) which may be classed as essentially inflammatory; of these 25 patients, 60 per cent. (15) were much improved; 20 per cent. (5) underwent slight improvement; in 4 per cent. (1) there was slight extension and tolerance; 8 per cent. (2) advanced; and 8 per cent. (2) died in Colorado—or 84 per cent. (21) improved or tolerated the disease; and 16 per cent. (4) grew worse or died, three of the four cases being examples of acute cheesy pneumonia. The catarrhal group, 17 per cent. (11 patients), included several in the second and third stages of the disease, and resulted in 28 per cent. (3), being much improved; 18 per cent. (2) slightly improved; 36 per cent. (4) undergoing extension with tolerance; and 18 per cent. (2) advancing—or 88 per cent. (9) being improved or tolerating the disease; and 18 per cent. (2) becoming worse or dying. The third division, chronic tuberculous, naturally embracing many of the third-stage cases, formed 45 per cent. of the whole (30 patients), and resulted in 20 per cent. (6) being much improved; 30 per cent. (9) slightly improved; 17 per cent. (5) undergoing extension with tolerance; 26.6 per cent. (8) advancing; and 6.6 per cent. (2) dying in Colorado—or 77 per cent. (20) being improved or tolerating the disease; and 33 per cent. (10) becoming worse or dying. Excluding the acute and far advanced cases, this analysis shows in a remarkable degree the more favorable influence of the climate

upon inflammatory than upon catarrhal, and upon the latter, than upon chronic tuberculous cases.

It is useful to study the hemorrhagic cases, and to learn what influence elevation has upon this class. Believing that the pulmonary hemorrhages seemingly caused by elevation, which have lately been written about,[1] have been mainly from cavities, I have arranged the 30 cases (45 per cent.) which are known to have been attended by hemorrhage or hæmoptysis, at any time, with reference to the existence or absence of excavation. Seventy per cent. (21 patients) had hemorrhage or hæmoptysis before coming to Colorado; of these, 71 per cent. (15) were probably without cavity and favorably affected, while 20 per cent. (6) were probably with cavity—four coming under the head of "extension and advance." Six patients had hemorrhages both before and after coming to Colorado, five of whom had cavities when they arrived. One patient, subject to repeated hemorrhages before coming, which were much mitigated by a residence in Colorado, subsequently died from metastasis of the disease to the brain, which, by the way, is not a strange occurrence, considering the special influence of the climate upon the respiratory functions. Three had hemorrhage after and not before coming to Colorado. In reference to these three patients, it is well to explain that one with probably no cavity had heart disease and complete hepatization of the right lung—at best a most unfavorable subject for high altitudes. Another had slight hæmoptysis from a cavity while going over the divide; a third, with probable cavity, had hemorrhage, following a chill caused by extraordinary exposure in a storm, and all were evidently little injured by the hemorrhages. On the other hand, four of these 30 hemorrhagic patients had hemorrhage on going to lower levels, three of them being much injured thereby, and two probably dying in consequence. The synopsis of hemorrhagic cases shows a decidedly favorable effect of the elevated climate upon such patients as had had previous hemorrhage without cavity, while a somewhat less favorable effect can be inferred upon hemorrhagic patients with cavity than upon the general average of those with excavation. Therefore, the advantages of high altitudes are pre-eminently for hemorrhagic cases in the first stage, while hemorrhagic cases with excavation should be interdicted from going to great heights.

It would be instructive to compare these results with those in other climates, but we have none at hand so recorded as to furnish very definite conclusions. However, allowing for the absence of similar data in the records of others, the table may be considered as a favorable one, especially for cases in the early stage of phthisis. It is difficult to compare it with Dr. C. T. Williams's records of English and continental resorts, because, in his analyses, those patients who died during the time of observation do not appear to have been included in the estimate. We are only told that "since the climatic treatment 45 of the 243 patients (who spent 386 winters in English stations) have died."[2] Really it would seem to be an important inquiry whether any died during the climatic treatment.

There is strong evidence in favor of high altitudes to be inferred from the experience of Prof. Austin Flint, Sr.,[3] who draws conclusions from

[1] Some Remarks on Diseases peculiar to Mountainous Regions. By W. H. Gardner, M.D., U.S.A., Am. Journal of the Med. Sciences, July, 1876.
[2] Lecture II., Lettsomian Lectures.
[3] "Phthisis," by Austin Flint, M.D., p. 239.

his analysis of 670 cases of consumption, being all he has recorded, but not all those in which he has been consulted. For though he has not kept records of cases for the past eight or ten years, he says, "I should not have omitted to make records of cases of arrested phthisis coming under my observation." With this explanation, he continues, "seventy-five cases must be nearly all in which I have known an arrest of phthisis to take place, either with or without complete recovery, during a practice of thirty-eight years." Now, though there is no analysis of these 670 cases in reference to altitude, several are mentioned, especially among these instances of arrested phthisis, which may be said to have received the special benefit of elevation. Among his patients were several who resorted to the Adirondacks with benefit; seventeen who went to Minnesota, and were, with four exceptions, cured or benefited while there; one who went to the Peruvian Andes and was cured; and one who passed eleven weeks in New Mexico with decided improvement while he remained. Since, on the one hand, there is no evidence produced against high altitudes, and, on the other, so few of those patients who resorted to elevated stations for relief, we are compelled to present the latter fact as the only excuse for Dr. Flint's giving high altitude almost no consideration, and for his arriving at the conclusion "that the benefit derived from the change of climate is due, not so much to a climatic influence *per se*, as to the circumstances incidental to the change." I shall be the last to take issue with this renowned author, or any one else, as to the importance of out-door life and change of occupation; all the most useful means we can combine will not be more than enough to make the consumptive multitude reasonably healthy. Sometimes, by unfavorable cases, the physician is impelled to agree with Pope, that—

> "As man, perhaps, the moment of his breath
> Receives the lurking principle of death;
> The young disease, that must subdue at length,
> Grows with his growth and strengthens with his strength."

Accepting Dr. Flint's collection of cases of arrested phthisis as the largest, or one of the largest, that can be made from the experience of any one man in America, the results of the few cases I have presented must be admitted as an argument in favor of high altitude, in that the ratio of improvement is greater, although covering a shorter period of observation. From my every day experience among the people of Colorado, I am led to believe that as many cases of arrested phthisis (75) could now be found among any average collection of 5000 people at the base of the Rocky Mountains.

One further result of my analysis should not be omitted, so important is its bearing upon the time invalids should remain in high altitudes. The facts noted are that of six patients who have died after leaving the elevation, fatality thereby has evidently been hastened in three; that four of the hemorrhagic patients (a large proportion of those who left the heights) had hemorrhage afterward; and that nearly half (9) of all those who went back to the lowlands, became worse by so doing. Prof. Weber's cases furnish similar evidence: of thirteen cases arrested in high altitudes, two were twice relieved, the disease twice returning on going down to sea level. In six other cases phthisis was rekindled after a descent to sea level; and two patients, who were residents of high altitudes, moved to the lowlands and became consumptive, were apparently

cured on returning to the highlands, and were again taken sick when they returned to sea level.[1]

Jourdanet, among many striking illustrations of a failing or falling off of consumptives after leaving the heights, mentions the case of a young lady who three times went down to a lower level with a return of active phthisis, and three times, in turn, was decidedly relieved by a return to Puebla. Dr. C. T. Williams mentions almost as remarkable an instance of this loss of benefit by the return to low levels. Three of my cases show, in a marked degree, the injurious effect of going from Colorado to Southern California; one patient, in the third stage of the disease, died in two months after leaving the high altitude, and one in the second stage, in the vicinity of San Diego, lost ten pounds, had five hemorrhages, advanced to the third stage, succeeded in getting back to Colorado, and has improved since. A third patient, in the first stage, was one of six young men, four of whom, I believe, were consumptive, who went to Southern California under the restless impulse which often controls such persons. This young man, with two others of his party, were made perceptibly worse by their sojourn in California, and returned quickly to Colorado, considerably disgusted by their experience in the humid air of the Pacific slope. All of these patients were doing well enough in Colorado, and most of them assumed the responsibility of going to lower levels contrary to advice. As Jourdanet expresses it: "I preserve among my notes a great number of examples of the relief of the malady and of the obstinacy of the invalids in returning prematurely to the climate which kills them."[2] And in reference to those who have returned to a lower altitude, he adds: "I often ask myself if actual residence would not have finally vanquished the malady, which a few months' stay had relieved." By inference, I think, an argument might be drawn from the analogous experience of those gymnasts and athletes who abandon their accustomed exercise and so render their lungs liable to disease from lack of use; for we are all the time speaking in favor of healthful respiratory *activity* as opposed to the *rest* which is so desirable in the treatment of most diseases. Such is the experience which leads to the conclusion that a partial recovery necessitates a permanent residence. This statement carries with it the idea, consonant with experience, that complete recovery does not necessitate permanent residence, so that the time invalids should consider it imperative to remain in high altitudes is less for incipient or first-stage cases, in which complete recovery may be expected to occur.

Gathering from these arguments the conclusions which may serve as a guide to the physician in staying the progress of phthisis, many obstacles to successful work may be suggested. Among them, after the hindrances to the patients' following the most useful combination of measures prescribed, comes a difficulty in early arriving at a correct diagnosis of the existence of the disease. It is referable to ignorance and neglect on the part of the invalid, and, I fear, too often to inadequate knowledge on the part of the physician. Often I find the invalid's assertion that his indisposition is slight, that his doctor has examined him and said so, unverified by a correct and careful examination. The previous existence of dyspnœa, night-sweats, loss of strength and weight, are not belied in the real condition of the lungs. We must accept the conclusion which is inevitable, in view of physicians so often differing from each other, as they do, in opinion, that the discovery or invention

[1] Op. cit.

[2] Op. cit., p. 304.

of new means of physical diagnosis, and the thorough teaching of this branch in medical schools, should be earnestly encouraged by the medical profession.

The conclusions of my prolonged, and I fear too tedious, study, I have embodied in the following deductions, in which I respectfully ask your concurrence:—

I. Cool and dry, are better than warm and moist, climates.

II. The most favorable climatic attributes of low altitudes, especially the diathermancy and dryness of the air, are increasingly found with increasing elevation.

III. The favorable or positive influence of high altitude upon the progress of phthisis is best shown in the incipiency of chronic, inflammatory, and hemorrhagic cases; and in others in proportion as these characteristics exist, the more acute the inflammatory process, or the more active the pulmonary hemorrhage, the more gradual and tentative should be the rise in elevation.

IV. Partial recovery necessitates a permanent residence, which is therefore required either in an advanced stage of the disease, or when the recovery has been incomplete.

V. The unfavorable or negative influence of high altitude upon the progress of phthisis, is mainly seen in proportion as the disease approaches or is complicated with the following conditions: First, cardiac disease, if associated with increased labor and abnormal activity of the heart. Second, the stage of softening, in acute cases, associated with extensive deposit, or with an irritable, nervous state, and lack of desirable will power aided by the stimulus and hope of youth.

VI. The generally admitted rule that change of climate and mode of life is favorable to the cure of pulmonary consumption, in proportion to its early adoption, is rendered more stringent in that the results are more positive in case of a resort to high altitudes.

VII. The stimulating effect of high altitude, associated with increased respiratory activity and power, is opposed to the idea of rest, and constitutes a most important agent in arresting chronic phthisis.

VIII. High altitude is a means of arresting phthisis independent of, and in addition to, change of occupation and out-door life, and is worthy to rank with such hygienic measures.

IX. Such is the importance of an early resort to high altitude, that in incipient cases the patient should receive the benefit of the doubt, and the physician give affirmative rather than negative advice, in view of possibility of error in declaring the non-existence of phthisis.

X. High altitude being an important attribute of successful climatic treatment, a resort to a well-chosen elevated climate should constitute part of the physician's advice to every consumptive (who can follow it) for whom the elevation is not specially contra-indicated.

Discussion on Dr. Denison's Paper.

After the reading of the preceding paper, Dr. Thomas S. Duffy, of Rutherfordton, N. C., said:—The paper is a very interesting one. The subject is one of experience and observation. The matter of moisture in the atmosphere is very important. I have known coughs, contracted during dry weather, defy treatment, and yet disappear spontaneously when the weather became damp.

I have taken observations of the difference in temperature between mountain and coast springs, and have found the temperature to be 60° at an altitude of 1500 feet above the sea level, and 59° on the coast. Where I live it is very difficult to find cases of consumption. As this disease is seldom cured, we should consider preventive rather than curative measures. Perhaps the habits of the people of North Carolina are as efficient in creating an immunity from consumption as the climate. North Carolinians are not in the habit of shutting doors to keep out the cold, but increase the heat of their houses, and put on more clothing instead. Going from a warm to a cold atmosphere will not give cold, unless the person exposed has been previously exhausted by exertion. It is customary for people in my State to halt while on a journey and warm themselves, and then proceed in the cold. They do not hesitate, when warm, to plunge into cold streams. Such an act might kill a man coming from the harvest field, because he would be exhausted; but where there is no exhaustion, there is no danger.

I have been in Southern California, and have observed its climate. In the daytime, during the dry season, there is a cloudless sky. After the sun rises, a fog becomes so dense that objects cannot be distinguished at a distance of twenty yards. The atmosphere is heavily laden; the roofs drip as though rain had fallen; vegetation is sustained without rain. It is not observed that the fog induces consumption, but, on the contrary, the people seem comparatively free from it. It is a matter of doubt whether a moist atmosphere is conducive to disease; a moist, cold atmosphere may be, but a moist, temperate atmosphere is not.

Dr. A. S. Baldwin, of Jacksonville, Florida, said:—I am happy to hear the remarks of the reader of the paper upon a subject which I have studied for thirty-eight years in my own State, as far as climate is concerned. It is doubtful whether all the beneficial effects mentioned by Dr. Denison are the result of altitude. I have witnessed the same effects in Florida, where the atmospheric pressure is slightly above 30°, which is assumed to be that of the sea level, so that it appears to be the change of climate rather than the altitude that is so beneficial. I have seen, in a post-mortem examination, lungs studded with obsolete tubercle, the result of disease acquired twenty or thirty years previously, the patient's death being caused by exposure following a debauch; and I have known other cases of consumption, partly cured by the Florida climate, recur when the patients have exposed themselves carelessly. Some medical men administer oxygen as a remedy for phthisis, but I doubt whether this is judicious. By visiting Florida, where the atmospheric pressure is so great, the patient can get oxygen without the effort required for the same purpose in Colorado. This would give rest; and I believe in the value of rest for these diseases, and think that that is one of the reasons why Florida proves so beneficial.

The rain-fall, taking the whole State, is about 51 inches. In my section of the State I have recorded 64 inches in one year, and 31 inches in another. It is impossible to get the truth about the humidity of the atmosphere without taking observations extending over a long period of years.

A sick person requires an even temperature, and we always try to keep them from sudden changes. Experience has taught us this fact. In the excessively dry climate of the western plains there is a change of 60° between the atmosphere of the day and that of the night; surely that is harder on a patient than a change of but 10° or 12°. Therefore, we must have, as one of the components of a healthy atmosphere, enough moisture in the air to prevent such radiation as to produce violent changes. We have that in Florida; we have about 14° diurnal variation throughout the year, and a difference of only 20° in the mean temperature between summer and winter. It seems cooler in Florida than in other places, even when the thermometer indicates the same figures. There is no need to avoid the sun; one can ride or walk in it with impunity, and sunstroke is unknown.

Dr. Henry Gibbons, of San Francisco, said:—When invalids change their location and improve in health, the benefit is attributed to the influence of the new climate and locality, when it is more likely to be due to the mere condition of change. Removal from a better climate to a worse one often does much good, for a time. This fact renders it very difficult to form a correct estimate as to the best resort for consumptives. Our opinions on this subject are constantly changing. Twenty-five years ago it was our custom, in San Francisco, to send consumptives to the Sandwich Islands; but we soon found that to be a mistake. Then we fixed on other localities from which the best reports were returned for a while, until it was discovered that they yielded no permanent good. The climate of Santa Barbara, and of the southern portion of California, was then selected, and every body was confident of their curative power, and hundreds of consumptives came there from all parts of the world. But Southern California is now wearing out. To talk of "the climate" of California is a great mistake. That country has almost as great a variety of climates as all the rest of the civilized world combined. You may journey ten miles across a range of hills, and get from a noonday temperature of 65° into an atmosphere of 105°. The Pacific Ocean, along the coast, has a temperature of 53° or 54°, winter and summer. Put the thermometer in the bay, at the foot of the wharves in San Francisco, in July or August, at high tide, and it will fall to these figures. Of course the superincumbent atmosphere has the same temperature, and this cold air comes in daily during the summer towards noon, as soon as the sun has heated the interior. It often carries with it a dense mist. This is the ocean climate, which prevails only along the coast as far as the first highlands. or where a break in the highlands allows the cold wind to pour through. This cold ocean wind, as it works inland, mingles with the heated air, and forms in many localities a climate highly agreeable and healthful. Farther in the interior, there is a glaring sun, whose rays are never intercepted by a cloud for five or six months of the year. Here the temperature is high by day, and the air very dry, with nights always cool, but never cool enough for discomfort to the invalid. Dr. Denison is mistaken in speaking of the humid climate of Southern California. At San Diego, to which he makes reference, there is but a yearly mean of seven or eight inches of rain. The fact is that consumptives are improved in many climates where a permanent residence may be injurious. I am consulted habitually by letter, and personally, as to the best health resort; but now, after twenty-five years of observation and experience in California, I have ceased to designate any locality. My advice is, to all who have strength enough, "Get a horse or mule, and seek a climate for yourself. Ride, or lie by, as it suits you. Eat good, nutritious food, and drink milk or cream; spend all the time you can out of doors. If you find yourself improving decidedly at any given point, stay there a while, if so inclined, but do not stay one hour after the improvement ceases. Keep moving until you find the right climate and the right place. The chances are that you will get nearly well in searching for it." In confirmed and advanced cases, however, my advice is to go home and die among friends.

Dr. A. B. Stuart, of Winona, Minnesota, said:—Ten years ago I went to Minnesota as an invalid, accompanied by a brother-in-law suffering from pulmonary phthisis, resulting from exposure during the civil war. In two years my brother-in-law recovered, and increased in weight from 125 pounds to 170, and to-day does not have the appearance of a phthisical patient, though when he came he had undoubtedly a cavity in the upper lobe of the left lung. I have often known patients come to Minnesota in the first stages of the disease, and remain until they have considered themselves well. Beneficial effects may be lost, however, by staying too long. If a person is unable to take out-door exercise, Minnesota is no place for him.

Dr. H. A. Johnson, of Chicago, said:—I have known a number of consumptives go to Denver, Colorado, to Florida, to the mountains of North Carolina,

to Georgia, to Tennessee, and to the south of France, and come back enthusiastic about the beneficial effect of the change of climate. It seems that we are all agreed as to the advantage of an open air life, exercise, not pushed so far as to fatigue, sunshine, and good food. Dr. Denison has attempted to establish the fact that in addition to these beneficial influences there is another, viz., altitude. Unfortunately for the solution of the problem he has had to deal with, altitude is connected with all these other influences. How can we separate the influence of altitude from the rest? I believe that in the earlier stages of the disease, patients are benefited by going to the mountains, but, on the contrary, when softening has set in, when cavities exist, I believe that it is hazardous for them to do so. A warm, mild climate is better then. I had about fifty patients last winter in Florida, and they came back better; even those in whose lungs cavities existed were better than they would have been had they staid in Illinois. I therefore think it impossible, in our present state of knowledge, to prove that altitude, *per se*, is beneficial, and will continue to advise patients in the first stages of phthisis to go to the mountains, and in the later stages to go to Florida or Georgia.

Dr. C. J. Hare, of London, said:—I wish, as a stranger, to thank the author of the paper for the valuable facts therein contained on the treatment of phthisis. I have often recommended long sea voyages, and have seen my patients benefited thereby. I have seen patients come back marvellously improved after a voyage to Australia, and an enjoyment of the wonderful climate of Queensland. The region in which phthisis exists, gradually descends from the centre towards the poles. In Iceland and Norway the disease is unknown. It exists in some of the higher valleys of Switzerland, but at the same altitude, where there is plenty of fresh air, phthisis is never found. A winter residence in these places is good, providing the patient can take exercise. To delicate patients I usually recommend a southern, genial climate.

ON THE TREATMENT OF SIMPLE ULCER OF THE STOMACH.

BY

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[TRANSLATED BY CHARLES WINSLOW DULLES, M.D., OF PHILADELPHIA.]

THOUGH the difficulty of treating simple chronic ulcer of the stomach often renders our efforts futile, yet there are few diseases in which our art can be of so much service to our patients, both as regards hygiene and by medicinal treatment, properly so called. We must, at the outset, consider the general indications to be met.

I. In the first place we should aid the digestive function of the diseased stomach by selecting for it food easy of assimilation, both from its quality and its liquid form or slight consistency, and by reducing its quantity to that strictly necessary for the maintenance of life, whenever we are obliged as much as possible to spare the labors of the organ. For this purpose we must not omit to call to our aid the absorbent power of the rectal mucous membrane to lessen by so much the work of the stomach; and a suitably regulated diet should always be conjoined with a proper hygiene, both physical and moral.

II. While thus diminishing the work of the affected organ we must guard against unnecessarily fatiguing it with drugs. Since, however, radically curative methods have been, even of late, recommended for this disease by physicians of eminence, I shall not fail to mention them, so as to leave to each practitioner an opportunity for free choice; but I must avow that to this day I incline rather to the expectant method, advising at the same time most careful symptomatic medicinal treatment, in which the true physician may bring to bear all his knowledge, his sagacity, and his desire to be of use to his fellows who suffer, or who are threatened with actual danger.

In the first place, I shall describe the methods proposed by Trousseau, Ziemssen, and Leube.

(1) Trousseau's plan[1] is as follows: The patient takes two or three grammes of the subnitrate of bismuth, an hour before meals, three times a day for ten days, after which time the nitrate of silver is substituted, in pills containing each one centigramme, taken three or four times a day for five days. Then the course of bismuth is resumed for ten days, and followed for four or five days by two daily doses of calomel of one centigramme each. This routine is repeated again and again for three or four months, when it is discontinued, to be resumed after a month's cessation. Evidently this mode of treatment is too systematic, and does not allow for the very varied accidents which may arise and call for modifications during its course.

(2) Ziemssen's[2] method consists in the employment of an artificial

[1] Clinique Médicale, tome iii. p. 96.

[2] Ueber die Behandlung des Magengeschwürs, Volkmann'sche Vortraege, 1871, S. 16.

Carlsbad salt, composed essentially of sulphate of sodium, with a little bicarbonate and chloride of sodium. The patient takes from eight to sixteen grammes each morning, in water heated to 44° R., and should have two or three stools in the course of the day, the quantity of the salt being increased or diminished according to its laxative effect. Gradually small quantities come to suffice.

(3) The method of Leube[1] adds to that of Ziemssen the solution of meat indicated by the author, in which hydrochloric acid in small quantity serves to extract from the meat its nutritious principles. Unfortunately many patients have a repugnance to this solution, which, moreover, does not always bear transportation to distant places. Milk is added to the diet, with Zwieback[2] (or Einback); and by degrees the patient can take a more substantial regimen of thick soups, roasted fowls, etc.

For my part, I prefer to abstain from all radical treatment, placing in the first rank hygiene, and in the second the treatment of symptoms.

I. *Diet and Hygiene.*—Absolute diet, or rather absolute absence of food, should be enforced only exceptionally and very transiently, as after perforation, or immediately after a violent hemorrhage. Then nutritious enemata only should be used. In other cases patients do not bear food well, and we are obliged to give them milk by tablespoonfuls only, every three or four hours, until a larger quantity can be borne. Under all these circumstances, absolute rest in bed is indispensable.

Milk Diet.—It is now half a century since Cruveilhier recommended the use of milk as an exclusive or principal food in the treatment of gastric ulcer, and experience has perfectly justified his plan. For my part, I have seen from it effects often so salutary, sometimes so surprising, that I have, from year to year, become more its partisan. Occasionally a certain repugnance is met with on the part of patients who pretend that they cannot take milk in any form whatever. These cases are rare, and, on the contrary, patients are afterwards grateful to the physician for not having allowed himself to be checked by their prejudices and objections which are often ill founded. The exclusive use of milk is, moreover, rarely necessary for more than from six weeks to two or three months, and by degrees other articles of food can be added to it. Milk is sometimes better borne warm from the cow, sometimes heated, sometimes of the temperature of the room; and iced milk can be taken by some patients who can bear it in no other form. The quantity is increased little by little until fifty, sixty, or one hundred grammes every three hours are successively reached, and total quantities of a litre and a half or two litres are taken during the day—an amount sufficient for good nourishment. Patients who bear only thirty to forty grammes at a time, ought to take it every two hours. At night, feeding should, if possible, be interrupted.

Cow's milk is the most useful kind and that generally employed. Some patients, however, at first bear ass's milk better. Buttermilk and curdled milk, preferred by some, are exceptional forms. Where cow's milk of good quality cannot be had, it may be replaced with goat's milk. Sheep's milk, much the richest in nutritious matters, is sometimes used with success by the owners of large flocks and by their families. At the start

1 Krankheiten des Chylopoetisches Apparats, V. Ziemssen's Pathologie. Bd. vii., S. 113.

2 [Zwieback is made by taking rolls, cutting them in half, and putting them into an oven until they are brown and crisp. These are much used in Germany and Switzerland, crumbled and soaked in some liquid article of food. Einback is simple bread or biscuit.—TRANSLATOR.]

pure milk is sometimes badly borne, especially if there is a marked tendency to acid secretions. In such cases I add one-fourth part of lime water, or as much bicarbonate of sodium as will cover the point of a table-knife. Patients who at the beginning of the milk-treatment are constipated should take one or two cold water enemata daily. When the milk habit is once fairly acquired, then may be added to each cup one or two teaspoonfuls of arrowroot or rice flour; or small quantities of biscuit, of Zwieback, or of white bread may be taken with the milk. Sometimes capricious stomachs—especially in nervous and hysterical women—bear fat substances very well. To these cream may sometimes be allowed. They are the patients who at a later period often bear ham and smoked or salt meat better than fowl, veal, or fillet of beef. It is always well to mistrust the gastric caprices of hysterical women, since their sense of veracity is often warped.

When the stomach is habituated to milk, and partially reinstated in its digestive force, it can be accustomed by degrees to other forms of nourishment; to the farinaceous articles already mentioned, to beef-tea, to Leube's solution of meat; and, better still, to good chicken or beef broth, to which are added farinaceous substances, or small quantities of meat very finely hashed or cut; or the yolk of an egg may be beaten in, or some granulated gluten added—which is very nutritious because of its azotized constituents. Milk-porridges may also be alternated with broths.

Every one knows the great difficulty of getting good milk, especially in large cities, at the seashore, etc. It is, therefore, necessary to have a substitute for milk which can take its place, and be within the reach of all purses. Many years ago I learned to appreciate, in this respect, the excellent milk-powder (*poudre lactée*) of Nestle, of Vevey, which is recognized as much the best substitute for milk in the nutrition of young infants. I have likewise come to employ it much in cases of difficult digestion and embarrassed nutrition, in the treatment of chronic diseases of the adult, especially in that of chronic affections of the stomach, and particularly of ulcer. Since I have lived in Vevey I have thoroughly studied the process of manufacturing Nestle's Farina, and have been able to satisfy myself that the milk used is of the best quality, and that the same is true of the other ingredients, and above all of the methods and manner of manufacture. Fresh milk is first well examined, and then poured into the apparatus, heated by steam, and evaporated *in vacuo* at a temperature which does not exceed 40°–45° C. There is, thus, simple condensation without change of quality. The bread added to Nestle's Farina is made of the best wheat flour, by a plan which preserves its gluten; and the crust alone being reduced to a very fine flour it is so much the richer in azotized constituents. The extremely fine division of the milk in powder, and of the flour of the crust, rich in nitrogen, greatly adds to its digestibility.

A thousand parts of the milk-powder are found by analysis to contain 49.40 parts of water, 931.10 parts of organic matter, and 19.50 parts of ash. The azotized constituents form 21.40 parts in 1000. By microscopic examination I have found a division so minute that most of the molecules do not exceed $\frac{1}{500}$ of a millimetre in diameter, this greatly increasing the facility of mixing with water.

For each meal ten grammes may be mixed or cooked with fifty grammes of water, and gradually the quantity is carried up to twenty grammes in one hundred of water, it being well to warm this mixture to 37° C.

Gradually we may rise to thirty grammes in one hundred, and in place of two or three little meals give one of a hundred grammes every three hours, and even oftener, provided always that the preceding meal appears to have been completely digested. I should be glad if this mode of feeding, which has already gained an established place in the nourishment of earliest infancy, could be thoroughly tested in that of diseases of the adult. And as experimentation in such a matter is better than trusting to any recommendation whatever, I have requested M. Monnerat, one of the proprietors of the article, to place at the disposal of hospital physicians the material necessary for this study; to which M. Monnerat, who is a Syndic at Vevey, has very willingly consented. So that all my colleagues, who would like to experiment with the lacteous powder in pathological nutrition, may address him directly to obtain the quantity required for complete investigation.

In continuing the progress toward a more varied regimen, the stomach at first often bears better well-prepared mucilaginous soups of barley, oatmeal, etc., than broth; to these sago or tapioca may sometimes be added with advantage, and if, besides, they have beaten up in them the yelk of an egg, and contain some finely divided fragments of light meat, these soups can be made as nutritious as they are savory. When, later, we come to allow tender and well-roasted meats, such as chicken, veal, or beef, in small quantity, with a little white bread, it is still needful to enjoin the patient particularly to chew solid food thoroughly, and to eat slowly. We may now also permit a little wine, either pure or mixed with water, at meals. A little later, fish, oysters, and game may be allowed, as well as potatoes, green vegetables, stewed fruit, etc. Too careful a watch can never be kept upon the quality and especially the quantity of food taken. It is well to bear in mind constantly that a single indigestion may undo the good effect of many weeks of prudent and well-ordered feeding.

Tea and coffee, in small quantities, diluted with two-thirds or one-half of milk are allowed comparatively later. The mineral water of Bilin, or that of Vichy, will serve for some time to dilute the wine at the principal meals. From my experience I prefer the natural water of Bilin, in Bohemia, to that of Vichy.

When a patient has become profoundly anæmic in consequence of violent gastric hemorrhage, there is a strong temptation to soon replace the severely prudent regimen by a more analeptic diet—a fault which I have often seen committed. But the more cautiously an increased allowance of food is provided, the more surely is the patient strengthened.

The *hygiene* which accompanies the diet, ought to be conformed to the principles announced. There must be repose of both body and mind, and especially must the patient, after each meal, avoid every rapid or violent movement, as capable of provoking perforation, and must even avoid being too long in a carriage or railroad car. In regard to walking, fatigue must be avoided, and when the patient can resume his occupation he should do so slowly and gradually. It is very essential that a daily evacuation from the bowels should be secured, while any great effort in the expulsion of the contents of the rectum should be avoided. There are also some periods in this disease in which the examination of the patient requires precautions on the part of the physician, in avoiding all strong pressure, and all deep or prolonged palpation.

II. *Symptomatic Treatment.—Pain* is often advantageously combated

by the diet and hygiene indicated. But often, also, we cannot dispense with sedatives; among which opium holds the first rank. The muriate of morphia may be given in doses of one centigramme internally, or better still, by subcutaneous injection. Sometimes suppositories of cacao butter, containing from one to one and a half centigrammes of morphia, may be used, or clysters of eighty to one hundred grammes of water, with ten to twenty drops of laudanum. These as well as the bimeconate of morphia are very soothing, but some patients bear better the extract of opium in doses of from one to three centigrammes several times a day. Other sedatives replace opium very imperfectly, because it acts in much smaller doses, and is equally efficient when combined with alterative medicines which are themselves efficacious against gastric catarrh, such as nitate of silver in doses of one to two centigrammes, and the subnitrate of bismuth in doses of half a gramme, or a gramme, or even more. Brinton recommends the compound kino powder, which contains fifteen parts of kino, four of cinnamon, and one of opium; of this from three to six decigrammes are given at a time. Locally, wet applications to the epigastrium often do much good, acting by cold, if frequently renewed; if left in place, as cataplasms. One or the other method is chosen according to the effect upon the patient. Sometimes large blisters, followed by endermic applications of morphia, give great relief, and in certain rebellious cases moxa, applied to the epigastrium, have done me good service.

Hæmatemesis demands, beside absolute rest, the internal use of small pieces of ice, and its external application to the epigastrium. Among hæmostatics I place in the first rank the aqueous extract of ergot, given every hour in doses of five centigrammes, with the addition of one centigramme of extract of opium. If this fails, the solution of sesquichloride of iron in five-drop doses, given with a mucilaginous liquid, is indicated. In other cases we may succeed better with tannin, in doses of two or three decigrammes, or with acetate of lead, three to five centigrammes, with a little extract of opium added. Alum whey is a good adjuvant draught. If the patient, exhausted by hemorrhage, is in imminent danger of death, generous wines should be employed, with large doses of musk, camphor, etc. When he can again take nourishment, iced milk is much the best food, while nutritive enemata of milk and yelk of eggs, or those of Leube, containing meat hashed fine with sweetbread, may render good service. Enemata of the Nestle's lacteous farina, in doses of twenty grammes to one hundred of water, repeated three times a day, may become very useful. Iced solutions of this powder may also take the place of milk very well, when this cannot be had of good quality.

If the patient gets better, and his appetite returns, we must oppose too prompt a recourse to restorative diet, the consequence being often a renewal of pain and dyspepsia. It is the same with iron, the too early use of which does harm. We should begin with a mild preparation, such as the tincture of the malate of iron, in doses of ten to twenty drops, or the lactate or citrate of iron, and pass but very gradually to average doses and to the stronger preparations. I often order an effervescent ferruginous powder, made up of three grammes of the lactate of iron, nine of tartaric acid, and twelve grammes of bicarbonate of sodium; to be kept in a wide-mouthed bottle, well stoppered, to avoid moisture. The patient takes from a half to one teaspoonful in a small wineglassful of water two or three times a day.

Dyspepsia.—The mistake of attempting to aid digestion by the help

of stimulants and a tonic regimen is often committed. On the contrary, by lessening the quantity and regulating the quality of the food, when dyspepsia is aggravated, much better results will be obtained. And here is applicable all that I have said about diet and hygiene. Excessive acidity of the secretions, or acid fermentation, often form an obstacle to digestion, and in such cases, whatever may be said, the alkalies and especially the bicarbonate of sodium in doses of from one to four grammes are indispensable. The alkaline salts of magnesium, of calcium, and of ammonium, are less well borne in the end, whilst the bicarbonate of sodium, if of good quality, can be taken for quite a long time. An effervescent powder made with bismuth has often done me good service in such cases. It is for these patients, too, that I prefer the natural water of Bilin, as a beverage at meals, to other mineral waters of the same kind, as Vichy, St. Galmier, Vals, etc. The water of Bilin is as alkaline as that of Vichy, and has in it, beside, fixed carbonic acid and sulphates. It can be drunk pure or mixed with a little Bordeaux wine. If the bicarbonates neutralize the acids of the stomach too strongly, the tincture of rhubarb, the extract of orange-peel, the tincture of nux vomica, the bitter tincture,[1] or that of quassia or calumba, modify the secretions advantageously, and act in a more lasting manner, so that it is well to begin their use after that of the alkalies, as soon as the stomach is able to bear them, which is much facilitated by giving them at first in small doses. Among the mineral water cures used methodically, I know none better than that of Carlsbad. When patients are much weakened I make them go through a course of two or three weeks at Franzensbad, after a season of four or five weeks at Carlsbad, where they must take the waters in small quantities, one to three hundred grammes daily, avoiding also the wells that are too warm, and looking upon a temperature of 50° C. as the highest admissible. Some patients even derive more benefit from the use of Carlsbad water when cooled than when at its natural heat. Anti-fermentatives, such as creosote, hyposulphite of sodium, etc., have been highly praised, but have never done me much service in this disease.

Vomiting.—Diet is the best anti-emetic, and here iced milk, aided from time to time by swallowing small pieces of pure ice, may be very useful. Effervescent powders with the subnitrate of bismuth, a mixture of bicarbonate of sodium with lemon-juice, iced soda-water, and saturated alkaline solutions prepared with lemon-juice, with the addition of small doses of laudanum, are the best medicinal remedies in such cases. Pills of opium, each containing one centigramme of the extract, sometimes answer best, three or four being used in the twenty-four hours. Often the iodide of potassium has seemed to me to stop the vomiting better than any other means. I cause four grammes of it to be dissolved in thirty-two grammes of bitter tincture, of which ten to twenty drops are to be taken three times a day.

Constipation ought not to be permitted. Cold enemata and artificial Carlsbad water are good preventives. I often direct two or three pills, each containing five centigrammes of aloes, to be taken before going to bed. The powder or extract of rhubarb suits better for some patients. The essential point is to regulate the stools, without provoking either

[1] [The author doubtless alludes to the tinctura amara of the G. P. which consists of Fructus Aurantii Immaturi, Herbæ Centaurii, Radicis Gentianæ, āā partes ii, Rhizomatis Zedoariæ, pars i, Spiritus diluti, partes xxxv.—TRANSLATOR.]

numerous or liquid evacuations. One stool of moderate consistence daily is enough.

The occurrence of *perforation* demands the most absolute rest and the use of opium in full doses—three to six centigrammes of the extract at first every hour, and afterwards every two hours. The treatment may further be aided by the use of subcutaneous injections of morphia. Absolute diet should be enjoined, and ice may be applied to the epigastrium. This mode of treatment has already proved successful in some cases of this accident, which is one of the most dangerous to which man is subject.

Constriction of the pylorus is usually beyond the resources of art, even the best chosen diet furnishing only unsatisfactory results. In such desperate cases resort may be had to the expedient of washing out the stomach, after the method of Kussmaul.

In conclusion, the patient's *general condition* merits all our attention during the long course of the disease. Primary chlorotic anæmia, as well as that which follows upon a severe gastric hemorrhage, demands the circumspect and prudent use of the ferruginous and bitter preparations already mentioned. The effervescent ferruginous powder is a remedy which I am fond of employing under such circumstances. In summer, the air of the country, of the mountains, or of the sea-shore, may aid the action of an analeptic regimen and of tonics. It is essential also that, even when a cure has been obtained, the patient's diet and hygiene should still be watched over with the greatest care for years.

Discussion on Dr. Lebert's Paper.

After the reading of the preceding paper, Dr. W. Scott, of Cleveland, Ohio, said:—I have listened with great pleasure to this paper, which treats of the method of managing a very troublesome disease. I have always treated gastric ulcer in the manner described by Dr. Lebert. The primary condition to be observed is to keep the organ completely at rest, and to control pain by the use of opium or morphia. I have treated a case of this kind for thirty-one days without having anything nutritious in the stomach during that time.

Dr. H. P. Yeomans, of Canada, said:—I have found, when treating cases in the first stage, when there is much vomiting, that nitrate of silver in pilular form is of service. It may be given in small doses, combined with the compound extract of gentian. I have always found it successful.

Dr. C. J. Hare, of London, said:—Great difficulty is experienced in getting the stomach to retain anything during the course of treatment. I have found that milk, boiled for ten or fifteen minutes, and then skimmed, can be borne by the stomach very often when nothing else can. I think that the language employed in this and many other papers is adapted to create the impression that the disease is more frequently met with than is actually the case. Physicians ought always to be sure that the disease is really present before they begin any particular mode of treatment.

The President, Dr. Alfred Stillé, of Philadelphia, said:—I think the disease does occur frequently. While spending the summer at Lake George, N. Y., at a place where there is scarcely a village population, I have met with no less than four cases among the resident population. I am surprised that the condensed milk used in this country has not been, as far as Prof. Lebert's paper indicates, introduced on the continent of Europe. Condensed milk forms one of the best substitutes for fresh cow's milk that can be found. A gentleman skilled in infantile diseases has told me that he has been able to use it when fresh cow's milk was not tolerated, prepare it as he might. I think it rather

objectionable on account of the quantity of sugar usually employed to preserve it; but it has seemed to answer better than any other form of milk—even than boiled milk. In regard to food, I think that a good deal depends upon its temperature. I have known cases in which no other food than that rendered cold by ice could be retained and digested. A German gentleman, long resident in this country, had been examined by several eminent physicians who were unable to determine exactly what was the nature of his disease, but were inclined to believe it to be cancer of the stomach. After a careful investigation of the case, I concluded that it was one of simple gastric ulcer, and placed the patient under a strict dietetic treatment, when his vomiting ceased, and he gained flesh rapidly, and was able to take long walks and attend to his business as a merchant. He, however, relapsed, and I began to doubt whether I had been right in my diagnosis. The patient then inquired if a voyage would injure him, and asked whether I would object to his consulting several medical gentlemen in Germany. I advised him to consult certain eminent physicians, and wrote out for them a history of his case. He accordingly visited one or more of these physicians, who also regarded the case as one of simple ulcer, but whose treatment was not followed by marked advantage. Another, however, advised food made cold by ice, and no other remedies. The patient immediately began to improve, and returned home, after an absence of four months, having gained fifteen pounds of flesh. This confirmed me in my original opinion as to the nature of the disease. Soon afterwards the gentleman was attacked with hemorrhage of the lungs, and ultimately died of chronic phthisis. After death the lung was found to be universally tuberculous. In the stomach there was no active lesion, but in the greater curvature, and on the posterior wall of the organ, was a cicatrix, consisting of a white spot entirely denuded of the superficial layers of the lining membrane, from which radiated white bands of a fibrous aspect.

Dr. Michael O'Hara, of Philadelphia, said:—In a case of gastric ulcer attended with hemorrhage, I at first used the ordinary treatment with opium and bismuth. The latter drug was not of much service, but I found oxide of silver beneficial. The patient recovered, and some time afterward was again attacked with hemorrhage and recovered a second time under the use of opium and nitrate of silver. Not long since the same patient has passed successfully through an attack of typhoid fever.

Dr. Henry Gibbons, of San Francisco, said:—In the case of a child under my care, fresh and skimmed milk were tried unsuccessfully, and nothing but condensed milk was retained by the stomach. In another case nothing but milk warm from the cow could be tolerated.

Dr. J. W. Smith, of Charles City, Iowa, said:—I recall a case in which nothing could be retained but shavings of ice cut with a plane. The patient recovered, and was able to attend to his business in six weeks.

Dr. Darius Mason, of Prairie du Chien, Wisconsin, said:—A good form of condensed milk is that made simply by the evaporation of its watery portions. The use of the granulated article can be avoided by the employment of this condensed milk.

CASES OF PERNICIOUS PROGRESSIVE ANÆMIA,

WITH

SOME OBSERVATIONS UPON THE ANTE- AND POST-MORTEM CONDITIONS OBSERVED IN THAT AFFECTION.

BY

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Before such an audience as the present, and especially in this city, it is not necessary to make many introductory remarks upon the meaning of the terms "idiopathic anæmia" and "progressive pernicious anæmia." Englishmen are aware that, in 1855, Addison, of Guy's Hospital, applied the term "idiopathic anæmia" to a form of "general anæmia occurring without any discernible cause whatever," and "with scarcely a single exception proving fatal." Germans need not be informed that Biermer, of Zurich, in 1872, introduced the name "progressive pernicious anæmia" for the same class of cases, of which he had seen fifteen. And Americans are well aware that, in 1875, Dr. Pepper, of the city to which belongs the honor of entertaining the first International Medical Congress held in America, gave an able *resumé* of what was known on the subject down to the month of October last, together with the history of three cases which he had himself observed.

As the subject is of much importance and interest at the present time, I beg to invite the attention of the Section to four cases of the disease that have crossed my own path, and to some observations upon the *ante-mortem* conditions which have been observed in sixty-two cases, and the *post-mortem* changes found in fifty-one examples of pernicious progressive anæmia.

Case I.—J. H., aged 41, a shipping merchant, known to me for twenty years as of light complexion, average nutrition, and strictly temperate habits; has never had bad health till within two years, during which he has had at times uneasiness after eating, sore mouth, and a gradual loss of strength and color. Residence at the seaside for the last two months has not benefited him; has lost a sister by consumption, but his parents and the rest of his family are healthy.

On September 4, 1874, first consults me; walks in as if tired, and devoid of energy and spirit; complexion sallow, white, different from the hue of jaundice; conjunctivæ and nails colorless; mucous membrane of mouth and tongue very pale, with slight abrasions here and there. Careful examination reveals no disease in thorax or abdomen; liver seems to extend a finger's-breadth below margin of ribs; appetite poor; sense of oppression felt after eating; bowels act every day or two; urine not jaundiced, free from albumen; has not lost blood from stomach, nose, or rectum. Malignant disease was suspected; a careful diet, and pepsin with chlorohydric acid, ordered.

Seen a fortnight later in consultation with Dr. Sutherland, but nothing new was elicited. Patient had been restless at night, and had rather rapidly lost

strength; could no longer walk out, and the ankles had become œdematous in the evenings.

October 16, Dr. Campbell added to the consultation; no additional facts ascertained; agreed that internal carcinoma probably existed; claret and raw beef to be tried. Under date Nov. 1, it is recorded that patient only gets out of bed to have it made; is with difficulty induced to take beef-tea and oysters, which are his only food; sleeplessness persists; slight œdema of scrotum; loud systolic basic murmur; external jugulars pulsate visibly, and fill up from below; a dull, lemon-colored hue of skin; no patches of discoloration in axillary, inguinal, or genital regions; many moles on right side of neck; urine copious, not albuminous; blood pale; no excess of white corpuscles; has not been able to take iron in any form, but has persevered with the pepsin and acid. On November 16, signs of effusion into both pleuræ were detected; these increased, and he died chiefly by apnœa on November 22.

Autopsy.—Drs. Campbell, Sutherland, Ross, and Chipman, present with me. A pint and a half of clear, citron-colored serum in either pleura, and about six fluidounces in the pericardium; both lungs healthy, but extremely bloodless, except at posterior aspect; their bases collapsed; no coagula in venæ cavæ or tricuspid orifice; the latter large, admits three fingers; all the valves healthy. A few fluidounces of serum in the peritoneum; the liver, kidneys, suprarenal capsules, and pancreas, normal; spleen also, and of usual size; gallbladder moderately distended with bile; mucous membrane of stomach exhibits no trace of disease; pylorus and entire intestinal tract in normal state. All the organs, however, very pale and bloodless. Many of mesenteric glands rather enlarged, several of them to the size of a wild strawberry. I regret that as the viscera were not examined microscopically, the existence of fatty degeneration cannot be affirmed. The diagnosis of "cancer" or "latent ulcer of the stomach," was at fault; profound anæmia alone was found to account for death; and the case recalled Addison's description of "idiopathic anæmia."

Case II.—T. W., aged 37, a wholesale dealer in fruit, first consulted me on April 22, 1872, on account of breathlessness and palpitation when walking, and a gradual loss of strength. Sixteen years ago he had a single attack of ague for a few weeks; the only one he ever had. He had resided in Toronto for ten years, and in Montreal for the last eight years; and since then his complexion has gradually become pale, and a former tendency to short attacks of diarrhœa from slight causes has increased. Has not been dyspeptic; has never lost blood beyond a few drops from the nose a couple of times a year when suffering from a cold. For the last twelve months his complexion has been very pale.

He is an only son, and has had but one sister, a healthy woman. His father died suddenly of heart disease, at twenty-seven years of age, and his mother of some bowel affection; she had been subject to rheumatism. Patient is a large-framed, well-nourished man, but the subject of extreme pallor of the general surface, lips, and lining of the mouth, without œdema of ankles or eyelids. The pulse is small, soft, and regular; a soft systolic murmur, audible at mid-sternum, is propagated up the pulmonary artery and aorta; a continuous hum is heard in the jugular vein, and along the right border of the upper sternal region. The limits of the liver are normal; those of the spleen perhaps slightly increased. The urine is free from albumen; the appetite indifferent. A generous diet, claret, and a combination of iron and quinine were prescribed; and in June he was sent to the seaside, with instructions to persevere with the above measures. He was very much benefited, and did not revisit me till the February following (1873), when he presented the original symptoms of profound anæmia more intensely than before. A drop of blood from the finger-point was watery-looking and of a very pale red color, but contained no excess of white corpuscles.

A careful examination in conjunction with Dr. Campbell failed to discover any disease in lungs, heart, liver, spleen, or kidneys; nor had the patient

been suffering of late from diarrhœa or dyspepsia, although his appetite had been failing for some time. We agreed that the case was probably one of "idiopathic anæmia," like the previous one which we had seen together. All the various remedies, including iron, quinine, pepsin, etc., except phosphorus, that could be rationally prescribed, failed to meet the increasing pallor and failing strength. Food was given by mouth and rectum. The pulse grew smaller and weaker; faintness followed sitting up in bed; the appetite completely failed—indeed, food was loathed; the bowels became very torpid; epistaxis, sometimes severe, became a frequent symptom; a moderate elevation of temperature was noticed many times; his spirits sank, and towards the last a feeble delirium and childishness were observed now and then; œdema appeared in the legs, and death by exhaustion took place on April 22, exactly a year from his first interview with me. No autopsy could be obtained, so that the diagnosis of "idiopathic anæmia" wants that confirmation. No other view of the case, however, I submit, appears more rational, neither "Hodgkin's disease" nor latent carcinoma. This defect does not exist in my *third* case.

Case III.—Mr. T., aged 53, lost his wife very suddenly in her first confinement, about five years ago. Has been known to me for over twenty years as a temperate and healthy but not robust-looking man, which his occupation as a book-keeper may perhaps account for. He consulted me first in August, 1872, for a slight diarrhœa. He next came under observation in April, 1874, suffering from dyspepsia, which was benefited by pepsine and muriatic acid. On August 31, he still complained of epigastric uneasiness after eating, and looked more delicate, paler, and more spare than usual. No local disease in the abdomen could be discovered however, and nux vomica and cod-liver oil were advised in addition to the pepsine mixture. He did not return till Jan. 30, 1875, when he looked much paler and very sallow, and complained chiefly of weakness, so that he had of late driven to his office. A few days before he had passed blood at stool, but the hemorrhage did not recur. The anæmic looking surface was of natural temperature; the pulse 72; no enlargement of spleen, liver, or external lymphatic glands could be detected. The urine, free from albumen, deposited uric-acid crystals freely, and had a sp. gr. of 1030; no discolored patches on any part of body.

In the latter part of February diarrhœa occurred for a few days; patient's weakness and pallor were rapidly increasing; a soft systolic basic murmur was noted, and a drop of blood from the finger was pale and watery. The microscope showed no excess of white corpuscles; nine or ten only could be counted in the field, and they looked smaller than usual. The surface was very pallid and of a somewhat light straw color. In addition to pepsine at his meals, a pill containing reduced iron and nux vomica was prescribed, with beef-tea, claret, etc.

March 4. Urine still the color of pale sherry, and sp. gr. 1020. Deposits uric acid; is free from albumen; feet and legs œdematous. To have in addition to the ferrum redactum, $\frac{1}{50}$ gr. phosphorus (Warner's pills) after every meal.

He had been seen by Dr. Campbell in consultation with me on March 2, and no organic disease could be detected to account for the serious anæmia and prostration. Of course the case reminded us of our two previous examples of "Idiopathic anæmia," and as such I told the friends I regarded it. After this the downward course was rapid. Soon vomiting and a tendency to diarrhœa appeared. The appetite failed; the pulse grew gradually more frequent, 78, 84, 90, 102, and became weak and jerking, though regular; a constant "beating" in the head was complained of; faintness was experienced on sitting up in bed; and a mild delirium at night was followed by childishness and want of purpose in the day. The eyelids became somewhat puffy; the urine retained its high density and color, although the temperature of the body did not exceed the normal till a few days before death; fewer white corpuscles were found in the blood on March 18, three in one field and one in another;

the red corpuscles were of course deficient, but no attempt was made to estimate their number. The stain made by a drop of blood upon a white handkerchief was very pale. The voice grew feeble; the body wasted somewhat; the bloodless appearance of skin, mucous membrane, and nails, increased; and, during the last three days of life, a progressive stupor, dilatation of the pupils, slackening of the pulse-rate, retention of urine, and labored respiration, indicated a suspension of the cerebral functions. At this time the temperature of the body became somewhat raised. He died on March 28, 1875.

Autopsy,[1] twenty-seven hours after death; moderate cadaveric rigidity; some subcutaneous fat; no blood escaped while opening cavities.

Thorax.—About two fluidounces of clear citron-colored serum in left pleural cavity. Some bridles of adhesion over apex of left lung, in which are found two or three dry, black and white caseo-calcareous masses, almost the size of a small bean, and surrounded by a capsule of pigmented and indurated tissue. The right pleural cavity obliterated by very firm adhesions; in the extreme apex of right lung a patch of black indurated tissue about an inch in diameter, and a separate small nodule of mortar-like material. The rest of both lungs healthy, but œdematous, and very anæmic looking. Bronchial glands not enlarged. Pericardium contains about four fluidounces of serum; a good deal of fat on right side of heart; scarcely any blood in cavities of heart; very small colorless coagula in both ventricles; valves normal.

Abdomen.—Liver normal; half a fluidounce of green bile in gall-bladder; spleen very small, firm, and its capsule shrivelled; cut surface pale and smooth; mesenteric and lumbar glands unusually small; stomach and intestines perfectly healthy, but their coats very pale; supra-renal capsules and kidneys normal; all these organs contain very little blood.

The spleen, a portion of the heart, and blood from the splenic vein and from the inferior vena cava were taken away for microscopic examination by Dr. Osler, to whom I am indebted for the following report, as well as for other valuable assistance in the preparation of this paper:—

"*Blood* from the inferior cava and the splenic vein: The colorless corpuscles did not appear in any excess, averaging in a number of observations from six to eight in the field of No. 7, ocular 3. They presented slight variations in size, and the protoplasm was rather more granular than normal. Clusters of from six to ten adhering together were not uncommon. Nuclei variable; some had only one, others two or three. In the blood from the splenic vein the colorless corpuscles presented the same characters; a few retained their amœboid movements when placed upon the warm stage. Max Schultze's granular masses were abundant, and some of large size; the individual elements composing these masses were very distinct. The red corpuscles looked natural, a few only were crenated; a somewhat unusual variation in size was noticed among them. Many measured scarcely the $\frac{1}{5000}$ of an inch in diameter as seen with the No. 9 (immersion).

"*Spleen.* Weight one ounce and five drachms. Portions teased in saline solution and examined with the No. 9 (immersion) showed the ordinary structures met with in the organ (pulp cells, blood corpuscles, and connective tissue). The elements of the latter, as might be expected from the diminished bulk of the organ, were in excess, and consisted of the fibrous tissue of the trabeculæ and numerous nucleated fibre cells. Examination of their sections yielded the same results; the elements of the organ appeared normal, there being simply a disproportion between the connective tissue and the pulp.

"*Heart.* A small portion of muscle from the left ventricle showed marked signs of fatty degeneration."

I regret that the bones were not examined in either of these cases, but Dr. Pepper's paper had not been published when the last of them termi-

[1] My friend Dr. Osler kindly assisted me at the autopsy; Drs. Campbell and Chipman were also present.

nated. In the second autopsy a condition was found that, as far as I am aware, has not been hitherto observed in this disease, viz., marked atrophy of the spleen and great reduction in size of the mesenteric and prevertebral lymphatic glands. At first, I thought that this condition might account for the oligæmia in that case, but the subject of hæmatopoiesis in the adult is so unsettled that I do not venture to express an opinion as to the relationship existing between the anæmia and the state of the organs in question, although they are generally supposed to be concerned in the elaboration of the blood. A symptom observed in this last case that interested me much during life was the high color and density of the urine. It did not resemble the urine of ordinary anæmia, but rather that of rheumatic fever; and I am disposed to refer it to an excessive destruction of the red globules, as neither fever nor rapid wasting were present.

A fourth example of this disease came recently under my notice, in consultation with Dr. Greene, of Granby, to whom I am indebted for a report of the case.

Case IV.—Mrs. W., ætat. 46, a highly respectable married woman, the mother of four children, of whom two survive, and are robust and plethoric. In early married life she had had a few slight attacks of epistaxis, but had enjoyed tolerably good health till within about two years of the menopause, during which period menorrhagia existed. Then her health began to fail, and she became somewhat anæmic. No cause could be found in the condition of the organs for the hemorrhagic tendency. When first visited by Dr. Greene, nearly two years after (in March, 1875), she presented all the symptoms of intense anæmia, including the blood-murmurs in the great vessels. The only evidence of local disease discoverable was some fulness and tenderness of the liver, irritability of the stomach, and a yellow tinge of the conjunctiva. Although the digestive disturbance disappeared and she was able to enjoy carriage exercise during the summer, and had a very good appetite, the anæmia increased, and she had two or three slight attacks of epistaxis. In October the gastric symptoms returned with increased severity, and an aphthous condition of the mouth and fauces, which she had had more than once before, recurred, and was attended with slight diarrhœa, with a daily augmentation of the temperature, which could not be controlled by quinine or salicine. When seen by me last winter, in consultation with Dr. G., she was unable to leave her bed, and exhibited all the symptoms of advanced anæmia. A careful examination of all the organs, including the uterus, rectum, external glands, spleen, liver, etc., failed to detect local disease, and I expressed the opinion that it was a case of "progressive pernicious anæmia." A drop of blood taken from the finger-point was thin and pale in color; but was so spoiled by drying when I got back to Montreal that Dr. Osler could not satisfactorily make out its characters. The patient died on January 17, from exhaustion, preserving her mental faculties to the last. She had had two or three very moderate losses of blood from the nose since she had been seen by me.

Although satisfied in my own mind that the case just related was one of pernicious anæmia, yet as the diagnosis lacks the verification of an examination of the blood during life, and of the body after death, I would not have related it but that the family history appears to me important as proving that a predisposition to anæmia may exhibit itself in families and that it may perhaps be inherited.

This last patient's youngest sister has been anæmic since childhood; a married cousin has been anæmic since having an attack of typhoid fever about two years ago; and a grand-uncle died at sixty-two, with symptoms like those manifested by the niece, and was anæmic. Judging

from the papers which have been of late written upon this subject, it is not generally known that Dr. Wilks,[1] of Guy's Hospital, in 1857, published a series of cases of the disease we are considering under the caption, "Cases of Idiopathic Fatty Degeneration." "This designation," he remarks, "is given to a class of cases in which an *excessive anæmia* and *debility* are the peculiar phenomena observed during life, and a fatty degeneration of all parts of the body, but especially of the heart, the most remarkable appearance found after death." And further on he says, "The following seven cases will afford examples of this remarkably fatal anæmia and fatty degeneration." Two years[2] later, when describing another case of "Idiopathic Anæmia," he applies the latter title to his former series of cases. What a notable anticipation of Gusserow's observations in 1871, and of Ponfick's in 1873. It is due also to Prof. Lebert to say that he was one of the first to call attention to our subject. In 1853 and 1854 he had met with six cases of what he called "Puerperal Chlorosis," only one of which, however, had terminated fatally; and in 1858, under the title "Essential Anæmia," he published the details of that fatal case and of another subsequently observed, together with the appearances found after death. His third fatal case, with autopsy, occurred in 1859.[3]

These series of cases of Dr. Wilks and Prof. Lebert are not mentioned in Dr. Pepper's able paper on the subject, nor are the single cases of fatal anæmia published by Dr. Combe[4] about 1823, by Sir Dominick Corrigan in 1844, by Dr. Dickinson in 1863, and by Dr. King in 1871; nor Dr. Barclay's two examples published in 1851.

I have met with ten alleged examples of pernicious anæmia recorded since the appearance of Dr. Pepper's article: the first by Dr. F. Fedé[5] in October, 1875, however, appears to me to have been an example of so called "Myelogenous Pseudo-leukæmia," as the spleen was found after death of threefold the normal size, and its firm pulp "consisted of somewhat larger cells than in the natural condition, and they appeared very similar to the elements of the bone-marrow." The bones of the skeleton were thickened, the compact substance much reduced and replaced by a wide meshed spongiosa, in which the microscope found similar structures to the normal red marrow of bones; there were, however, numerous multinuclear (growing) cells and blood-corpuscle-containing elements. The liver, also, was a little enlarged; the blood contained no excess of white corpuscles.

In December, 1875, Dr. Pye-Smith[6] published two cases of "Pernicious Idiopathic Anæmia," with autopsies. Both occurred in men; no cause could be assigned for the disease in one; in the other, possibly, one or two attacks of intermittent fever experienced seven years previously may have stood in that relationship. The only morbid appearances discovered at the examination of the cadavera were fatty degeneration of the heart in both, of the liver in one, and thickening of the mucous membrane of the stomach in one. The bones were not examined.

This year Dr. Scheby-Buch[7] has recorded two cases of "Essential Anæ-

[1] Guy's Hospital Reports, 3d series, vol iii. pp. 203–213.
[2] Ib., vol. v. p. 108.
[3] Archives Générales de Médecine, Avril, 1876.
[4] Quoted by Dr. Dickinson, Trans. Path. Soc., xiv. p. 141.
[5] Quoted in Centralblatt f. d. Medic. Wissenschaften, Oct. 16, 1875.
[6] Virchow's Archiv, lxv., Dec. 27, 1875.
[7] Deutsches Archiv für Klin. Med., xvii. Band. p. 467, 1876.

mia." It is open to question whether either deserves the title of "Essential," as the man, aged 48, had had "ulcers on his legs which discharged from Easter to Christmas," and the woman, aged 60, had been "sickly for several years." The spleen was found enlarged in both. In the female there existed, in addition, dilated bronchi, fatty degeneration of the viscera, and "numerous cells resembling white corpuscles, in groups," in the marrow of one radius. The marrow was pale-red, and contained but "few fat cells," and only a few red globules. (Perhaps these cases ought to be regarded as examples of pseudo-leukæmia. I have retained them however.)

In the number of the Centralblatt for June 24, 1876, there is a notice of three cases of this disease recorded by Krieg,[1] and an autopsy. Only a very meagre report is given, and I have not been able to procure the original paper. Two of the three patients were men, aged respectively 52 and 45 years, and in good circumstances. No other details of these cases are given. The third case was that of a married peasant woman, aged 48. How many children she may have had is not stated. It is simply observed that she had been ordinarily stout till within a year. During that time petechiæ appeared upon the lower limbs, bleeding took place from the gums, and pleurisy set in. The autopsy revealed visceral hemorrhages, fatty degeneration of the heart's fibre, and a fibro-sarcoma at the base of the brain. Nothing is said about the condition of the bones.

An instance of this disease is related by Dr. E. H. Bradford, in one of the July numbers of the Boston Medical Journal for this year. The case is interesting, as affecting a girl only eleven years old, the earliest age at which the disease has been observed. No cause could be discovered. The blood globules were reckoned during life, and both red and white found to be numerically deficient. The red were paler than normal, and had lost their biconcave contour. "There was not much variation in size between the individual globules. All were smaller than usual. There was not much detritus. The white globules seemed abnormally granular, and were irregular in shape."

Amongst the peculiar symptoms mentioned, are severe pains in the ear and abdomen, and severe attacks of pain in different parts of the body; a daily expectoration of blood to the extent of about three ounces, and upon one occasion equal to a pint. The temperature during the three weeks before death, ranged from $101\frac{1}{5}$° F. to $99\frac{4}{5}$° F. Death was preceded for a day or two by loss of consciousness, a firmly contracted and flexed condition of the right arm, and a motionless state of the right leg. No disease of the spleen, lymphatic glands, lungs, or other organ could be detected during life. No autopsy was made.

The latest reported case that has come under my notice, was brought before the Société des Hôpitaux, on July 14, 1876,[2] by M. Lepine. The anæmia began with the third pregnancy of a delicate woman, who, however, had never been the subject of any serious disease, although she had many times suffered from gastric troubles. A careful post-mortem examination discovered only a circumscribed fatty degeneration of the heart, and an insignificant unilateral broncho-pneumonia. No disease of the spleen, glands, or bone-marrow, existed.

I have not tabulated Ponfick's, Grofner's, Quincke's, or Grenville's

[1] In Würtemb. Corr. Bl., 1875, No. 39.
[2] Vide Bulletin Générale de Therapeutique, 30 Juillet, 1876, pp. 85–87.

cases, not having been able to procure their details. Zenker's case of anæmia due to uterine hemorrhage, and Chadwick's case arising apparently from post-partum hemorrhage, I have also not tabulated, as they are at least not typical cases. For a like reason, I have excluded a case of extreme anæmia which occurred under my own care in a puerperal patient without undue hemorrhage or other evident cause to account for the profound anæmia; from which, however, she recovered after a long struggle. Perhaps all cases of recovery from apparently pernicious anæmia, should, in the present state of our knowledge, be excluded, while they should be very carefully studied in the hope that they may shed light on this obscure department of pathology.

An analysis of these cases of pernicious anæmia suggests the propriety of dividing them into distinct classes or groups, and even it may be of refusing to some of these the title of "idiopathic":—

Group I.—Thus all the cases which occurred during *pregnancy* ought to stand by themselves, for while no sufficient reason can be assigned why in such females the usual moderate impoverishment of the blood, which obtains during gestation, should have attained such a development as to have proved fatal, yet the existence of so well characterized a constitutional state as that of gestation, as a condition common to so many of the examples of so-called pernicious anæmia, cannot well be regarded as accidental. To this class belong by far the largest proportion of reported cases. All of Gusserow's five cases were in pregnant females; several of Biermer's were; one of Lebert's fatal cases, and one of Wilks's were; Corazza's only case was that of a woman who had had four children in five years, and died soon after delivery, and the latest reported case, Lepine's, began with the third pregnancy of a middle-aged woman who had only suffered from attacks of gastric disturbance.

Group II.—Some examples of the disease followed so closely upon *parturition*, quite independently of undue losses of blood, that they should perhaps be called puerperal cases. One of Marshall Hall's cases, one of Barclay's, and some of Biermer's, appear to have been in some way connected with the puerperal condition. Indeed Lebert met with several examples of severe anæmia under conditions which induced him to draw special attention to an "acute puerperal chlorosis," and many authors have written upon the chlorosis of pregnancy (notably Beau, Andral and Gavarret, Regnault, Cazeanx, and Sée).

As by far the largest proportion of the recorded examples of fatal pernicious anæmia have occurred during pregnancy and soon after delivery, it would be important to be able to arrive at an explanation of the relationship between child-bearing and pernicious anæmia. This is perhaps not possible. Even if it be urged that frequent child-bearing and more or less prolonged lactation may be regarded as spoliative processes of no mean power, and that amongst the poor, in whom these influences are especially prevalent, there is very frequently an insufficient supply of generous food and pure air to meet the spoliation, and hence the impoverished blood, it is difficult to comprehend why it is that pernicious anæmia is after all so seldom met with, while the influences just mentioned are so prevailing. Moreover, fatal anæmia occurred in one of Gusserow's cases during the first pregnancy, a circumstance which, in the light of the preceding observations, renders it probable that the pregnant state *per se* predisposes to progressive blood deterioration, an idea somewhat borne out by the rapid course of the disease in all Gusserow's

cases, one of them lasting only five weeks, from first to last; two occupying but four to five months; a fourth even less time; and the last ending in the eighth month of gestation.

Group III.—Closely allied to the pernicious anæmia of pregnancy appears, on first thought, that common affection still styled *chlorosis*. The latter, like the former, is most frequent in females; it is very generally accompanied by disturbance of the uterine functions, and its causation is unknown. On second thought, however, there is the striking difference that chlorosis very seldom proves fatal, while the anæmia of pregnancy appears to do so frequently; the former is amenable to treatment, the latter rarely if ever is cured.

The only examples of fatal anæmia in *young* females independent of pregnancy that I have tabulated are three cases recorded by Marshall Hall; one by Wells in a married woman 31 years old; one by Immermann, in a patient also married and of the same age; one by King in a woman, aged 27, who had had since puberty more or less menorrhagia and leucorrhœa; one by Pepper, in a woman aged 36, who had been exposed to malaria and had had bronchitis for five months before the anæmia appeared, and Bradford's case of a girl only 11 years old. Of these eight cases, only two, one of Marshall Hall's, in a girl of 18, and Bradford's case, can be fairly cited as typical examples of chlorosis proving fatal, and so far resembling pernicious anæmia. Inasmuch, then, as the ordinary chlorosis of young women is notoriously very seldom fatal, while that less frequent form incident to pregnancy frequently is, we may not conclude without further light that both forms of anæmia acknowledge the same causation, although we may grant that both may be called "idiopathic" or "essential," until some satisfactory explanation of their origin is forthcoming.

Group IV.—Another group comprises those cases which have been preceded by *chronic diarrhœa*. Biermer states that pernicious anæmia frequently follows that affection; but, with the imperfect history given in very many of the published cases, his observation remains unproven. It is true that diarrhœa is frequently mentioned among the symptoms of the affection, but many authors fail to say whether the diarrhœa preceded the other evidences of the disease, or merely accompanied them. Indeed, beside the cases given by Biermer, the only others in which chronic diarrhœa is recorded amongst the antecedent conditions of the anæmia are: Corrigan's case, in which diarrhœa had existed "off and on for years;" one of mine, in which a tendency to that affection had been observed over eight years; and one of Pepper's, in which "chronic follicular catarrh of the intestines had existed for many years;" this patient had also "chronic suppuration of the gall-bladder," and a former history of psoriasis of twelve to fifteen years' duration. These facts, then, scarcely bear out Biermer's statement.

Diarrhœa formed a leading symptom in the following cases: two of Lebert's; three of Wilks's, in one of which there were cicatrices and ulcers in the colon; one of Barclay's; one of Pye-Smith's, and Lepine's case—in all eight. But it appears from the notes of all these cases that an impoverished condition of the blood coexisted; and also some one or more of the other conditions known to have preceded or accompanied pernicious anæmia in other cases, such as pregnancy, parturition, hemorrhage, etc.

Group V.—Six more of the tabulated cases may be grouped together, inasmuch as the antecedent conditions were such as to involve *blood-*

waste; they might have been included with the last group, which also implies blood-waste, had not previous writers attached so much importance to chronic diarrhœa *per se* in relation to pernicious anæmia. Such are Wilks's case of an errand boy, aged sixteen, who had not been well for three years, and had often suffered from epistaxis, and whose colon contained cicatrices and ulcers; King's case, which had been preceded during thirteen years by menorrhagia and leucorrhœa; my fourth case, in which for two years before the cessation of menstruation, there had been menorrhagia; one of Pepper's cases, preceded by chronic bronchitis for five months; and Buch's two cases—one sickly for years, and the subject of chronic bronchitis and an abnormal condition of the marrow of the radius; the other, a case of pernicious anæmia, closely following upon ulcers of the legs, which had discharged freely from Easter to Christmas.

Cases of fatal anæmia occurring in persons who have long suffered from repeated attacks of loss of blood, or from protracted leucorrhœa, or from chronic bronchitis, or from chronic ulcers, while they may be in one sense included in the title "progressive pernicious anæmia," cannot strictly be regarded as examples of "idiopathic anæmia" as delineated by Addison in his masterly description of the disease. He confined the term to "cases in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, no renal, splenic, miasmatic, glandular, strumous, or malignant disease."[1] This is a point which has been so much overlooked of late that the terms "idiopathic anæmia" and "pernicious progressive anæmia" are not now synonymous.

GROUP VI.—There is a group of six cases (one by each of the following writers: Broadbent, Pye-Smith, and Lebert, two by the author, and one quoted by Buch) occurring in patients from forty-one to fifty-seven years of age, in which no antecedent condition is mentioned sufficient to cause the anæmia except *dyspepsia.* In only three of these did the indigestion last a considerable time, and in one of them (a case of my own) it was not severe. In the other three cases the dyspepsia occurred rather among the early symptoms than among the antecedent conditions. It must be admitted that even severe and protracted dyspepsia very rarely induces a fatal anæmia; and I incline to regard the relationship in these cases as that of coincidence only.

GROUP VII.—*Extreme poverty* or *poor diet* were the conditions under which pernicious anæmia was observed in four men (Lebert, Wilks, Dickinson, and Cazenave, each a case), and if these were the real causes of the blood disease, such cases require little discussion; the anæmia is merely a consequence of inanition. But why this fatal form of anæmia is comparatively so rare, while poverty and poor diet are so frequent, we cannot say.

GROUP VIII.—A remarkable case is related by Pepper, in which a man, after having had *jaundice* "about a month," then exhibited well-marked anæmia which proved fatal. In Corazza's case severe icterus was also an antecedent condition, but the patient was also pregnant, and had had four children in five years.

GROUP IX.—Finally, there remain nine cases in which no antecedent condition is mentioned that can fairly be regarded as causative unless it be a case of Pye-Smith's, in which there may have been two slight attacks of intermittent fever, years before, "although on this point [the patient's] account was somewhat uncertain," and two of Wilks's, one, in which

[1] Addison's Works, Syd. Soc. Ed., p. 212.

"domestic trouble and recent marriage" coincided with the appearing of the anæmia; the other, that of a woman aged fifty who had been "thirteen years in a lunatic ward." Failing health and progressive anæmia are a summary of the conditions mentioned in the whole nine cases (Wilks three, Barclay, Combe, Pye-Smith, Lebert, Immermann, and Howard, each one[1]).

It results from this analysis that the conditions—as far as they are known—under which these cases of pernicious anæmia originated, may be thus grouped:—(1) pregnancy especially where oft repeated; (2) parturition (and these two conditions appear to be especially frequent); (3) chlorosis, which is rare; (4) chronic diarrhœa or intestinal catarrh, which is by no means as often antecedent as some authors state; (5) blood-waste, direct and indirect, from protracted menorrhagia and leucorrhœa, from chronic ulcers, from chronic bronchitis, and from repeated epistaxis combined with ulceration of the bowels, conditions present in six cases; (6) dyspepsia, observed in three cases; (7) extreme poverty or poor diet, mentioned in four cases only; (8) jaundice, the only known antecedent in one case, and associated with frequent child-bearing in another; and last (9) no antecedent condition mentioned other than "failing health," which must exist when anæmia is developing.

Reflecting upon the frequency with which these several conditions obtain, they cannot fairly be held to be frequently productive of, or antecedent to, a progressive pernicious anæmia, and some of them seem to be quite incapable of producing so grave a disease. The conclusion is almost inevitable that either an inherited or an acquired predisposition to defective development or regeneration of the blood must exist in these cases of pernicious anæmia, otherwise such conditions could not be followed by such profound and unmanageable alterations of the blood.

Passing from the *ante-mortem* to the *post-mortem* conditions, it cannot be said that Morbid Anatomy has thus far explained the occurrence or the nature of the disease.

1. The *fatty degeneration* of the heart and other viscera first specially insisted upon by Wilks in these cases of fatal anæmia, is a consequence, not a cause, of the blood impoverishment, and is observed also after severe hemorrhages, and in protracted fevers, in starvation, and other analogous conditions.

2. The *small size* "hypoplasia" of the *heart* and of *the large arteries*, pointed out by Virchow as existing in some severe cases of "chlorosis," and which may be regarded perhaps as congenital, even if it were proved to be constant in chlorosis, could not be anticipated in the other forms of anæmia included under the term pernicious. It is true that we have not many observations on this point, because the subject has not been generally investigated. However, Pye-Smith found "the aorta of proper size and its coats healthy" in his second case; its condition is not given in his first; in both the heart was over the average weight. Buch, after comparative measurements, did not find the aorta smaller in his two cases than in other anæmic and cachectic bodies, if not so wide as in healthy persons.

3. In fifty-one autopsies of progressive anæmia that I have collected, either the *spleen* is described as normal, or the "viscera" (and therefore the spleen) are said to have been normal, thirty-six (36) times; as more

[1] Not having been able to get Krieg's original paper, I cannot say anything of his cases.

or less enlarged, usually but slightly, thirteen (13) times; once (1) as small and shrivelled; and once (1) its condition is not stated (Krieg's case):—

Spleen normal	36
" enlarged	13
" small and shrivelled	1
Condition not stated	1
	51

Biermer appears to have found this organ free from lesions in his fifteen cases; and Gusserow, although he mentions slight enlargement of the spleen in his cases, says it presented no characteristic changes of its pulp. In one of Pepper's cases, the spleen was enlarged by one-half; "its pulp was dark and much softened," without leukæmic lesions. In the other, it was "slightly enlarged;" its pulp "softened and purplish, without any leukæmic patches or enlarged Malpighian corpuscles." "Under the microscope, the field was crowded with small round cells (smaller than the majority of the cells in the marrow), spindle-shaped cells, and very pale red blood globules." Much importance cannot be attached to the softness of the spleen in this case, as it had undergone sufficient decomposition to crepitate when handled. In one of Buch's cases, the spleen was moderately enlarged and of a pale red-brown color; in the other, it was also somewhat enlarged (12½ centimetres long, 6 broad. and 4 thick), of a brown-red color and firm, and its glomeruli evident; in Immermann's single autopsy, this organ was also slightly enlarged and hard. In the only instance in which the spleen was found atrophied, it weighed but thirteen drachms, felt tough and cut with resistance. Its cut surface was pale, and the microscope proved an absence of pulp cells and blood corpuscles and a preponderance of trabeculæ and of nucleated fibre cells.

It cannot then be affirmed that this important blood gland, the *spleen*, usually presents any abnormality, much less any special lesion in pernicious anæmia.[1]

4. A similar remark may be made respecting the *lymphatic glands*. In the fifty-one autopsies, their condition is not mentioned sixteen (16) times; they presented no lesions thirty-three (33) times (including Biermer's and Gusserow's 20 cases), the mesenteric glands were "pretty generally enlarged" in one case; and in the remaining one the same glands and the lumbar were unusually small, as if atrophied. The last was the case in which the spleen also was atrophied. In it also one gland only in the body, an axillary gland, was found slightly enlarged.

Condition of lymphatic glands not stated in	16
No lesions in	33
Mesenteric glands generally enlarged in	1
Mesenteric and lumbar glands unusually small in	1
	51

5. One other lesion of great importance remains to be noticed. Prof. Pepper, in 1875, on examining one radius of his last case of progressive anæmia found the marrow "to be made up almost entirely of granular cells," which he considered as due to the hyperplasia of the marrow with production of lymphoid cells—a change described by Ranvier in

[1] Disease of the spleen, no doubt, may be a cause of anæmia, just as disease of the other blood-glands and lungs may be.

1867,[1] and by Neumann and others subsequently, as existing in some cases of leucocythæmia. Dr. F. Fedé, about the same time as Pepper, described similar alterations of the marrow of the bone, in a case which he published as one of progressive pernicious anæmia, but which, as already stated, owing to the accompanying alterations in the spleen, is rather an example of pseudo-leukæmia. This year Dr. Scheby-Buch examined the radius in a case of pernicious anæmia, and found the marrow of a pale-red color, and containing a few fat cells, single drops of fat here and there, and numerous cells in groups resembling white corpuscles, and only a few red corpuscles. On the other hand, in the latest recorded case, M. Lepine found no affection of the bone-marrow. The bones were not examined in the other forty-eight cases, so that the frequency with which this little-known lesion may have existed in them cannot be even surmised.

The import and nature of the change itself are scarcely settled. It has been observed in several cases of leucocythæmia, and in a few of pseudo-leukæmia. Dr. Horatio C. Wood, Jr. (of Philadelphia),[2] examined the long bones taken from patients dead of various chronic diseases, and only in one instance found any abundance of leucocytes in the marrow, and in that one there coexisted marked general enlargement of the lymphatics, and of the spleen. Circumstances prevented a complete examination of the case, so that it may also have been one of leukæmia or pseudo-leukæmia. He thinks it not improbable that the lesion occurs in scrofulosis and in chronic or even acute pyæmia; and I am indebted to my friend, Dr. Osler, Professor of the Institutes of Medicine in McGill University, for the details of an autopsy which he recently made upon a man who died of tuberculous peritonitis, in which the marrow of the femur, the only bone examined, was found to have suffered these changes. Passing over the notice of the case and of the condition of the periosteum found after death, I will relate only his description of the bone-marrow.

"*Medulla.* Upper half of left thigh removed for examination. On section of the bone, the medulla presented a uniform grayish-red color, nowhere having the yellowish fat-like aspect of normal marrow. In the cells of the cancellated portion the marrow was of a lighter red color.

"*Red blood-corpuscles.* A very evident difference in size existed in these elements, very many of which measured $\frac{1}{4500}$ of an inch in diameter; a few were even smaller. Curious irregular forms were numerous, due to the protrusion of portions of the stroma of the corpuscle. Nothing special was noticed about their color or general characters.

"*Colorless corpuscles.* These elements outnumbered considerably the red corpuscles in all the specimens examined. Three or four varieties were noticed: (1) Corpuscles agreeing in general characters with those of the blood; protoplasm firmly granular, nucleus vesicular, variable in number, ranging from one to three or four. Many of these were larger than ordinary white blood-corpuscles, and the nuclei were evident without the addition of any reagents. (2) Somewhat smaller corpuscles with coarsely granular protoplasm and one or two vesicular nuclei. They were not very abundant, and corresponded closely with the coarsely granular corpuscles which are sometimes met with in the blood. (3) Corpuscles with clear vesicular bodies and solid nuclei, usually only one. These were not very numerous, three or four only occurring in the field of the No. 9 (immersion). (4) Small bodies like free nuclei, protoplasm finely granular. These correspond closely with the nuclei

[1] Vide Jaccoud, Nouveau Dict. de Méd. et de Chir., t. xx. p. 430.
[2] Am. Journ. Med. Sci., Oct. 1871, p. 376.

of the former variety, and are probably identical with them, having been freed by the rupture of the cell. Of these varieties No. 1 was by far the most abundant.

"*Transitional forms.* Nucleated, colored corpuscles were noticed in each specimen examined, as many as three or four occurring in the field of the No. 9. They ranged in size from $\frac{1}{3000}$ to $\frac{1}{2500}$ of an inch; the majority of them being slightly larger than the ordinary red corpuscle. The nucleus in every instance was single, and the protoplasm finely granular. Three corpuscles were seen with the nuclei dividing. The coloration of the cell substance was in each case very distinct, and only exceptionally was it less distinct than in the surrounding colored elements.

"*Cells containing red blood-corpuscles.* Two or three only were observed; in one case an ordinary white corpuscle contained a single red corpuscle and two nuclei; in another a somewhat irregular protoplasmic mass contained three red corpuscles and one distinct nucleus. None of the ordinary myeloid or giant cells were found."

In this case of tuberculous peritonitis, then, there existed the changes in the bone-marrow, which have been observed in so called myelogenous leukæmia, and in Pepper's case of pernicious anæmia.

It is not logical, in the present state of knowledge, to conclude that a condition of the marrow which has been found in so many different affections, as leukæmia, Hodgkin's disease, pernicious anæmia, and tuberculous peritonitis, is a pathological state essential to those affections. Moreover, quite recently, Dr. Moxon,[1] of Guy's Hospital, and more recently still, Dr. Schtschastny, of St. Petersburg,[2] have maintained that the bodies resembling white corpuscles found in the bone-medulla in leucocythæmia "were in reality leucocytes which had escaped from the blood." Even if it were true that the condition of the bone-marrow in question was constantly present in pernicious anæmia, the bearings of the fact upon the nature and origin of the disease would remain to be worked out, as physiologists have not altogether accepted the views of Neumann and Bizzozero, that in the adult the medulla of bones is concerned in the development of the white corpuscles of the blood (Flint, Virchow, and others). It is worthy of notice that the two cases of pernicious anæmia in which the medulla was found in a morbid state were complicated, and such as could scarcely be called by the original name of the disease, "idiopathic anæmia." Pepper's patient, aged fifty, had suffered from an obstinate form of psoriasis for twelve to fifteen years; from follicular catarrh of the intestines for twenty-five years; and from the want of nearly all his upper teeth for twelve years before his death, and would not wear false ones. The autopsy revealed not only changes in the medulla of the bones, but chronic suppuration of the gall-bladder, enlargement of the solitary glands of the intestines, and enlargement of the spleen. Scheby-Buch's patient had been sickly for several years, and the autopsy discovered, in addition to disease of the marrow, chronic inflammation and dilatation of the bronchi, and a somewhat enlarged and firm spleen.

I will make only one observation upon the diagnosis of the disease. Quite lately, Dr. H. Eichorst[3] has affirmed that the disease may be *certainly* diagnosed in the early stages from the microscopic characters of

[1] Lancet, May 4, 1876.
[2] Petersburger Medicin. Wochenschrift, No. 20, 17 Juli., 1876.
[3] Centralblatt, Berlin, Juni 24, 1876.

the blood. He says some of the red globules are of normal size, but very pale, and have lost their tendency to form rouleaux; others scarcely attain one-fourth the diameter of a normal perfect corpuscle, so that they look like drops of fat tinged red, and have lost their biconcave appearance. As the disease advances, these "red drops" or foreign elements increase in number; so that before death he has found them as numerous as the intact red corpuscles. The white corpuscles are always in small number.

The diagnostic character here insisted on, viz., the presence in the blood of red globules of much smaller size and paler color than natural, has not always been found according to the few accurate reports made in recent cases. The red globules were observed to be small and pale in one of Buch's cases. In Bradford's case, in which the blood was specially examined, while "there was not much variation in size between the individual globules, all were rather smaller than usual; there was not much detritus." The "white corpuscles seemed to be abnormally granular, and were irregular in shape." In one of my cases (Mr. T——) Prof. Osler noticed "a somewhat unusual variation in size amongst the red globules. Many of them scarcely measured $\frac{1}{5000}$th of an inch in diameter; the white also presented slight variations in size, and were more granular than normal, and Max Schultze's granular masses were abundant."

On the other hand, Prof. Pepper carefully examined the blood in one case, and makes no mention of this variation in size of the red globules, which he describes as "pale, and less biconcave than normal," and adds, "no abnormal elements were seen." And Pye-Smith reports in one case, three weeks before death, that "the colored elements of the blood are not evidently changed either in number or characteristics, but they form no rouleaux."

It should be borne in mind, too, that M. Laptschinsky, of St. Petersburg,[1] has described precisely similar alterations of the blood in various diseases in which marked febrile symptoms obtain, with the addition, however, of an increase in the number of white corpuscles.

It may be that the characters of the blood described by Eichorst are, when present, highly suggestive of pernicious anæmia, but they do not appear to be always present in that affection, and further investigation of the subject is needed before accepting that author's confident statement.

I must conclude this paper, already too long, without an examination of the relations existing between pernicious anæmia and Addison's disease, leukæmia, pseudo-leukæmia, and purpura hæmorrhagica, or of the part taken by the sympathetic system in the production of the blood-changes. Time will not permit of such an undertaking.

The conclusions that I venture to draw on this subject are:—

I. That all the various forms of anæmia, *i. e.*, forms as determined by the conditions under which they occur, may occasionally take on progressive and pernicious characters.

II. That such is not unfrequently the case with the anæmia of pregnancy and parturition, while the converse is true of chlorosis.

III. That it has not been proved that there is a distinct variety of

[1] Lancet, Oct. 31, 1874, p. 157.

anæmia, having an etiology and pathogeny peculiar to itself, and demanding the name "progressive pernicious."

IV. That neither the spleen nor the lymphatic glands usually present any, much less any special, lesion in pernicious anæmia.

V. That it remains to be proved that hyperplasia or other change of the bone-marrow, is a cause of anæmia.

VI. That if it be a cause, it has yet to be shown whether it is the cause of a variety that should be especially styled pernicious and progressive, and that at present the weight of evidence appears to be opposed to that view.

VII. That it is premature to regard pernicious anæmia as a myelogenous pseudo-leukæmia.

VIII. That while pernicious anæmia is perhaps rather more frequent in females than in males, the difference in the liability of the sexes is not very great. Omitting Biermer's 15 cases, the majority of which belonged to the female sex, there remain in my table 47, of which 20 were males, and 27 females.

IX. That with the view of obtaining information as to the nature and causation of the defective hæmatosis in all cases of anæmia, and especially fatal cases, it is necessary to obtain a careful history of the patient's previous health and that of his family, to investigate the progressive changes that take place in the blood and urine as the disease pursues its course, and to examine *post-mortem* the condition of the bones and their marrow, as well as of all the other organs of the body.

Table showing some Details of Sixty-two Cases of Pernicious Progressive Anæmia.

Author.	Date.	Cases.	Sex. M	Sex. F	Age.	Autopsy.	Viscera.	Spleen.	Glands.	Liver.	Heart.	Kidneys.	Blood.	Marrow of bones.	Previous condition of health. Remarks.	Duration.
Dr. Combe, Trans. Med.-Chir. Soc. Edinb.	ab't 1823	1	1	..	47	1	Healthy.	Normal	....	Normal	Pale—like flesh.	Normal	....	Not exam'd	Anæmic symptoms the first indication; hydrothorax.	9 months.
Marshall Hall, Dis. Females, p. 84.	1830	4	..	4	18	1	Healthy.	....	....	....	....	....	....	Not exam'd	The fatal case with autopsy occurred after confinement. The other three patients never were pregnant.	
Sir D. Corrigan, Dub. Med. Jour. p. 506.	1844	1	1	..	21	1	Healthy.	....	....	....	Softened.	....	....	Not exam'd	Diarrhœa off and on for a year.	
Dr. Barclay, Med. Times and Gaz. ii. 480.		2	1	..	34	1	Chalky mass in apex of lung.	Normal	Normal	Normal	Pale, healthy	Cyst in one.	....	Not exam'd	Ill twelve months; pain across loins and scanty urine; coma.	13 months.
	1851		..	1	40	1	Healthy.	Normal	Normal	Normal	Normal	Fatty.	....	Not exam'd	Several children; last, four months before death; sore month soon after confinement; no other previous history. Two months before death diarrhœa, which recurred; continued to nurse for three months.	4 months after confinement.
Dr. Wilks, Guy's Hospital Reports, 3d series, vols. iii. and v.	1857	7	..	1	31	1	Fatty.	Normal	Normal	Fatty.	Fatty.	Fatty.	No increase white corpus.	Not exam'd	No pregnancy; domestic trouble, and recent marriage. One year of failing health; no hemorrhage nor diarrhœa; a servant.	About 1 year.
	1857		1	..	52	1	Fatty.	Normal	Normal	Fatty.	Fatty.	Fatty.	Not stated.	Not exam'd	6 months poor diet; lupus in youth; rheumatic pains and sometimes headache; tanner.	Not given.
	1857		..	1	50	1	Fatty. "Semilunar ganglion healthy."	....	....	Fatty.	Fatty.	Fatty.	Not stated.	Not exam'd	13 months in lunatic ward; no pregnancy; tolerable health till last few months of life; no hemorrhage or diarrhœa.	Not given.
	1857		1	..	16	1	Cicatrices in colon and ulcers.	Normal	Normal	Fatty.	Fatty.	Fatty	No increase white corpus.	Not exam'd	No ague; ill three years; frequent epistaxis; of late diarrhœa and epistaxis both obstinate; an errand boy.	Ill 3 years.
	1857		..	1	31	1	Lungs, stomach, and intestines all healthy.	Normal	....	Fatty.	Fatty.	Fatty.	Not stated.	Not exam'd	No history, but that was very ill of bloody diarrhœa and occasional hæmatemesis during last few weeks of pregnancy, and violent epistaxis; never had ague; occupation not stated; died 7 weeks after delivery.	Not stated accurately.
	1857		..	1	30	1	Fatty.	Large but healthy.	....	Fatty.	Fatty.	Fatty.	Not stated.	Not exam'd	Good health previously; no cause assignable; gradual weakness before admission for four months, and two weeks before, blood per anum.	
	1859		..	1	46	1	Healthy.	Normal	....	Fatty					[illegible] health failing [illegible] years since catamenia [illegible]hœa.	3 years and 7 weeks ill.

Author.	Date.	Cases.	Sex. M	Sex. F	Age.	Autopsy.	Viscera.	Spleen.	Glands.	Liver.	[illegible]	[illegible]	[illegible]	[illegible] bones.	Previous condition [illegible] health. Remarks.	Duration.
Dr. H. Lebert, vide Arch. Générales, Avril, 1876, p. 400–412.	1858	3	..	1	24	1	Healthy.	Normal	....	Enlarged.	Pale-yellow.	Normal	Not stated.	Not exam'd	Good health before; latter half of second pregnancy became weak and pale; died 15 weeks after delivery.	About 9 months.
			1	..	45	1	Healthy.	Normal	....	Enl'gd, healthy not fatty.	....	Normal	Not stated.	Not exam'd	Poor and badly fed; good health till within a year; six months after, having become thin, and pale, and weak, diarrhœa set in.	14 months.
			..	1	55	1	Healthy.	Normal	....	Fatty.	Fatty.	Normal	Not stated.	Not exam'd	Passable health; hysterical; Oct. 1858, extreme weakness and other symptoms of anæmia; tendency to attacks of diarrhœa.	3½ months.
Dr. Dickinson, Trans. Path. Soc. xiv. 141.	1863	1	1	..	44	1	Healthy. Great sympathetic also natural.	Normal	Not mentioned.	Normal	Fatty.	Normal	No increase white corpus.	Not exam'd	Two years ill and very poor; at first only symptoms of garrulity and oddness of manner; later, anæmia; a butler out of employment.	Not stated.
Cited by Scheby-Buch, Wiener Krankenhans-berichten, 1858, p. 24.	1858	1	1	..	57	No	....	....	....	....	....	....	....	....	Two years headache and indigestion; healthy in youth; laborer; no autopsy; died comatose.	Not stated.
Cazenave, Journal de Méd. de Bordeaux, Février, 1860	1860	1	1	..	21	1	Normal.	....	....	....	....	....	....	Not exam'd	Lived under bad conditions; stone breaker.	
Dr. L. Corazza, quoted by Pepper, Meissner, Buch, and others.	1869	1	..	1	24	1	Not stated generally.	Slightly swollen.	....	Fatty.	Fatty.	Fatty.	....	Not exam'd	Never regained health after fever at 15; had 4 children in five years; in middle of last pregnancy had jaundice; confined in November, but remained feeble, suffering frequently with diarrhœa, dyspnœa, and somnolence before death. Died January 27.	
Dr. A. Gusserow, vide Biennial Retrospect, 1871–72, p. 397, and Arch. f. Gynäk. Heft 2, 1871.	1871	5	..	5	24 to 36	5	Normal, but fatty, some of them.	Slightly enlarged.	Normal	Fatty, several.	Fatty, several.	Fatty, several.	No increase white corpus.	Not exam'd	Good health; all pregnant; 4 had several children; 1 had only 1 child. Without apparent cause and slowly during pregnancy severe anæmia appeared, and towards eighth month fœtus was expelled, and each patient died soon.	From 5 months to 5 weeks.
Dr. King, Brit. Med. Jour. 1871, p. 613.	1871	1	..	1	27	1	Normal.	Normal.	Normal	Normal	Normal	Normal	No increase white corpus.	Not exam'd	Childless; subject to menorrhagia and leucorrhœa 13 years; always rather delicate. A year before admission began to have diarrhœa and vomiting, and to lose flesh for two or three months, then got better; but three months later symptoms returned for two months and again remitted. Died exhausted.	About 13 months.
Biermer, of Zurich, Correspondenzbl. für Schweizerische Aerzte, Jahrgang 2, 1872, No. 1, quoted in Med. Times and Gaz. Nov. 21, 1874, p. 581.	1872	15	..	..	18 to 52	15	Healthy, but fatty.	Normal.	Normal	Fatty.	Fatty.	Fatty.	No increase white. corpus.	Not exam'd	"Frequently followed chronic diarrhœa, and childbearing seemed especially to predispose to it." Majority women.	

TABLE SHOWING SOME DETAILS OF SIXTY-TWO CASES OF PERNICIOUS PROGRESSIVE ANÆMIA—*Continued.*

Author.	Date.	Cases.	Sex. M	Sex. F	Age.	Autopsy.	Viscera.	Spleen.	Glands.	Liver.	Heart.	Kidneys.	Blood	Marrow of bones.	Previous condition of health. Remarks.	Duration.
Immermann, Deutsches Arch. f. Klin. Med. 1874, xiii. 209.	1874	2	1	..	18	No	....	....	....	....	....	....	....	....	Healthy; shoemaker; no autopsy.	
			..	1	31	1	....	Slight enlargement and hardness.	Not stated.	Fatty.	Fatty.	Fatty.	No increase white corpus.	....	Healthy; married; number of children not mentioned.	
Dr. Broadbent, Practitioner, Jan. 1875.	1875	1	..	1	43	1	Healthy all except pancreas.	Normal.	Normal	Normal	Not exam'd	Not exam'd	White corpus. deficient.	Not exam'd	Good health till 4 months before admission. Indigestion and severe vomiting for two months; no hemorrhage; jaundice. Pancreas larger, firmer, and more vascular than usual; blood globules rather larger and thinner.	
Dr. Pepper, Am. Jour. Med. Sci., lxx. pp. 313–347.	1875	3	..	1	26	No	....	....	....	....	....	....	No increase white corpus.	....	A dressmaker; single; bronchitis 5 months; lived in malarious region, and had doubtful symptoms of ague. Six or seven months after recovery from bronchitis great weakness and irregular fever in afternoon and decided anæmia all present. Died comatose. No autopsy.	14 months from origin of bronchitis.
			1	..	57	1	Fatty.	Enlarged one-half.	Normal	Fatty.	Fatty.	Fatty.	No incr. white corpus.	Not exam'd	Good health; slight sunstroke in 1871; jaundice in March, 1872, and when it disappeared a month later anæmia found present.	
			1	..	50	1	....	Slightly enlarged.	Normal	Fatty.	Fatty.	Fatty.	No incr. white corpus.	Diseased marrow	Psoriasis 12 years: no upper teeth 12 years: chronic follicular intestinal catarrh and chronic suppuration of gall-bladder.	
Dr. Pye-Smith, Virchow's Archiv, lxv.	1875	2	1	..	52	1	Healthy: muc. memb. of stomach thickened.	Normal.	Normal	Normal	Fatty.	Not fatty.	No increase white corpus.	Not exam'd	Porter; parents died at 78 and 98. Cough; anorexia; transitory diarrhœa for four months, and then when first seen was intensely anæmic.	7 months.
			1	..	47	1	Solar plexus normal.	Normal.	Normal	Fatty.	Fatty.	Not fatty.	No incr. white corpus.	Not exam'd	Doubtful attacks of ague; in last seven years has suffered several times from dyspnœa, and his face paled and strength failed gradually.	Could not be determined.
Dr. Scheby-Buch, Deutsches Archiv, Bd. xvii. pp. 466–490.	1876	2	1	..	48	1	Healthy.	Moderately enlarged.	Normal	Normal	Not exam'd	Not exam'd	No increase white corpus.	Not exam'd	Carpenter; no previous disease; no unfavorable conditions of life; ulcers on legs discharged freely from Easter to Christmas, 1871, and since then pale; died in collapse.	About 17 months from failure of health.
			..	1	60	1	Dilated bronchi.	Moderately enlarged.	Not stated.	Fatty.	Fatty.	Fatty.	No increase white corpus.	Diseased marrow	Nurse; sickly for several years; well-fed; from Easter 1873 grew worse and more anæmic; died gradually collapsed and comatose.	Not to be determined, only 2 weeks in hospital.
Dr. E. H. Bradford, Boston Med. and Surg. Jour. July 1876.	1876	1	..	1	11	No	....	....	....	....	....	....	About 1 white to 245 red	Not exam'd	Health good; well-fed; other members of family healthy looking; during March began to lose appetite, and gradually lost strength and color. No disease found during life.	About 71 days only.
Krieg, quoted in Centralblatt, Berlin, 24 June, from Würtemb. Corr. Bl. 1875, No. 39.	1876	3	..	1	48	1	Sarcoma in brain.	....	....	....	Fatty.			Not	Married peasant; not stated how many children; was [illegible] before death: during [illegible]ns and pleurisy.	
			1	..	52	No	....	....	....							
			1	..	45	No	....	....	.....							

Author.	Date.	Cases.	Sex. M	Sex. F	Age.	Autopsy.	Viscera.	Spleen.	Glands.	Liver.	Heart.	Kidneys.	Blood.	Marrow of bones.	Previous condition of health. Remarks.	Duration.
Dr. R. P. Howard, Sept.	1871		1	..	41	1	Healthy.	Normal.	Mesenteric enlarged.	Normal	Normal	Normal	No increase white corpus.	Not exam'd	Forwarding merchant; healthy till two years before; a sister died of consumption; rest of family healthy. For last two years at times uneasiness after meals, sore mouth, and gradual loss of strength and color.	About 2 years and 2 months.
" April.	1872		1	..	37	No	....	....	....	....	....	....	....	....	Merchant; ague sixteen years ago; a tendency to diarrhœa. Came to Montreal eight years ago, and since then complexion had gradually grown paler and tendency to diarrhœa increased. Father died at 27 of cardiac disease, and mother of bowel disease. No autopsy, but no disease could be detected during life.	Could not be determined.
"	1874	4	1	..	53	1	Calcareous masses in lungs.	Atrophied	Atrophied mesenteric and lumbar	Normal	Fatty.	Normal	No increase white corpus.	Not exam'd	Bookkeeper; healthy, not robust; slight diarrhœa in 1872. April, 1874, indigestion, which continued in a mild form all the year after, and pallor showed itself in August.	About a year from time he consulted me.
Notes given me by Dr. Greene, of Granby.	1875		..	1	46	No	....	....	....	....	....	....	....	....	Respectable married woman, four children; years ago had few slight epistaxes; menorrhagia for two years, and then began to be anæmic; seen by Dr. G. two years later; intense anæmia existed; died exhausted. Jan. 17, 1876. Family history indicated predisposition to anæmia. No disease of organs detected during life.	
Dr. F. Fedé, quoted in Contralblatt f.d. Medic. Wissenschaften, Oct. 16.	1875	1	..	..	30		....	3 times normal size and cont'ning larger cells than natural, and like those found in marrow.	....	Little enl'ged	Fatty.	....	No increase white corpus.	Tumor on ribs; diseased marrow	Unmarried; lived under unfavorable conditions; had been always sickly, and was exhausted by repeated hemorrhages; for a year had suffered rheumatic pains in joints; case of myelogenous pseudo-leukæmia(?).	

ALCOHOL IN ITS THERAPEUTIC RELATIONS AS A FOOD AND AS A MEDICINE.

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We propose to consider the subject of this paper[1] by inquiring into—

I. The value of alcohol as a food;

II. Its value as a medicine;

III. How far its value is modified by variability in the composition of spirituous liquors.

I.—Any article, to rank as a food, must be convertible into tissue or force, in such a way as to contribute to healthy vitality and aid the body in the performance of its normal functions. This includes that energy which the body needs in the execution of its own processes of nutrition and repair, and that which must be generated to fit it for the expenditure of proper force in its contact with the world about it. So definite is the relation between the human system and the usual foods by which it is sustained and propelled, that in respect to most of them we are not left in doubt. If we take any one of the ordinary aliments, and subject it to chemical analysis, and then apply the same process of examination to human material, we are not slow to trace the correspondence of the two. Since chemistry has come to be more perfect in its methods of analysis, and physiological investigators have been careful to study relations, and test them by science and art combined, these adaptations are seen to be systematic and definite.

[The ascertainable qualities which constitute a food are, in the original paper, illustrated by a comparison of the various aliments with the organic constituents of the body. A description is given of the ascertained processes of their conversion into tissue and force. The testimony of Wanklyn, Fowne, Attfield, Bridges, Liebig, and Bernard is adduced as to the accuracy of our knowledge of foods, with allusions to the experiments and testimony of Voit, Bischoff, Wilson, Playfair, Lankester, Frankland, Haughton, Mapother, and others. It is shown that alcohol, as subjected to such tests, fails to establish itself as a food.]

There has been such unanimity of consent among those of divergent views in other regards, as to alcohol, that it is not a tissue-building food, that it is by quite common consent excluded from this class. We have never seen but a single suggestion that it could so act, and this a mere guess. One writer (Hammond) thinks it possible that alcohol may somehow enter into combination with the products of tissue decay, and "under certain circumstances might yield their nitrogen to the construction of new tissue." There is no parallel to this in Organic Chemistry, nor can any evidence be found in Animal Chemistry to surround this guess with the aureola of a possible hypothesis.

It has been conclusively proved, says Lionel Beale,[2] that alcohol is not

[1] [The entire paper being too voluminous for insertion, the author has furnished a synopsis for the Transactions, reserving the right to publish the whole in a separate form.—EDITOR.]

[2] Med. Times, 1872.

a food, and does not directly nourish the tissues. "There is nothing in alcohol with which any part of the body can be nourished."[1] "It is not demonstrable at present that alcohol undergoes conversion into tissue."[2] Alcohol contains no nitrogen; it has none of the qualities of the structure-building foods; it is incapable of being transformed into any of them; it is therefore not a food in the sense of its being a constructive agent in the building up of the body.[3] The food tables of Letheby, Frankland, E. Smith, etc., give no place to alcohol as a tissue-forming food, and only allude to ale and porter as containing some nutritious elements from the presence of other substances. The period of youth is that in which it should be used to give muscular development and structure-building power, if possessing these properties, but authors and experimenters with one accord exclude it from the diet of children.

In Comparative Histology we find the structure of animals, and the laws of growth, repair, and tissue-secretion, quite similar to those in man: yet none of the careful experiments as to foods, or the development of the animal kingdom, have ever given alcohol a place for the construction of tissue. This undoubted exclusion of alcohol from nitrogenous foods is all the more significant because recent investigations have shown that this class of foods not only makes cells, but that it has very important correlation in heat producing processes.

[Why alcohol cannot be placed amid carbonaceous, or heat or force producing foods. It is now shown how uniform is the production of heat or force as the result of the union of oxygen with the carbons; how accurately it can be tested, and how alcohol fails in this test. The accuracyof experiments illustrated by those of Pettenkofer, Fowne, etc., with confirmatory quotations from Liebermeister, Edward Smith, Parkes, and H. C. Wood, Jr., showing why alcohol fails as a producer of heat and force.]

The idea that alcohol is a heat-producing food, and so, like calorific foods, is consumed in the system according to the usual law of disintegration, has been so far accepted that Anstie, speaking of its *anti-pyretic* effect, as now admitted, says, "Until within the last few years it was never even suspected."[4] In his advocacy, while claiming that somehow alcohol must be oxidized and converted into force, he then says "this force cannot be heat." In other words, the most remarkable exception in all the laws of energy must be assumed to exist in order to make tenable the view that alcohol undergoes oxidation, so as somehow to elaborate life force. Yet as H. C. Wood, Jr., in his recent work on Materia Medica, expresses it: "No one has been able to detect in the blood any of the ordinary products of its oxidation."[5]

[Examination as to whether alcohol increases the excretion of carbonic acid (carbon-dioxide), which is a leading proof of the food value of the hydrocarbons, of oils, and of such liquids as in their change impart vital force. It is shown that "the quantity of carbon-dioxide and water produced" in vital processes is as accurately determined as in inorganic analysis. Testimony of the leading experimenters examined and compared. The "weight of evidence is that it diminishes the elimination of carbonic acid" (H. C. Wood, Jr.). So far from combining in any way so as to form water, alcohol is the great parcher of tissue. It is inquired next "Is not the formation of fat a result which proves a nutritive process, and is it not secured by the use of alcohol?" Whatever may have been the results from malt liquors, or from sugary or other mixtures containing alcohol, it is shown that alcohol neither contains any fat material, nor is any conversion of it into fat, such as takes place with some foods, ascertainable. Explanation of cases of increase of fat while liquors are used. Remarks as to how imperfect a test of healthy nutrition, is mere increase of fat. Examination of facts as to the reappearance of alcohol in the secretions, and how far its quantitative

[1] Cameron, Manual of Hygiene, p. 282.
[2] Hammond, Tribune Lecture, May, 1874.
[3] Richardson on Alcohol, p. 21.
[4] Practitioner, vol. ii., 1873, p. 422.
[5] Op. cit., p. 107.

absence is a test of its appropriation as a food. The imperfection of the evidence as shown by a survey of the experiments and views of Anstie, Ford, Parkes, Thudichum and Dupré, Richardson, Duroy and Massing, Ringer, etc.]

Parkes says, in reply to Anstie,[1] "Even if complete destruction within certain limits were quite clear, this fact alone would not guide us to the dietetic use of alcohol. We have first to trace the effect of the destruction, and learn whether it is for good or for evil. You seem to think that the destruction must give rise to useful force, but I cannot think that this is necessarily so." When an article appears to a considerable extent in the excretions, it seems to indicate at least that it is not readily assimilated. If under experiments absolutely complete, instead of very defective, alcohol could not all be found, we would look for such results of disintegration as the laws of animal chemistry would indicate, and see how the changed products had been made available as food. Finding none of those products which are derived from alcohol as consumed out of the system, nor such as occur when foods are appropriated in the system, we would be quite excusable for asking some evidence of food-value beyond the fact that the exact quantity imbibed could not be recovered from the excretions.

We can find no chemical law or practical excretory result which defines alcohol as a food. "The mode of destruction is in fact unknown."[2] We search in vain for products of oxidation, except a slight change perhaps of a portion into acetic acid, in the stomach, and, in the case of the imbibition of large quantities, the acidity of the urine which is found slightly increased. "Yet in animals poisoned with alcohol, Buckheim and Massing could find no acetic acid in the blood."[3] No one has as yet been able to show a method of consumption such as ought to be manifested if this article acts as a food. If decomposed at all, it is, says Richardson, probably "at the expense of the oxygen which ought to be applied for the natural heating of the body."

But if those who would defend alcohol as a food are unable to identify it as such by any of those tests which indicate nutritive or energizing foods, may we not assign it a place in what are called accessory or auxiliary foods?

[Accessory and auxiliary foods defined, and the terms shown not to be an indefinite escapement for failures of evidence. How as a so-called accessory or auxiliary food alcohol fails to find for itself such a place as is assigned to water, chloride of sodium, condiments, fruits, etc. It is shown how modern chemistry has made even those which we once called accessory foods to have a definable place.]

Alcohol not only does not merit a place as an accessory food, according to any known laws of such foods, but it is very well known to induce deterioration of tissue, and to disturb the relation between the nitrogenous foods and water. In appropriating the water so as to dry the tissue, as already noted, it thickens membranes intended for that vital transmission of fluids known as endosmose. Thus local "sclerosis or a hardening of nerve tissues is one of its frequent manifestations."[4]

[Special examination of the attempt to define alcohol as an accessory food by saying that it delays metamorphosis of tissue. The whole subject of progressive and retrogressive metamorphosis of tissue examined. Their importance as physiological processes shown to be such that delay of metamorphosis is in itself, as a rule, a disturbing and invaliding process. It is so exceptionally beneficial that it must be directly shown, in the case of any special medicament, that it is conservative of health, and how it is. Quotations from

[1] Practitioner, p. 85.
[2] Parkes, p. 272.
[3] Ibid.
[4] Hammond.

Pappenheim, Cameron, Beale, Hammond, and others, showing the inconclusiveness of this kind of evidence. Full examination of this argument as attempted to be substantiated by diminution or change in excretions, and by its impeding of corpuscle and other blood-changes.

Examination of the claim that alcohol belongs to the catalytic substances, and modifies metamorphosis by the "action of presence."

Next, does alcohol show a food value by the tests of dietetic and sanitary experience? Examination of the cases of Anstie and Inman, and rebutting evidence. It is concluded that alcohol is not shown to have a definite food value by any of the methods of chemical analysis or physiological investigation.]

II.—We next propose to discuss the place of alcohol in the Materia Medica, and its therapeutic action. "There is," says Lionel Beale, "no more important question in medicine to be determined than the action of alcohol in disease." As we come to inquire into the value of alcohol as a medicine, after having found its reputation as a food unsustained, it is well to remember that the terms food and medicine are often more nearly allied than the mention of the words is apt to indicate. A medicine is that which helps to heal or repair, for that is both the etymological meaning of the word and the practical design of the article used. The process of restoration or repair is often but an application of the process of natural nutrition. Amid the progressive changes of food into tissue, and the retrogressive disintegration which all life implies, we must not deceive ourselves by terms. Much of the discussion, therefore, as to the value of alcohol as a medicine, is in reality to be determined by what it can do toward repairing the waste of tissue which occurs in disease. What it can do in this regard depends largely upon the determination of its food value. This we have seen to be so small and indeterminate, that it will not do to push it forward as a valuable medicine in those respects in which medicine chiefly concerns nutrition and the production of animal heat.

When we find that alcohol has no nitrogen with which to nourish; that it does not respond to the laws by which animal heat is usually evolved; that it at best undergoes such imperfect change in the system that much of it is found unchanged in the secretions, excretions, and tissues; that the products of its primary or secondary change cannot be identified; that it is not settled whether it diminishes the carbonic acid, or urea, or other excreta, or whether such diminution would be reparative, we may well hesitate to assign this drug a place in the category of medicinal agents. When it has eluded the ingenuities of science, the persuasions of art, and the astute diligence of interest, to extemporize it into a food, it must be remembered that it has made a signal failure to vindicate its position as a medicine in any one of the particulars in regard to which it is most frequently urged as of value.

[Next to the consideration of the place of alcohol as a medicine, we come to inquire how far the variability in the composition of spirituous liquors and their unreliability modify their therapeutic value.]

III.—If we are to consider any article therapeutically, we ought to be able to know that it is the article which it professes to be. The clinician who claims uniformity of result, and a declared therapeutic effect, from a given article, has reason to question whether the effect was a result of the conceived cause, when he finds that the alleged cause was necessarily inoperative by reason of absence or admixture. If the article used was not tested either as to quality or quantity, if the general rule in respect

to it is unreliability, we surely cannot trust general opinions as to its dietetic or medicinal value.

We have been used to hear criticism as to the composition of liquors without much professional impression derived therefrom, supposing that these criticisms were chiefly based upon general accusation, or the trial of bar-room specimens, and that the bonded warehouse and the druggist's label were an assurance of purity. But in the medicinal examination of liquors we find some facts which address themselves to us most directly in their clinical bearings. In our inquiry in this line we shall pursue the same course as when examining into food value, seeking our information from chemists, from medical experts, and from the best accepted authorities on the composition of alcoholic medicines.

There is much difference in different spirituous liquors, and in different specimens of the same kinds of liquor, as to the amount and kinds of alcohol and other ingredients, independent of any question of adulteration. A reference to the Proportion Tables of Brande, Fontenelle, Bence Jones, Thudichum and Dupré, etc., will show at a glance such variations as these: Wines, in general, contain from 8 to 26 per cent. of alcohol; Madeira from 19 to 26 per cent.; Port from 17 to 25 per cent; Sherry from 19 to 25 per cent.; and various other wines from 16 to 18 per cent. Thudichum and Dupré state that a natural wine may contain from 9 to 16 per cent. of alcohol. Fowne says of them, "The quantity of alcohol varies very much." Sherry often has from 6 to 8 gallons, port wine 3 gallons, and the best port 13 to 15 gallons of brandy, added per cask. This is not considered admixture, but original "fortification," as it is called. Native European wines, by the English law, must not have over 26 per cent. of alcohol. Champagne, Burgundy, and the Rhenish wines, contain an amount varying from 8 to 14 per cent., by measure, of alcohol.

Such statements are made still more significant from the authority of Parkes, who says: "So various is the amount of alcohol in wines from the same district, that a very general notion only can be obtained by tables, and samples of the wine actually used must be analyzed."[1] Brandy, whiskey, and gin contain from 50 to 60 per cent. of alcohol, and rum from 60 to 80 per cent.; apple brandy should have from 40 to 46 per cent. As we shall see hereafter, these are so varied in other regards, in the various processes and methods of manufacture, that their more restricted alcoholic variation is an inadequate measure of their therapeutical import.

If, as claimed by most, alcohol is the chief available medicinal ingredient of alcoholic stimulants, it is clinically requisite to know just how much we are administering in each dose prescribed. This all the more when leading representatives of alcoholic treatment now claim that the limit in health is within two ounces in twenty-four hours; and that the limit in disease is far narrower than once believed. In face of the grave facts which come to us from pathology and from practice as to the effects of over-doses on the nervous and circulatory systems, and upon secretion and excretion, it is not good practice to deal with an article without some definite knowledge of the dose administered. While effects are a proper measure of degree of use, when they are so watched and classified, and so numerous, as to justify inferences, yet that must

[1] Op. cit. p. 265.

ever be an imperfect clinical study of any medicine which is unable to state the amount administered in any given case.

But the time is already at hand when, in speaking of alcoholic liquors, we must define what form of alcohol we mean, as well as how much of it is present, since they are often mingled, and their physiological and medicinal effects are totally different. Let us gather up briefly from chemists and experimenters, such as Fowne, Attfield, Richardson, and Prescott, a few significant facts, which are, we believe, not gainsaid, and which present the question of the medicinal use of alcohol, so called, in quite a new light.

Fowne gives a list of twelve alcohols, as belonging to what is known as the monatomic series. Of these, five chiefly concern us in medicinal and dietetic relations; these are the methylic, CH_4O; the ethylic, C_2H_6O; the propylic, C_3H_8O; the butylic, $C_4H_{10}O$; and the amylic, $C_5H_{12}O$. Fowne says: "The term alcohol, originally limited to one substance, viz., spirit of wine, is now applied to a large number of organic compounds, many of which, in their external characters, exhibit but little resemblance to ordinary alcohol." Yet, as we shall see, no usual liquor is clear of some one of them besides the ethylic, which is the one known as common alcohol; and, secondly, their effects are medicinally quite different..

"The first of the series, methyl alcohol, naphtha, or wood spirit, is not an original impurity in manufactures of an ethyl (or common) alcohol; but in the form of methylated spirit has been added in the falsification of liquors."[1] Pure methyl alcohol is a colorless, thin liquid, very similar in smell and taste to ethyl alcohol, and, like it, is a mobile, watery liquid.[2] "It has an aromatic odor, free from empyreuma, and a sharp but not acrid taste."[3] The methylated spirit has been used fraudulently in the manufacture of the sweet spirit of nitre.[4] Richardson[5] has made some important inquiries in respect to this substance. He names the case of a well-known physician who himself used it in preference to common alcohol, regarding it as less injurious. Richardson has long used it in his practice as a substitute for the common alcohol. He says "it is much more rapid in action, and much less prolonged in its effects, than is common alcohol, so that it produces its effects promptly, and, what is of most importance, it demands the least possible ultimate expenditure of animal force for its elimination from the body. This latter fact is of great moment, for, in the end, all these alcoholic fluids are depressants, and although at first, by their calling vigorously into play the natural forces, they seem to excite, and are therefore called stimulants—they themselves supply no force." He notices its special power of reducing the temperature (p. 32), and calls it the lightest and least injurious of the alcohols (p. 23). If we are giving an alcoholic medicine made of this substance, we are using the spirit most allied to, and yet very distinct from, ethylic alcohol, or the purported kind.[6]

The propylic alcohol is not yet much understood in its medicinal action. Chancel[7] obtained it from fusel or amylic alcohol by fractional distillation. One of its isomeric modifications has a very close resemblance to ethyl alcohol. It is not unfrequently found with it in liquors, and, of course, modifies its characteristics. Its medicinal action is not

[1] Prescott, Peninsular Journal of Medicine, Detroit, April, 1876.
[2] Fowne, p. 510.
[3] Prescott, loc. cit. 262.
[4] Attfield, p. 384.
[5] Cantor Lectures on Alcohol, p. 30.
[6] See also Monroe, "Is Alcohol Necessary?"
[7] Fowne, p. 31.

yet well enough understood to draw the contrast between it and the genuine ethyl drink or medicine. It is, if possible, even more greedy of water than the latter, and so still more robs the tissues.

Butylic alcohol is often contained in amylic alcohol, and so is present in the usual fermentations. When valerianic acid is artificially prepared from butylic alcohol, as is commonly done, the various valerianates are often contaminated by butyrates.[1] There is reason, therefore, to believe that it is often present with the other alcohols. Richardson seems to have shown that this heavier alcohol is much more apt than the ordinary alcohol to cause tremors of the kind occurring in delirium tremens. Its effect is certainly very different from that of the alcohol designed for common medicinal use, and it must be studied by the physician in its own distinct effects.

The next in the series is known variously as amylic alcohol, potato oil, grain oil or grain spirit, and fusel oil. In its relation to the ethylic or assumed common alcohol of alcoholic liquors, it is of most interest to the physician. "It constitutes," says H. B. Prescott,[2] "the chief part of fusel oil, which also contains the fourth alcohol (butylic), and traces of the third (propylic), and these together appear quite invariably in the alcoholic fermentation of sugar, their preparation being varied by conditions." "It is the great and naturally occurring impurity of liquors," says Fowne (p. 535). In the manufacture of brandy from corn, potatoes, or the must of grapes, the ethyl (common) alcohol is found to be accompanied by an acrid oily liquid called fusel oil, which is very difficult to separate completely from the ethyl alcohol." Attfield[3] says, "Amylic alcohol is a constant accompaniment of ethylic or common alcohol, when the latter is prepared from sugar which has been derived from starch." This, of course, applies to all alcoholics prepared from grains or vegetables, either by brewing or distillation.[4] Not only does it thus occur naturally, but some of that found in wines and spirits is probably added. Much of it is imported to England, it is believed, for that purpose.[5] No doubt much of it is thus returned to us as a direct re-importation, marked pure from the bonded warehouse. As common alcohol dissolves it readily,[6] as it has a pungent but not acrid taste by dilution, and has a fruity taste and smell, it is much relished. It is the heaviest and most injurious of the five alcohols thus far noticed. It has a burning, acrid taste, its vapor producing oppression of the chest.[7] When produced as so often in manufacture, or when added for profit and improvement of flavor, it is "an extremely dangerous addition to ordinary alcohol." Its action is the same as that of butylic alcohol, "except more prolonged by reason of its greater weight and insolubility."[8]

In this country, where whiskey and other fermented products are so largely used, the art of their concealment and modification so as to give a relished flavor, is well understood. There is little doubt that certain effects of alcoholic liquors of late years are due to the more profitable utilization of the "contemporaneous" alcohols with the ethylic. It is also true that the addition of such alcoholics as the methylic, or of various substitutes for all the alcohols, has left what is called the experience of prescribers somewhat mythical.

[1] Attfield, Med. and Pharm. Chemistry, p. 322.
[2] Loc. cit.
[3] Op. cit. p. 389.
[4] Fowne, p. 520.
[5] Richardson, op. cit. p. 36.
[6] Fowne, p. 535.
[7] Ibid. p. 536.
[8] Richardson, p. 37.

[Next it is shown how, in the various modes of preparation and manufacture, variations occur not only from different vintages, distilleries, and breweries, but from difference in method and the financial interests of makers—all within the limit of legitimate preparation.

Next it is shown, by the testimony of chemists, how different alcoholics are modified by adulteration or imitation. Testimony of Thudichum and Dupré, Mülder, Parkes, Richardson, Battershall, Prescott, Cameron, etc.]

There is still another most important therapeutical view to be taken of the various alcoholics.

The time has already come when prepared liquors which have never seen a vintage or a distillery, dispute their right to be accounted genuine, and not to be branded with the epithet "adulteration." The ablest and most candid French work and authority on the manufacture of liquors (Duplais), discusses the question from the stand-point of business integrity, and leaves it without adverse decision. Prof. A. B. Prescott, the able chemist of Michigan University, in his work on the Chemical Examination of Alcoholic Liquors, says: "The term brandy, as used in commerce, without qualification, must be held by common consent to include artificial brandy." He makes three divisions, viz.: veritable brandy, artificial brandy (or that made of the same ingredients according to chemistry), and fictitious brandy; *i. e.* a fraudulent imitation. There are many preparations known as brandy essences, which are so combined with spirit as to make artificial brandies difficult of distinction from the veritable.[1]

Griffen, who is an authority, and who has examined many specimens, says that he found what he called Public House Brandy not to be a distilled spirit, and yet that it had the taste of a good cognac. He gives a German recipe which he says, after long keeping, gives a brandy very similar to real cognac in taste and odor. He adds that this sort of sophistication is evidently becoming an ordinary commercial practice. The recipe, as given by him, contains much purified spirit, but only eight quarts of wine to 150 quarts of mixture.

Duplais's most able and scientific work, of 700 pages octavo, translated by McKennie, and published by Baird, Philadelphia, 1871, would be incomplete without its pages on "Imitations." Speaking of cognac, as the most difficult of all brandies to prepare artificially, it says (p. 293): "It is a fact worthy of note that the brandy obtained in consequence of the addition of a spirit foreign to the wine, in limited proportions, cannot be distinguished from the natural wine by itself, that is to say, without the addition. Finally, brandy resulting from the new method, defies all methods of investigation. We may suspect the mixture, and even know of its existence, but we cannot furnish the proof; neither the most skilful and practiced taste, nor the persevering researches of the most skilful and learned chemists, have been able to detect it. M. Payen himself acknowledged some time since that, in the actual state of the science, the discovery of the mixture presented insurmountable obstacles." Such a book as that of Pierre Lacour, on "The Manufacture of Liquors, Wines, and Cordials, without the aid of Distillation," and others well-known to makers, and but little to physicians and pharmacists, show to what perfection the experts have attained. The chapter of Duplais on the preparation of various liquors, of which alcohol, sugar, and water, are the basis, also shows how artificial and successful are such compounds. The author, after admitting that "the immoderate use of spirits, and

[1] Prescott, p. 23.

even liquors, is pernicious," and after designating the drunkard as "a wretch who is unworthy to live" (pp. 437–8), claims that the "aromatics and sugar are good for digestion," and the small quantity of alcohol not injurious to persons in health.

We have had occasion to examine recent popular books of recipes for the imitation of liquors, and it is wonderful to what perfection the art has been elaborated. When we look into our recent chemistries and see how many natural products are duplicated in art, and that fruit essences and flavors are so well imitated that connoisseurs cannot distinguish the artificial from the real, it is to be expected that the great profits accruing would induce the production of liquors without the trouble of vintage, wine press, or distillery. More Madeira wine is in the market than could be made on the island of Madeira if each grape made a gallon. Critical examinations have gone far enough to show that in all alcoholic drinks, as consumed for thirst or medicine, the veritable article is largely reduced or superseded by admixtures and imitations. The editor of the London Lancet well says: "We suggest it were well worth our while in the future for us to transfer our allegiance, when we would prescribe alcohol, from the wine and spirit merchant to the chemist."[1] When imitations are repudiating the reflection of being "spurious," and claiming legitimate place beside the "veritable" products, and when the "fictitious" are difficult of distinguishment, is it not high time for the therapeutist to take reckoning as to his professional position in reference to alcoholics as at present prescribed?

In face of facts, such as we have adduced, it is a burlesque on the precision of modern therapeutics to deliver *ex cathedrâ* opinions as to the medicinal value of alcohol, when we are using all sorts of remedies under the name, are attributing to them results which are shown to be impossible, and prescribing them with an unscientific laxity which would be scouted in the confines or even in the purlieus of any exact science or any expert art.

In the light of accumulating facts as to the whole alcohol series, as to the manifold and variable combinations prescribed under the cognomen of alcoholics, and with candid regard to the wide and contradictory range of their composition, we must needs review the record of our materia medica. We must recognize the unscientific and empirical data on which we have rested, as insufficient to overcome the facts of histological and pathological research, and feel that demand is made upon us to show the conservative *modus operandi* of a medicine which so significantly asserts itself as a toxic and depressant.

Such questions as these are presented for exact consideration, to a degree that neither science nor practice would ignore.

(1) What is the effect of ethyl alcohol as a medicine?

(2) What trustworthy experiments have we as to its physiological effects, and as to its therapeutic indications?

(3) What guide as to the quantity to be given?

(4) What testimonies of experience as to its clinical use?

(5) With present light as to the manifold variations of alcohol in legitimate manufacture, and its change by admixture, are the usual testimonies as to the advantage of ethyl alcohol as a medicine of any professional value, unless composition has been determined?

[1] Lancet, Nov. 1, 1873, p. 639.

(6) How far can methyl alcohol take the place of ethyl alcohol as a medicine?[1]

(7) How much does any alcohol or fusel oil occur in various alcoholics, either by virtue of manufacture or admixture?

(8) How far are the effects of wines dependent upon their numerous exhilarating ethers, their albuminous matters, their sugars, acids, and salts, and how far are the effects of each kind due to the varying proportions of these? The same inquiry may be made as to all alcoholics from fruits.

(9) How far is the effect of whiskey due to its ethers, essential oils, acids, sugars, malt, etc., and so as to all distilled liquors from grains?

(10) How far do the effects of malted ales, beers, etc., depend on sugar, starch, etc.?

(11) How much are clinical experiments worth which speak of the value of alcohol as a medicine, with utter disregard of such items?

(12) Is it possible so to deprive wines, etc., of all alcohol, as to secure their action independent of the spirit?

[The rest of the paper reviews the changes in the use of alcohol in various diseases, and shows how assumptions as to its value have been modified. It shows, also, how other exhilarants can easily be substituted in a large majority of cases.]

The following are the conclusions submitted to the Section of Practical Medicine:—

I. Alcohol is not shown to have a definite food value by any of the methods of chemical analysis or physiological investigation.

II. Its use as a medicine is chiefly that of a cardiac stimulant, and often admits of substitution.

III. As a medicine, it is not well-fitted for self-prescription by the laity, and the medical profession is not accountable for such administration, or for the enormous evils arising therefrom.

IV. The purity of alcoholic liquors is, in general, not as well assured as that of articles used for medicine should be. The various mixtures, when used as medicines, should have a definite and known composition, and should not be interchanged promiscuously.

On motion of Dr. J. M. Ridge, of Camden, N. J., seconded by Dr. N. S. Davis, of Chicago, Ill., the conclusions of Dr. Hunt's paper were adopted as expressing the opinion of the Section, and were ordered to be transmitted to the National Temperance Society, the Women's National Christian Temperance Union, and the Friend's Temperance Union of New York, in reply to communications addressed by those bodies to the International Medical Congress, and by the Congress referred to the Section on Medicine.

[1] Richardson says that it is practicable to substitute it. A. B. Prescott expresses the opinion that the views of Richardson are correct. The introduction of methylated spirit, in place of the excise proof-spirit, has been a most important measure in Great Britain. See Report of Professors Graham, Hoffman, and Redwood, Quarterly Journal of Chemical Society, vol. viii., and Fowne's Chemistry, p. 518.

REPORT OF INVESTIGATIONS MADE UNDER THE DIRECTION OF PROFESSOR RUDNEW AT THE INSTITUTE OF PATHOLOGICAL ANATOMY OF ST PETERSBURG.

COMMUNICATED BY
PROFESSOR RUDNEW.

I. Sclerosis of the Vessels of the Lungs.

By Dr. Beresnewitsch.

Many eminent writers maintain that the vessels of the lungs are subject to a disease known as "Sclerosis," or the "Atheromatous process." Dittrich, in his work entitled "Uber den Laenneckschen Lungeninfarct," especially describes this disease, and states that it affects the arterial branches of the lungs, and causes the formation of a substance within the inner and middle coatings of the vessels, which he further asserts becomes, in time, either a fibrous tissue, or fatty or calcareous matter. Dittrich further says that this affection occurs more frequently with cardiac disease, especially of the right ventricle, or when the lungs are themselves affected with atrophy and emphysema.

Far from agreeing with this authority, we are led to believe from more recent observations, that instead of sclerosis resulting from emphysema, emphysema is more probably due to sclerosis. Other writers and anatomists seem to have given but little attention to this form of lung disease, some being disposed to accept Dittrich's theory, while others maintain the opinion that sclerosis of the lung vessels is extremely rare. In consequence of the small number of careful observations made in respect to this disease, and the diversity of opinion which appears to exist, I have instructed Dr. Beresnewitsch, one of my pupils, to determine by the minutest microscopical observation, (1) what arteries are directly affected (whether bronchial or pulmonary), and (2) under what conditions this disease occurs, the latter question to be investigated before the former.

Dr. Isaacson, one of my pupils, some years ago investigated, under my direction, many cases of emphysematous degeneration of the lungs, and found that this disease was always due to an affection of the lung vessels. The development of emphysema begins when the bloodvessels become closed by coagula, around which the tissues disappear by reason of want of nourishment. The occurrence of this coagulation is due to the destruction of the inner coating of the artery, and now Dr. Beresnewitsch has shown that this destruction is due to the presence of syphilitic infection, which in course of time manifests itself not only in the capillaries but also in the larger vessels, sclerosis consisting in the development of connective tissue between the layers which form the inner coating of the artery.

II. On the Development of Cancer in the Lymphatic Glands.

By Dr. Afanasieff.

Cornil, Thiersch, Waldeyer, Rudnew, and others, by their investigations, have already proved that the primary development of cancer always originates from the morbid condition of true epithelium. The greater part of the writers of the present day agree with this opinion, while the number inclined to differ therefrom becomes gradually less and less. The minute and careful investigations of later days tend to confirm this view, as we hear of no cases at the present time, in which cancer is reported as arising otherwise than in the epithelium.

This theory has been based upon the fact, only lately discovered, that there are many new formations which have hitherto been considered as cancerous, but which, although very similar, are not cancers at all, but tumors which are called endotheliomatous, a term which perfectly separates them from cancers which are epitheliomatous.

Upon the secondary knots of cancer, the opinions of different writers do not accord—some affirming that the secondary formations actually arise from the passing of cellular particles from the primary growth, while others maintain that the cellular particles transferred, impregnate those already existing, and thus together lead to the formation of the secondary knots. Waldeyer, Lücke, and a few others, accept the former view, but refuse to accept the latter. These authorities appear to base their opinion upon the doctrine of Remak, who asserts that the cells of the upper and lower layers of the embryo never mix with the elements of the middle layer.

In conjunction with my colleagues, I have lately undertaken a careful examination of the lymphatic glands affected with secondary cancer, and we find that, whether the glands are sound or affected by previous morbid processes, the condition of development of cancer knots remains the same, and in either case is due solely to the cellular particles transmitted from the primary cancer, and does not in any way result from the already existing elements of the glands, which elements, far from participating in the formation of the cancer knots, undergo a retrogressive change.

III. Pathology of Sclerosis of the Arteries.

By Dr. Engelhard.

The morbid changes to which the arteries are subject have been known a long time. Crell, Haller, Morgagni, and other writers of past times, described thickness and ossification of the arterial walls, but they did not possess the means now brought to bear in investigation, and seem to have considered the different stages of the disease, not as we now understand them to be, various phases of the same affection, but as totally distinct morbid conditions. The opinion of the authors referred to, upon the causes of disease of the arteries, seems to have been vague and contrary to fact, inasmuch as they attributed this form of disease to the absorption of foreign matter previously infused into the blood. Lebert and Stokes are, however, of opinion that sclerosis of the arteries arises from local changes produced by the nutrition, while Rokitansky, who formerly adhered to his own theory that the disease was due to the

deposit of a certain substance from the blood, now agrees with Dittrich and Virchow in attributing its origin to abnormal extension of the arteries.

With these three distinct forms of opinion, at variance with each other, which is to be considered as the correct one? The recent investigations made by myself and colleagues tend to convince us that not one of the already stated views is according to fact.

We find that the first morbid appearance is due to a rupture of the media which permits a passage from the adventitia to the intima. This rupture causes irritation of the adventitia, and provokes inflammation, the products of which pass through the seat of rupture and propagate within the intima. This rupture, crevice, or cleft, as it may be termed, although probably of minute dimensions at first, is greatly influenced by the amount of the product of inflammation, so that, as that product increases, so also does the crevice become increasingly large, until in due course the media in certain parts are totally destroyed, and the regular circulation of the blood is markedly obstructed, thus giving rise, after a time, to the various forms of aneurism. This will explain the manner in which sclerosis and aneurism of the arteries originate in certain cases.

Another form of the disease may be regarded as arising, not in consequence of an affection of the media, but of the intima itself. The internal surface of the intima ceases to retain its natural smoothness and rotundity, becoming undulating in appearance from an irregular thickening of its tissues, which thickening gives rise to *scores*, the ulceration of which, being permitted to increase, involves in destruction first the media and afterwards the adventitia. When the destruction of the media is complete, aneurism soon follows.

Having thus classed the forms of this disease under two distinct heads, permit me to specify the several causes by which they are produced. The first and most frequent cause of the changes of the vessels may be considered to be syphilitic infection of the body; the tissues of the media, under a syphilitic influence, being extremely fragile and liable to be ruptured from distension. Another very frequent cause is found in the morbid effect produced by alcoholic drinks. I may mention in reference to these two causes, that the first or syphilitic affection generally attacks the media, while the influence of alcohol is found to affect chiefly the intima. Among the many other causes of the production of sclerosis of the arteries, is the absorption of certain poisonous substances, such as mercury, phosphorus, etc., as well as of those poisonous matters the chemical nature of which is but little known, as those of typhus and intermittent fever, etc.

Now permit me to describe the anatomical changes, microscopical as well as macroscopical, to which the coats of the bloodvessels are liable. The *macroscopical* changes consist in a local thickening of the intima, having the appearance of yellowish spots dispersed over the inner surface of the arteries. The next stage or change is confined to the enlargement of the artery until it reaches the condition of aneurism. In describing the *microscopical* changes, I would distinguish very clearly the two different changes in the coats of the vessels. In the first or syphilitic affection the disease begins with a rupture in the media, which permits the inflammatory product of the adventitia to pass to the intima. This inflammation has generally the character of granulated inflammation. The second form commences, on the contrary, in the intima, the first

change consisting in the appearance of minute cellular particles collected into nests of a size more or less large. These cellular particles arise from two sources; sometimes they are formed by division of the tissue-cells, but more generally their existence is due to the extravasation of the white blood-corpuscles. These nests do not long retain their form, but in consequence of the degeneration of the cells are transferred into scores, giving rise to the Endarteritis ulcerosa.

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9

THE MICROSCOPY OF THE BLOOD.

BY

CHRISTOPHER JOHNSTON, M.D.,
OF BALTIMORE, MD.

A DISTINGUISHED writer, Dr. Thomas Williams,[1] states that "in the category of the nutritive fluids unmixed fluidity is not known; solids are introduced. Fluids are not self-productive; they require either the direct or the catalytic agency of solids. The fluids even of the vegetable organism bear floating corpuscles." And again farther on he insists that "no single real example occurs in the whole animal kingdom of a nutrient fluid totally destitute of morphological elements. This rule (of the corpusculation of the animal fluids) is ultimate and absolute."

The blood proper, corpusculated therefore, the chiefest of nutrient fluids, differs from any other, both in vertebrates and invertebrates, in that it is contained in a closed system of vessels, and moves in a determinate circulatory orbit. It was "this circulation, the discovery of which does not, and could hardly," says Flourens, "belong to one man, or even to a single epoch. Galen opposed Erasistratus; he opened the way which, afterwards followed by Vesalius, by Servetus, by Colombo, by Cæsalpinus, by Fabricius of Acquapendente, led us to Harvey." Whilst naming the great Servetus, however, we ought possibly to have cited another Spanish claimant, Francisco de la Reyna, of whom Don Francisco Benito Jeronimo Feyjoo (a very eminent and learned man, who "did more for the intellectual life of his country," observes Ticknor,[2] "than had been done for a century"), in the third edition of his "Cartas Eruditas," published in Madrid in 1774, makes the following mention. In the twenty-eighth Carta he says: "The literary phenomenon which you bring to my notice is rare, and curious as rare. Is it possible that

[1] British and Foreign Medico-Chirurgical Review, Oct. 1853, p. 361; Art. I., The Blood, etc.

[2] Ticknor's History of Spanish Literature.

a Spanish farrier can have been the first discoverer of the circulation of the blood? You write me that a friend of yours has a Book of Farriery, by the Farrier Francisco de la Reyna, printed in Burgos in the house of Felipe de la Junta in the year 1564, and that he saw the fellow to it in the Royal Library, probably the only copies in Spain. You send me the transcript of a passage in Chapter 94 of the said volume, so clear, so decisive, as to the circulation of the blood, that it establishes the fact that the aforesaid Reyna was acquainted with it. This little sentence banishes all doubt: "*So that the blood goes round and circulates throughout all the members* (De manera que la sangre anda en torno, y en rueda por todos los miembros)." Or, as Feyjoo, on page 318, quotes, "our Farrier, who first, before Harvey, used the word *circulation*,[1] and who left it distinctly written that the blood goes round and circulates through all the members (la sangre anda en torno y rueda por todos los miembros)."

However this may be, the circulation of the blood, the passage of the blood from the arteries into the veins, was *proven* as a fact by Harvey, but the direct demonstration was wanting. It came at last, the microscope afforded it; yet it was denied to the grand old age of Harvey, who died without witnessing it. But in 1661 Malpighi *saw* the blood pass from the arteries into the veins of the mesentery, the lung, and the urinary bladder of frogs, and by that act crowned the work

"By the side of the Wolffian bodies, on the axial side, the general epithelial lining of the great lymph sac, the pleuro-peritoneal cavity," thickens, its cells assume a columnar shape, they proliferate freely, and then form a raised mass of "germ epithelium." "From this layer the ova are subsequently developed, whilst the vascular stroma of the ovary is formed by an outgrowth from the side of the Wolffian body." In these few words a modern writer, while noticing the advanced genesial labors of Dr. Foulis,[2] expresses upon certain points the common belief of embryologists, and points out, in terms which they may or must accept, the manner of a new departure of nucleated protoplasm in the production of "primordial ova" from the germ-epithelial corpuscles. And again, according to Schenck,[3] we assert that "originally the entire pleuro-peritoneal fissure is lined on its inner surface with cylindrical cells, which originate from the subdivided lateral portions of what was at first the middle germinal plate." And this layer of cells serves exclusively to form the subsequent peritoneal epithelium. But the cylinders soon disappear, leaving as their descendants entirely flat, pale elements; the median angle corresponding to Remak's middle plate, and later the Wolffian bodies, which grow out from this spot, alone retain their covering of cylindrical epithelium. Since then this peculiar cylindrical epithelium, this germ-epithelium, gives rise out of its own elements to the primordial ovum, it follows that all tissues whatsoever reckon these extraordinary elements as their ancestors; and that, whatever be the intermediate forms, the transition is neither sudden nor remote from cylinders of germ epithelium to the corpuscles of the blood, that fluid organ of which it is now our province to speak.

The microscopical study of the Blood, however fascinating, is beset with difficulties even if we confine ourselves to the human subject, and

[1] Error: "Huic sanguinis *circulationi* ex dextro cordis ventriculo per pulmones, etc." Andreas Cæsalpini Quæstionum peripateticorum, lib. v. p. 125. Edition of the Giuntas, Venice, 1593.

[2] Lancet, March 14, 1876.

[3] Stricker, p. 540, Beitrag, etc.

these are greatly increased if our observations extend to other animals which are spoken of as standing lower in the scale. Its state or condition as deprived of life, its constituent parts motionless and tending to decomposition, must be regarded, or else it must be studied in the energy of life as it hurries on in the performance of its errand, making its circuit, and coming again and again to the *primum moventem;* as the sweet bard of Avon spoke of "those ruddy drops which visit this sad heart." Nor is the blood simply alive, it is also the seat of life; it is life itself. "Vita est in sanguine, vita est sanguis ipsa." Again the blood may be studied microscopically or otherwise, as an organ self-sustaining, replenishing its substance, providing for its own nutrition and loss by wear; as an organ, "mère de tous les tissus,"[1] offering to every structure the pabulum for its needs; as an organ, through or by its anatomical constituents, brought into intimate association with the great acts of nutrition; and as an organ gathering up the molecules of somatic attrition everywhere and conveying them to an emunctory. The blood also offers for consideration various accidental, incidental, and temporarily abiding matters, such as therapeutical remedies, introduced by the mouth or by the hypodermic needle; poisons of every kind; bacteria, on the high authority of Billroth and others; and Filaria sanguinis, as seen by many in the blood of dogs, but studied in India, in the human blood, by T. R. Lewis, M.B., and by him associated with that singular condition of which chylous urine is a marked feature.

The elements of which this exceedingly composite fluid is made up, in the vertebrata, may be arranged in several categories, *one* of which would include all of the many components, whether tending centripetally or centrifugally, whether these be a solvent, or be themselves held in suspension or solution, and all regarded and scrutinized in the state in which they absolutely exist in the blood; *another* would segregate the same elements, and present them isolated, each in its own distinct and independent physical condition; and finally *a third* would establish a distinction between the elements having form and those having no definite figure, or being formless in their actual mode of being in the living, moving, sanguineous stream.

It is true that the microscope must be the handmaiden of science in each of these fields, limitrophic but yet autonomic, whether the objects be originally, primarily, or secondarily encountered in them; but it is with the elements of the *third category* that the microscope chiefly exhibits and exerts its magical power, presenting to the eye, "and to the intelligence by the eye,"[2] life-histories of the forms which swarm in its illuminated circle, whether the tendencies be intrinsic or extrinsic, normal or pathological. The elements which have a form, characteristic and determined, and an approximative size, are the red corpuscles and the leucocytes, white corpuscles, or white globules; and with these we propose to deal exclusively, regarding them from two points of view, viz., (1) that of anatomy and physiology, and (2) that of medical jurisprudence.

It is with the origin and reproduction of the blood-disks, as with the genesis of a new being; the most intense interest as well as curiosity is excited, and all possible means of research are called into requisition to push, *plus ultra*, the limits of knowledge, and to mark with tangible definiteness the boundaries of legitimate speculation. And it so happens

[1] Dugès, Physiologie. Art. Sang. [2] Flourens, Leçons Orales. [C. J.]

that an integer of the "germ-epithelium" becomes or is a primordial ovum, and that from certain of its derivatives, even in the vascular system itself, the ruddy corpuscles arise, oscillate, and hesitate with uncertain aim, until the *primum movens* gives direction as it acquires power, whereupon they speed away in ceaseless commotion. But the microscope ows the forms of the "primordial blood-colored blood corpuscles,"[1] and ognizes these bodies in all their after career; and yet so differing are gments that they are asserted by Afanasieff to originate in the fowl contemporaneously with the first vessels of the vascular area, or in the vascular layer and area opaca, detaching themselves from the walls of the vascular spaces, or originating, according to the views of His, in the form of groups, from large masses of protoplasm in the walls of the vessels, and at a later period bursting into their lumen.[2]

In mammals it is impossible to adduce other expression than Kölliker's,[3] that at a very early period nucleated colored corpuscles in process of fission are visible in the blood of the embryo.[4] In proportion, however, as the embryo and spleen especially undergo development, these forms are less abundant; and, after a certain time, leucocytes first make their appearance in the blood of the liver. Or that of Schmidt,[5] in a paper "on the origin and development of the colored blood corpuscles in man," which is that "the facts already observed [by him], and being in accord with each other, strongly indicate that the primary birth-place of the colored blood corpuscles in the human embryo is to be sought in the gland-like follicles of the umbilical vesicle." We can, therefore, says the same author, "look upon the blood corpuscle as a *gland cell*," and consider that in early embryonic and in later life the organs in which the processes of generation and formation of the blood corpuscles in man take place, represent nothing else but glands, and that the leucocytes, or rather the nuclei of the leucocytes, are metamorphosed into colored corpuscles.

In January, 1875, the younger Flint,[6] in the first volume of his Physiology, gives a summary of our knowledge, in the present state of which the following seem to be the most rational views with regard to the development and nutrition of the blood corpuscles:—

(1) "At their first appearance in the ovum they are formed by no special organs, for no special organs exist at that time, but appear by genesis in the sanguineous blastema.

(2) "When fully formed they are regularly organized anatomical elements, subject to the same laws of gradual waste and repair as any of the anatomical elements of the tissues.

(3) "They are generated *de novo* in the adult, when diminished in quantity by hemorrhage or otherwise, and, under these circumstances, they are formed in the liquor sanguinis by the same process by which they take their origin in the ovum."

From what has been advanced it appears that in all of the fœtal life of man, regarded as a representative of mammals, four conditions of the blood manifest themselves, to wit: (1) the primordial, colored, flattened corpuscles existing alone at a very early period; (2) the appearance of leucocytes associated with the preceding; (3) the occurrence of circular,

[1] Remak; Rollett, in Stricker, p. 291.
[2] Rollett, p. 291.
[3] Zeitschrift für Rat. Med., 1867.
[4] Rollett.
[5] H. D. Schmidt, M.D., Monthly Microscopical Journal, February 1, 1874.
[6] Flint, Physiology, vol. i., Blood, etc.

bi-concave disks mingled with the two other forms; and (4) the withdrawal from the blood-mass of the first formed, the temporary embryonal colored corpuscles, upon which the blood corpuscles, red and white, adopt a relation which is only to be altered by disease or physical violence. And here we are immediately struck with the difference between the transient form as nucleated, and the permanent form as non-nucleated, and we ask ourselves if it can be, as Flint has declared, that the red disks are probably generated by the same process in the embryo and in the adult. If, for a while, we wander from the domain of fact to the regions of fancy, we may find in the works of Leeuwenhoek, that great man largely in advance of his time, a wide generalization deduced from observations made with indifferent lenses, or ill prepared objects. That was about the end of the seventeenth century. In the fourth decade of the eighteenth century (before 1745, the year of his "calling to a new and holy office"), we find that learned visionary, Emanuel Swedenborg, in a tract "written in Latin before his illumination"[1] on the Red Blood, asserting that "the blood contains all organic forms from the primary spiritual, to the ultimate angular, and in this respect is the compendium and complex of all the forms of nature." That "each blood globule contains within it, and carries in its bosom, details more numerous than the eye can ever discover, or the mind conceive." And further he lays down this postulate, that "the red globule admits of division into six lesser and pellucid globules. For six lesser globules have been actually seen in one large blood globule, and, indeed, the mode in which they are united, and the relative position they occupy to each other, have been described and drawn, and wax models representing the whole have been constructed. Moreover, the several lesser globules have been seen in the act of separating from the compound body; and furthermore, experience has shewn that these prior or simpler globules are themselves divisible into still lesser ones, and even into least, when [illogically enough he says] sight can follow them no longer." "All these particulars must be received as true, because the observer, Leeuwenhoek, and the witnesses to his observations, are worthy of credit." And finally, as far as we care to quote this author, Chapter V. opens with the declaration that, besides the lesser pellucid globules which constitute the large globules of the red blood, there are also in the blood certain angular or saline and earthy parts.

Rather more than a century later, in 1846, Dr. Carpenter[2] assures us that the blood corpuscles "are to be regarded as nucleated cells, conformable in general character with the isolated cells which constitute the whole of the simplest plants." And of this there can be, says this eminent author, no reasonable doubt. From this we should infer that they have no power of reproducing themselves; and the recent observations of Dr. Barry and other microscopists seem to confirm the statement made long ago to that effect by Leeuwenhoek. The first change said to take place is the appearance of delicate radiating lines between the nucleus and the periphery, dividing the disk into several segments, usually six in number. The margin is soon observed to be crenated by indentations at corresponding points, and these indentations become deeper until a complete separation takes place, setting free six young cells or disks which seem to have been formed around the border of the nucleus of the parent cell. And therefore should we expect to find, as

[1] Translated by John James Garth Wilkinson.
[2] Principles of Human Physiology, 3d ed., p. 107.

we do meet with, every size intermediate between the small newly generated disk and the full-sized corpuscle. To sum up, therefore, with regard to the appearance in question of the red blood disk being constituted of six parts (Leeuwenhoek[1]) in the usual state, or divisible into seven parts (Barry, Carpenter, and others), six around the assumed nucleus, it may be fairly stated that now-a-days the crenation of the red disk is regarded as the evidence of parting life, or of the earliest post-mortem change.

If we now turn to *form*, we meet with two varieties of colored corpuscles in vertebrated animals, the elliptical and the circular, the former occurring in *fishes* (with the exception of a few Cyclostomata, which have circular disks), all amphibia, and birds, and, among mammals, in the Camel and Auchenia;[2] and the latter characterizing the blood of man and other mammals with the exceptions noted. The red blood disks of all mammals are, according to Rollett, smooth, and give no indication of any difference in the index of refraction of their several parts.[3] In birds, amphibia, and fishes, however, the colored corpuscles do not appear homogeneous, but offer a prominence on either side of the long edge when viewed in profile, which corresponds to a central area somewhat whitish or less deeply tinted when seen on its face. This prominence is absent in Amphiuma.[4] It appears, then, continues Rollett, that we may divide the blood corpuscles of animals into "two classes, the nucleated and the non-nucleated, for the behavior of the central spot is so different from the remaining substance of the corpuscle, but shows so great an agreement with the structure termed the nucleus in other animal cells, that we designate this structure as the nucleus of the blood corpuscle." On this point, as we shall see, Rindfleisch offers a conclusive observation. And we may add our own experience as with Amphiuma, in the staining with magenta and carmine of the large granular nuclei of the colored blood corpuscles in even measure with the nuclei of the giant ciliated epithelial elements of the mouth. Yet before proceeding further it is necessary to recall attention to the fact that nucleated colored corpuscles characterize the embryonal blood of man and other mammals.

Gulliver classifies all blood disks of vertebrates as the Pyrenæmatous and the Apyrenæmatous, which divisions correspond to the oviparous and non-oviparous groups, the corpuscles of all the latter being circular in form and flattened, with the exception of the Camel's and Auchenia's, which are oval but still apyrenæmatous, whereas the colored disks of all oviparous vertebrates are nucleated. But Henle, even in 1843, while claiming the human red blood disks among the nucleated in their early condition, utters words which strongly remind us of the modern Beale in reference to formed and germinal matter. Henle[5] says, "We have yet to show, with regard to the higher vertebrates, as man, that when the coloring matter accumulates in the blood corpuscles, and when their envelope is flattened, their nuclei are dissolved or resorbed, and that consequently the cellule of the blood, arrived at its perfection, is a simple vesicle containing a fluid."

The color, and especially the structure of the disks which we are considering, have greatly occupied the scientific world from the time when Malpighi saw "globulos quasi subluteos in quibus non vidi motum rota-

[1] Transactions Philosophiques, 1674; see Henle, Anat. Générale, p. 497.
[2] Rollett, p. 265.
[3] Rollett, p. 266.
[4] Author.
[5] Henle, Anat. Générale; Paris, 1843, p. 491.

tionis" until the present time.[1] In man they have been declared to be of a pale-yellow or a reddish-yellow color, quite in opposition to experience elsewhere, which is that no mass of either yellow or reddish-yellow could ever so behave with light as to assume the scarlet hue deep and glowing of the human blood. For this reason we hold the yellow hue in doubt, and because no observer can ever with unaided eye ascertain the true color of a single colored disk. The microscope, in enlarging surface, dims its brightness, and also dilutes or attenuates its color, so that while it is altogether possible by apparatus below the stage of the microscope to compensate for loss of light by the higher powers, no art can restore color to a disk, probably crimson, when its image is amplified ten, a hundred, or a thousand times in diameter. Now what holds good for mammifers, obtains in all probability with the lower orders.[2] In evidence of this statement it will be remembered that the eosenic red of a single corpuscle intensifies if a number of corpuscles lie superposed in the field. In the lower animals the color of the blood, or, what is the same, of the colored corpuscles, varies to some extent, and were our appreciation of the *cachet* of the tints of crimson sufficiently discriminating, we might by recognizing them also identify, probably in this way alone if dimensions failed to aid, the several possessors of the colored disks, as Dugès asserts that the bloods of many creatures have been distinguished from one another by their characteristic aura.

Leaving color we encounter structure, and here most probably begin the real difficulties of our task. The corpuscles of the blood of mammalia, by reason of their size, do not afford facilities for the solution of the first problem as to whether the disks have a limiting capsule. But the larger blood cells of amphibians would seem to offer themselves as easy solvents of the question, especially the frog's ovate bodies and the giant colored and largely nucleated corpuscles of Amphiuma tridactylum. Unfortunately, however, microscopists are not in accord upon several structural points, nor is disagreement confined to them alone. We have already noticed the composite structure attributed to the "red globules" (as they were called by Leeuwenhoek), referred to by Swedenborg, and reproduced by Drs. Barry and Carpenter. But for more than a century, from the time of Malpighi himself, who saw the circulation of the blood and its corpuscles, the idea of an investing membrane and its contents was debated in science, having been suggested most probably by Robert Hooke's description of the cellular structure of plants in 1667. Indeed we find it implied by writers of a later period, as Mandl in 1839,[3] or distinctly expressed, as by Donné,[4] who, in 1844, speaks of the blood disks as being "flattened vesicles containing a more or less fluid substance." And he speaks of the substance of which the vesicle is composed as being of an albuminous nature.

Kölliker[5] entertained the same view, for in 1854 he says "that the blood corpuscle is constituted of a very delicate but nevertheless tolerably firm, and at the same time elastic, colorless cell membrane." "The corpuscle is consequently a vesicle, whence the name blood-cell is to be preferred." More recently Gulliver, in the Proceedings of the Zoological Society, February 25, 1862, speaks of "the embryonic corpuscles as

[1] Malpighi, Opera Posthuma, p. 92.
[2] Quarterly Journal of Microscopical Science, 1855. [C. J.]
[3] Mandl, Traité pratique du Microscope; Paris, 1839, p. 113.
[4] Al. Donné, Cours de Microscopie; Paris, 1844, p. 66.
[5] Manual of Microscopical Anatomy; Philadelphia, 1854, p. 704.

constituting a first or temporary set." "These cells," continues the distinguished author, "both in structure and size are the true analogues of the red corpuscles of oviparous vertebrates." And again "the corpuscles of the second set are those which replace the first, and, subject to waste and supply, are the red corpuscles of the blood during the greater part of utero-gestation and until death. The corpuscle is not homogeneous, but is composed of a colorless membranous part or basis, separable by water, with a fluid viscid matter in which color resides."

We cannot here enumerate the great authorities who have entertained or who still entertain the view above quoted; it suffices for us to present it as one which still finds favor with many. The contrary opinion, more bold in its enunciation, because at variance with accepted laws, finds its able exponent in Beale, among others. He says in "How to Work with the Microscope," p. 134, that "by drawing a needle across the thin glass under which the red corpuscles are placed, they may be divided into many small globules. This proves that the red blood corpuscles consist of a mass of soft viscid matter, the outer part of which is somewhat hardened." Or again, several red corpuscles may, under certain circumstances, coalesce, and even crystallize without exhibiting indications of any cell wall whatever. Besides the subdivision into small spheres or spheroids, the same author[1] adduces other reasons, as that the mass of the germinal matter in the case of the nucleated blood corpuscle of the frog and other vertebrates, or a portion of it, may pass right through the red viscid material of which the outer part of the corpuscle is composed, without the rupture of any membrane. Now Rollett[2] declares that, "after the cautious addition of water," it frequently happens in the nucleated ellipsoids that the nucleus changes its position in the corpuscle with a jerk, and then lies eccentrically in the corpuscle. Finally Beale remarks, "when water is added to blood corpuscles they swell up just as a piece of jelly would swell up, but they do not burst as is generally stated."

Whichever way we incline, we have fair grounds for our belief, as also excellent authorities and companionship; but we are disposed to favor the modern view, as being more consistent with observations upon fresh blood and blood dried on slides, and as being entirely in accord with what we know elsewhere of protoplasm, especially in the pyrenæmatous colored corpuscles.

Of the leucocytes, what shall we say? What are they? Whence come they? Whither go? And do they appear pathologically under the form of pus? Whatever interest may attach to the *red disks*, the white equally claim our attention; for, in the language of Rindfleisch,[3] "the red blood corpuscles would form one pole, the amœboid colorless cells the other, and, between the two, formation and deformation go continually to and fro."

The colorless morphological constituents of the blood, according to Max Schultze, are of several kinds;[4] "first, round cells, less than the red blood corpuscles, composed of a thin layer of cell substance investing one or two nuclei. With these may be associated other forms, equalling in size the ordinary red blood corpuscles, and like the former possessing

[1] The Microscope in Practical Medicine, 1867, p. 170.

[2] Loc. cit., p. 277.

[3] Experiment. Stud. u. d. Histol. d. Blutes, v. Dr. Edward Rindfleisch; Leipzig, 1863, p. 41.

[4] Rollett, p. 288.

nuclei. Lastly, finely and coarsely granulated amœboid cells are met with, and various intermediate forms between them." "In freshly drawn blood," continues Rollett, "these last appear as more or less rounded or irregularly shaped forms. At a temperature of from 95° to 104°. F., lively movements resembling the creeping motions of an amœba occur." "Such cells," says Frey,[1] "have been seen to pass out (after the manner of an amœba) through the walls of the capillaries (A. Waller, Cohnhein [and Col J. J. Woodward, M.D., in his beautiful photograph of inflammation of the mare's stomach]), towards or through the lining tissue, and to take up into their contractile cell-like bodies small particles, such as molecules of indigo, aniline, cinnabar, and carmine, the finest milk globules, and even extravasated colored blood corpuscles."

These inclusions of red corpuscles by amœboid leucocytes are not to be confounded with "the so-called cells containing blood corpuscles," mentioned by Rindfleisch,[2] who declares that "the opinion only appears to me admissible here that a secondary separation of fibrin has formed about a group of blood corpuscles." And Preyer[3] saw portions of the red blood corpuscles of extravasated blood in amphibia, taken up by white blood corpuscles, and thus explained the nature and mode of occurrence of the bodies that had been previously called blood corpuscle-holding cells. But themselves or their analogues are to be found, according to M. Cornil,[4] in the blood of the spleens of patients who have died in the third week of typhoid fever, in the form of white globules in large numbers, inclosing red globules to the number of five, six, or even more in a single cell. Although, continues the reporter, the existence in the blood of these large cells containing red globules is nothing new, nevertheless Cornil is the first to insist upon their multiplication in typhoid fever.

Besides the remarkable properties of the white corpuscles above mentioned, others equally surprising are most readily brought to the notice of the observer. The migration of these bodies in a drop of the blood of the newt or frog, placed in a moist cell, beyond the limits of a coagulum to the circumvallating zone of serum, has been shown by Rollett[5] to occur, and he points out the advantage of this method of isolating the amœboid cells, and suggests that "the migration of the individual cells renders it clear that the individual movement is the chief if not the exclusive cause of their emigration." In confirmation of the alleged amœboid inclusive movements of the white corpuscles, the famous experiment of Cohnheim was done in the manner to be indicated: "A small quantity of finely granulated coloring matter, suspended in water, is to be injected by a Pravaz's syringe on several consecutive days into one of the large lymph spaces which lie under the skin of the frog. On examining a drop of blood, a considerable number of the colorless cells will now be seen to be stuffed with the granules."[6]

Four chief matters of interest in connection with the white corpuscles deserve at least passing notice in this place:—

(1) Their origin, which elicited from Rollett[7] the following summary: "Whether the colorless corpuscles always undergo multiplication within

[1] The Microscope, etc., Dr. Heinrich Frey; New York, 1872, p. 102.
[2] A Text-book of Pathological Histology; Philadelphia, 1872, p. 65.
[3] Rollett, p. 290.
[4] Monthly Microscopical Journal, May, 1876, p. 233.
[5] Rollett, loc. cit.
[6] Frey, The Microscope; New York, 1872, p. 231. [7] Rollett, loc. cit., p. 292.

the blood itself, and by what mode of cell genesis they multiply, are still open questions. It is certain," says that author, "that a large number of white corpuscles are added to the blood, not only during the period of development and growth of the animal organism, but also throughout life by the agency of the lymph current, the corpuscles of this current originating in localized germ-producing organs situated external to the blood (lymphatic glands)." Our own observation of the blood of a young seventeen inch long Amphiuma tridactylum, goes to show that the leucocytes had greatly multiplied in number after the head had become much inflamed in consequence of repeated attempts of a larger Amphiuma to swallow that which was the subject of experiment. Again, the pathological process of leukæmia,[1] so called, seems to be associated with an increase in volume of the spleen, and often at the same time of the lymphatic glands, although but seldom caused by the enlargement of the latter organs alone.

(2) Their transformation into red or colored corpuscles, for which in the frog we have the authority of Recklinghausen, Sclarewsky, and Golubew, but which if it do occur in the human subject, happens very slowly in convalescent leucocythæmic patients. The intermediate forms met with in the blood of the frog were known, but not correctly referred, by the earlier observers, as Wharton Jones and Hensen.[2] Besides we have also the authority of Bizzozero and Neumann[3] for the statement that the transformation of the lymphoid cells may be seen in the red bone-marrow of mammalian animals; and this bone-marrow itself, is, according to some authors, as Frey, one source of origin of the colorless cells of the blood.

(3) Their relative proportion to the red corpuscles cannot be regarded as fixed, for the averages of the best observers, as Welcker and Moleschott, differ sensibly. Thus, Welcker states his average as one white corpuscle to three hundred and thirty-five (335) red, while Moleschott finds one in three hundred and fifty-seven (357). The proportion also varies with age, their number being greater in the young than in adults, and, according to Hirt, they increase during full digestion, but diminish in fasting. And lastly, Hirt[4] "found the number of white corpuscles seeming greatly to differ in various parts of the vascular system; thus in the splenic vein they are as one to sixty (60) red; in the splenic artery as one to two thousand two hundred and sixty (2260); in the hepatic vein one to one hundred and seventy (170); and in the portal vein as one to seven hundred and forty (740).

(4) The migration of the lymphoid cells into the space of the connective tissue, and their relation to the pus cells. In this connection the great name of Cohnheim immediately rises before us. His observations. confirming the older views of A. Waller, possess, says Frey, "an extraordinary range, and have rapidly called forth an entire literature." In connection, however, with this subject, interest may be awakened by reference to the conviction of Mandl, expressed in 1839, that "we[5] demonstrate the perfect identity of globules of pus and of the fibrinous globules of the blood . . . as we understand it." Henle[6] soon after declared that he had "shown that the nuclei of the lymph corpuscles go through the same metamorphoses as those of pus; they differ by their volume

[1] Frey, op. cit., p. 232.
[2] Rollett, loc. cit., p. 291.
[3] Frey, op. cit., p. 307.
[4] Hirt, apud Rollett, p. 290.
[5] Mandl, op. cit., p. 120.
[6] Henle, op. cit., p. 509.

only." And in a note added by Prof. Stricker to the American edition of his Histology, 1872, page 47, that eminent scientist assures us that his "later investigations have shown that the process of division of pus cells can be directly observed under the microscope." But most curious are the general conclusions arrived at by Mandl in his Memoir, read before the Société Médicale d'Émulation, June 3, and found in the Gazette Médicale de Paris, July, 1840. They are as follows:[1] "1. The fibrinous globules of the blood, the globules of mucus, and those of pus, are identical. 2. All the globules are the product of the coagulation of the fibrin in the serum, which has transuded through the walls of the bloodvessels. 3. The liquid in which the globules swim constitutes the difference between pus and mucus. 4. If the fibrinous globules remain fixed to the surface of the membrane where they are secreted, they become the nuclei of epidermoid cells, which constitute the elements of the epidermis. 5. If, on the contrary, the fibrinous globules remain free on the surface of the membrane, they are expelled by the organism, and form an element of pus and mucus. 6. These two elements are simply filtered blood; that is to say, they contain the elements of the blood, except the globules, the serum at the same time undergoing chemical alteration."

In opposition, in Virchow's Archiv, Bd. lxii. S. 47 (1876), we find that Professor J. Arnold discovers "the white corpuscles themselves in the act of escaping in an inflamed tissue, through the cement substance, that is the stigmata, and that gelatine and vermilion also pass through the cell wall at the position of the stigmata and cement substance."[2] How well Mandl saw, but failed to interpret, may be understood by a comparison of his conclusions, in 1840, with the beautiful researches of Waller, Cohnheim, Woodward, and J. Arnold, in later years.

As soon as the corpuscles of the blood were an assured acquisition to science, they very naturally attracted and received great attention for themselves as wonderful objects on account of the rôles which they were supposed to play in physiology, and because of the very great importance which their possibly positive recognition in the several classes or genera of animals would necessarily assume in medico-legal investigations. The interest in these elements has continued unabated until the present hour, and all the resources of art have been put in requisition to insure not only their detection under all and difficult circumstances, but also to make certain their specific identification. To this end many devices have been put in practice, such as bettering the modes of presenting the corpuscle in every state for investigation; the use of high powers, and improvement in the quality of the objectives employed in their study; their exact mensuration, relative and positive; their enumeration; the establishment of the normal proportion existing between the colored corpuscles and the leucocytes; and, finally, an appeal to the spectroscope, which might discover in the minutest trace of cruorin the ghost of even one departed red disk which had yielded up its form.

With regard to the first of these efforts, the best condition under which these little bodies may be thoroughly investigated, we have nothing to urge in behalf of the various preservative fluids which have had fortune with microscopists, being satisfied that, as far as our own experience extends, the question is not as to how excellent a substitute may be pro-

[1] Microscopical Journal, 1841, p. 80. [2] Monthly Microscopical Journal, June, 1876.

vided for the liquor sanguinis, but rather how greatly superior is the liquor sanguinis itself. Of course, this view involves the necessity for a limited range of observation by each of the scattered students of comparative hæmatology, for it is not given to each and several of these to push inquiry with all the special bloods in their living or immediately recent state. Amphiuma, proteus, and the musk deer, are not only strictly local, but are very widely separated geographically; the standard bloods, however, are almost everywhere accessible, for the frog, the horse, the cat, and the dog, fishes and newts, are almost *passim;* and thanks to itinerant menageries and fixed zoological collections, many strange animals are assembled in unfamiliar districts. Besides, we can have, or ought to have, human blood at our own finger ends.

For use as artificial standards, the corpuscles, mounted dry on the under surface of the thinnest cover, are decidedly of the greatest service, for as all mammiferous red disks, and perhaps a few of the other smaller forms, confessedly shrink but little, and as in medico-legal investigations the expert has usually to deal with dried hemorrhagic stains or clots, like is directly compared with like, as could also easily be done if exceptionally immediate blood were obtainable as food for the microscope or spectroscope. But let it be well understood that we greatly prefer the recent, the living blood, if possible, on every account. Form is unaltered; all physical qualities are unchanged; characteristic behavior at the normal temperature, and in presence of such reagents as magenta, acetic acid, water, or an alkali, is conspicuous; and heat, or life, or light, or other modes of motion, start up in microcosmic play.

With regard to the methods of perfecting objectives, we propose to be silent, leaving the problems which these create to our own Tolles and Spencer, to Powell and Lealand, and the Becks, to Wales and Ross, to Zeiss and Seibert, to Scheik and Plössel, and last, but not least, to Woodward, and Wenham, and Keith. But we accept the superb work of the great artist-scientists of the day, and we rejoice in the splendor of their instruments.

More germane to our subject is an expression of opinion as to the degree of amplification requisite for exhaustive and conclusive study of the blood; and at once the microscopist is struck with the necessity for the employment of high powers for all research excepting only what might be called a general view of the field. For enumeration of the corpuscles, a medium power suffices; for acquiring a collective idea of blood, either in the vessels, or freshly drawn, placed on a slide, and subjected to changes of temperature and the action of reagents, moderate amplification offers the best hope; but for higher views, higher objectives must be put to use, and an immersion[1] $\frac{1}{16}$, $\frac{1}{30}$, $\frac{1}{50}$, or even $\frac{1}{75}$ inch objective, of the best maker, and of large angle, in the hands of a really good manipulator, will afford a demonstration of all that is at present attainable. If size alone be in question, it is unscientific to hope to distinguish, under low powers, human red corpuscles with an average diameter of $\frac{1}{3230}$ of an inch from those of the dog with an average measurement of $\frac{1}{3576}$ of an inch;[2] and it is worse than unscientific to obtain large images by forcing low objectives with short eye-pieces. We hold, therefore, that, with the results of mensuration of the blood-corpuscles, all the conditions attending the measurements must be given, if their authors

[1] Woodward.

[2] Prof. Theo. G. Wormley, letter to author, May 16, 1876; New Measurements.

expect to win and maintain the confidence of the world of science. Thus Prof. Theo. G. Wormley regards the condition of his material as attached to the under side of a thin cover; the thickness of that cover; the temperature of the slide, by averaging measurements made at all seasons of the year; the position of the collar of adjustment of the objective; and the identical disposition of the light in all observations; and he employs a $\frac{1}{32}$ inch objective. Besides, he has repeatedly tested the assumed value of each of the divisions of his micrometer, and confined himself to the employment of that or those only of which he has assured himself as being absolutely true to the standard.

Before proceeding further with mammalian blood, let us not lose sight of what is most probably quite as important as size, namely, the *cachet*, or stamp of individuality possessed by each of the kinds of blood disks, the determination of which is or will be a matter of necessity in microscopy simple or applied. In support of this view, we would ask a serious contemplation of two similar bloods on the same slide, of a feline, and of an herbivore. Difference in diameter under a tenth objective may not be a striking feature of the contrast, but a sentiment of something beyond, for which we may not as yet account to ourselves, immediately but stealthily takes possession of our senses.

It was with the intention of arraying in the opposition of contrast human blood and the dog's, and human blood and the cat's, that that well-known scientist, Dr. Joseph G. Richardson, flowed drops of blood from different animals so nearly in contact on a glass slide, that portions of the two drops appeared in the same field, and could be photographed together.[1] Dr. C. Leo Mees, assistant to Prof. Wormley, in Columbus, Ohio, modified this plan, spreading the bloods by the present writer's method, a half field upon a slide, and another half field upon a cover, which is then turned down and placed directly upon the slide. The method consists in catching a small drop of blood with the true edge of a well-ground slide, and gently drawing it, that slide being inclined, over the face of another slide or cover. Dr. Mees obtains half fields by scraping. "In this manner," says Prof. Wormley, "we obtain the bloods virtually upon the same plane for powers under the $\frac{1}{5}$ inch." But after inspection, we fear much for the assumption "virtually upon the same plane," even for a large angle, $\frac{1}{5}$ inch; nor are we prepared to admit the availability of such slides for Prof. Wormley's very fine $\frac{1}{32}$ inch objective. And besides we are persuaded that the cachets of the two bloods, if they ever can be recognized as distinctive, will hardly bear comparison when the disks of one blood present their attached and flat surfaces towards the observer, and the other disks their free, and, in the instance of circular mammalian disks, their excessively concave sides.

As measurement of the blood-corpuscles, colored and pallid, is nowadays scrutinized by the keen eyes of microscopical science and of the law, we propose to offer in this paper some of the best modern and most careful observations. But as a preparation for accepting the results of so much patience and labor, and great skill, we beg to present a list of measurements of the human blood in Hodgkin's and Lister's Microscopical Observations, contained in the Philosophical Magazine, vol. xi. p. 133, as given in the Microscopical Journal for 1841. It is as follows:—

[1] American Naturalist, May, 1876.

Hodgkin	$\frac{1}{3000}$	of an inch.	
Jurine	$\frac{1}{5240}$	"	
Jurine	$\frac{1}{1940}$	"	(Second measurement.)
Bauer	$\frac{1}{1700}$	"	
Wollaston	$\frac{1}{5000}$	"	
Young	$\frac{1}{6060}$	"	
Kater	$\frac{1}{4000}$	"	
Ditte	$\frac{1}{6000}$	"	
Prevost and Dumas	$\frac{1}{4076}$	"	
Nasse[1]	$\frac{1}{3703}$	"	

A difference so remarkable as that between the $\frac{1}{1700}$ and the $\frac{1}{6060}$ of an inch, inspires little confidence in the means and the method, and such series of observations must be now regarded as curiosities of microscopy.

Passing over the great hiatus separating the year 1840 from the time in which we live, we would deal directly with four papers relating to comparative measurements of the colored blood disks of vertebrates, two of which are from the pen of Joseph G. Richardson, M.D., of Philadelphia, one by Col. J. J. Woodward, M.D., U. S. A., and the last and most recent, by Prof. George Gulliver, F.R.S. In the first of these[2] "On the value of high powers in the Diagnosis of Blood Stains," an article of much merit, the author "endeavored to work out a conclusive answer to the question," "whether practically we can or cannot discriminate the stains of human blood from those made by the blood of oxen and sheep." "In conclusion," he says, "I submit that the results of my experiments above narrated, *prove* that since the red blood-globules of the pig ($\frac{1}{4230}$ of an inch), the ox ($\frac{1}{4267}''$), the red deer ($\frac{1}{4324}''$), the cat ($\frac{1}{4404}''$), the horse ($\frac{1}{4600}''$), the sheep ($\frac{1}{5300}''$), and the goat ($\frac{1}{6366}''$), are all so much smaller than even the ordinary medium size of the human red disk, as measured in my investigations, we are now able, by the aid of high powers of the microscope and under favorable circumstances, to positively distinguish stains produced by human blood from those caused by the blood of any of the animals just enumerated; and this after the lapse of five years from the date of their primary production." "We may add to these, the average measurements, those of recent blood disks procured from a blood spot moistened with a one per cent. solution of common salt, and stained with a drop of [red ?] aniline solution. Thus the mean of ten corpuscles so treated was expressed by the fraction $\frac{1}{3425}$ of an inch. the minimum being $\frac{1}{3572}$ of an inch, and the maximum $\frac{1}{3125}$ of an inch."[3] Notwithstanding the expression of belief[4] that the red corpuscles "rarely contract," the author gives the maximum $\frac{1}{3231}$ of an inch, minimum $\frac{1}{3500}$ of an inch, and mean $\frac{1}{3355}$, of twenty recent red corpuscles of a white male aged thirty. But again, and on the opposite side, "the measurement of twenty corpuscles from part of the same specimen dried in a thin film upon a slide gave a maximum of $\frac{1}{2800}$, a minimum of $\frac{1}{3621}$, and a mean diameter of $\frac{1}{3182}$ of an inch."[5] And in the Oswego Greenfield trial, in June, 1876, Dr. Richardson testified that "drying diminishes all kinds of corpuscles."

The second paper, by Col. J. J. Woodward, M.D., U. S. A., almost exhaustive in reference, and most ably presenting the author's research and observations, appeared in the Monthly Microscopic Journal of February, 1875, five months after the valuable article just noted, and was suggested by it as the first line distinctly assures us. The title is as follows: "On

[1] Valentin's Repertorium, 1840.
[2] Monthly Microscopical Journal, September I, 1874.
[3] Loc. cit., p. 140. [4] Loc. cit., p. 137. [5] Loc. cit., p. 135.

the similarity between the red blood corpuscles of man and those of certain other mammals, especially the dog, considered in connection with the diagnosis of blood stains in criminal cases." The general tenor of this paper is a dissent from the conclusions reached by Dr. Richardson. Among the most striking points of stress of Col. Woodward, is the statement: "For myself, after repeated measurements of the blood of the dog and of human blood, I can only say that I find no constant difference between them, whether the fresh blood or thin layers dried on glass be selected for measurement. The mean of fifty corpuscles, taken at hazard, is seldom twice the same, and sometimes that of human blood and sometimes that of the dog's blood is a trifle the larger." In the same connection, a little further on, the author gives two tabulated records of measurements of red blood corpuscles, which very remarkably substantiate the afore-mentioned doctrine, the one being measurements of red corpuscles of human blood and the other of the same elements of the dog. In both cases the blood was obtained from five individuals; fifty corpuscles formed the basis of each observation; of the thirteen specimens of human blood measured, four were moist; of the nine of dog's blood, five were moist, and, singularly enough, "in each of the measurements" of the two bloods "the great majority of the corpuscles ranged from 12 to 17 divisions of the eye-piece micrometer: that is, from .00024 to .00034 [or $\frac{1}{4166}$ to $\frac{1}{2941}$] of an inch."

Before quitting this important field, Col. Woodward reminds the reader that "the variations between the mean diameters assigned to human blood by different observers are quite as great as the variations recorded by any of them between the blood of man and of the dog, or even greater." Although there is admitted to be "no constant difference" between the wet and the dry corpuscles, when fresh and used moist, or quickly dried on thin glass, it is denied that a differential diagnosis can be possible in the case of "blood dried *en masse*, when sprinkled on weapons, clothing, wood, etc., for no one will pretend that it can be any easier to make the diagnosis of such stains than it is in the case of moist blood or of thin films dried on glass."

In the same Journal, for May, 1875, Dr. Richardson replies to the strictures of Col. Woodward, but states explicitly his "full corroboration" of Col. Woodward's assertions, and declares that "there is at present (1875) no method known to science for discriminating, microscopically or otherwise, the dried blood of a human being from that of a dog, monkey, rabbit, musk-rat, elephant, lion, whale, seal, or in fact any animal whose corpuscles measure more than $\frac{1}{4000}$ of an inch in diameter." With this admission we would fain conclude our notice of the article, but fairness demands that the criticism of Col. Woodward, which we have reproduced, should be followed with an explanatory statement from Dr. Richardson, who says: "I wish to insist most emphatically that all my statements in regard to the diagnosis of blood stains are applicable to *stains* only, and not to *masses* of dried blood clot." And we here would call attention to the fact that in the case of State *v.* Lodicia and Albert Friedenburg, for the murder of Orlo Davis, tried in Utica, in November, 1875, Dr. Richardson testified[1] that he had examined certain spots on an axe helve and "had come to the conclusion that the corpuscles, or red blood disks, or globules, were those belonging to human blood." And again in the Greenfield trial, held in Oswego, in June, 1876, Dr. Richardson, after

[1] Utica Morning Herald, Friday, November 19, 1875.

examining dark spots on some chips belonging to the rail of a bedstead, testified as follows: "If the question is narrowed down to whether this is human blood or sheep's blood, I can say positively that this is man's blood. Because the corpuscles are such as could never circulate in the veins of a sheep."[1]

In opposition to such testimony and teaching, we find in Dalton's Treatise on Human Physiology, sixth edition, 1875, an expression of views which will meet with favor with the majority of advanced observers and thinkers. He says: "If a blood stain which in a watery solution gives the common spectrum of hæmoglobuline, be found to contain oval nucleated globules, this would show it to be the blood of a bird, reptile, or fish; and the oval form alone would show that it was not human blood. The question, therefore, whether a particular specimen be composed of human blood may often be decided with certainty *in the negative* by microscopic examination. But if the specimen contain circular globules without nuclei, it will be impossible to say positively in any instance that they belong to human blood, and not to that of some animal, such as the ape or dog, whose red globules nearly approach the human in size. In most of the domesticated quadrupeds the globules are smaller than in human blood, but in both the sloth and the elephant they are larger. If it were only required to decide whether a given specimen of fresh blood belonged to man or to the musk deer, for example, or even the goat, no doubt the difference in size of the globules would be sufficient to determine the question. But within nearer limits of resemblance it would be doubtful, because the size of the red globules varies to some extent in each kind of blood; and, in order to be certain that a particular specimen were human blood, it would be necessary to show that the smallest of its globules were larger than the largest of those belonging to the animal in question, or *vice versâ*. The limits of this variation have been tolerably well defined for human blood, but not sufficiently for many of the lower animals to make an absolute distinction possible. In the examination of stains or blood spots the difficulty is increased by the fact that the drying and subsequent moistening of the globules introduces another element of uncertainty as to their original size."

The last paper we propose to notice, and the most remarkable of its kind which science possesses, is entitled Observations on the Sizes and Shapes of the Red Corpuscles of the Blood of Vertebrates, with drawings of them to a uniform scale, and an extended and revised table of measurements, by George Gulliver, F.R.S.; and appeared in the Proceedings of the Zoological Society of London, for June 15, 1875. The "present purpose" of this extremely sagacious and very laborious production "is to give simply the averages, with brief explanatory comments, of numberless measurements all made by me," says Prof. Gulliver, "in the hope that they may be useful towards further researches of the same kind." "The measurements now recorded show the labors of many years," and consist of "many additions and revisions by new observations made since publication, upwards of a quarter of a century ago," of Prof. Gulliver's tables of measurements, which originally appeared in the Proceedings of the Zoological Society, in 1845.

Simple as is the announcement, the variety and originality of the observations, inferences, comparisons, and deductions would be very surprising were it not for a knowledge of the author, and of the fact that they are the

[1] Oswego Daily Times, Saturday evening, June 10, 1876.

developed germs of more than thirty years of toil. It is foreign to our purpose to follow this observer in all the paths that he has trod, but we confine ourselves to the matter of measurements, and in this circle alone we find abundant food for careful thought. Although the table of averages "has been deduced from innumerable and generally correct observations," the red corpuscles are found usually to differ among themselves in every field of vision, commonly to the extent of one-third larger or smaller than the mean. And when we reflect upon "the real variations in size produced in the bird alternating between rest and excitement; the probable diversities in some vertebrates at different times and seasons; the possible ones influenced by climate, rest, and violent exertion, periodical rise of temperature—as in python, sex, the system—whether arterial or venous, the ever-varying state of the liquor sanguinis,[1] pathological or septic changes," it need not surprise us that the author advances the proposition "or fact that in a single healthy species the corpuscles are so prone to minute variations of size that no two observers, nor even one observer, can be certain of obtaining precisely the same average measurements." Nevertheless "the relative value of the measurements, though probably not unexceptionable, may be entitled to more confidence as fair approximations to the truth."

The applications of what has preceded are extremely numerous, and if it had been possible to extend the scope of this paper their number would have been measurably increased. But we will here refer to but two, as perhaps the chief among them, the physiological and the medico-legal. In the former connection we encounter bodies doubtless of the same significance and function, but which differ strikingly in size, *cachet*, and form, and what is more remarkable the almost perfect correspondence established between a majority of[2] mammalian vertebrate and apyrenæmatous red blood corpuscles, having a discoid-biconcave form, and between oviparous vertebrate and oval pyrenæmatous blood disks. But while the oval blood disks of the exceptional camel conform in smallness of size to the circular corpuscles of other mammalia, they are non-conformable to the oval pyrenæmatous type of oviparous vertebrates. In general, with regard to size in apyrenæmata, we are called upon to admit as "a rule that the largest corpuscles occur in the large species, and the smallest corpuscles in the small species of a single order or family. It extends to the whole class of birds, but neither to reptiles, batrachians, nor fishes, except in some partial instances which seem to be rather indeterminate or accidental than regular."

After form and size, we naturally encounter number, which conspicuously has importance in relation to physiological or pathological action. If with Schmidt[3] we assume the blood mass in a human subject to weigh 10 kilogrammes (26 pounds 9 ounces, troy), then the sum of the blood cells would amount to 56 billions 570,000 millions. But this vast figure gives us no advantageous information, and its consequence fades out before the more practical efforts of Vierordt (1852) and Welcker (1853), the former of whom counted all the red disks in a known cubic measure of blood, by successive instalments poured and dried upon a slide, and reducing his calculation to one millimetre, obtained at first 5,171,400, and at a later period 5,055,000. Welcker, both by counting the red corpuscles in blood diluted with albumen, with the aid of a crossbar

[1] Also Kölliker, op. cit., p. 605.
[2] Gulliver, loc. cit., p. 482.
[3] Kölliker, op. cit., p. 569.

ocular micrometer, and by comparing diluted blood with an established color scale, fixed his estimate at 4,600,000 corpuscles in a cubic millimetre for a healthful person, 3,800,000 in an hysterical person, and 2,400,000 in a patient far advanced in consumption.[1]

We next refer to the advance made by Potain[2] in estimating the number of red corpuscles contained in a cubic millimetre, by counting a small portion of diluted blood, and to the labors of Mélassez[3] in blood counting. The hæmatimètre of Hayem and Nachet, recently described by Mélassez,[4] in the hands of Drs. E. L Keyes and L. A. Stimson, gave as an average of 33 counts the number of red corpuscles in one cubic millimetre as 4,990,550, a table derived from exceptionally good sources. "The above estimate," says Dr. Keyes, "of nearly 5,000,000 blood corpuscles for the cubic millimetre, I believe to be a high average for Americans in the neighborhood of New York, during the summer and autumn."

"To compare the above results with those obtained by other investigators, we find that Hayem[5] places the mean at about 5,000,000 more or less, for the blood of the finger of the healthy adult male; Vierordt[6] established as a mean for his own blood something over 5,000,000; Welcker[7] estimated it at 4,600,000; Cramer at 4,726,000. Mélassez gives the lowest average, placing the number at something less than 4,500,000, but he is not very definite upon the subject of healthy average, being more interested in his writings in a description of the instrument and in considerations of diseased blood." "In the most extreme cases of anæmia (excepting only two cases), Hayem never counted less than 3,000,000." In this respect my own experience, concludes Dr. Keyes, "has been similar to his."

In this last connection it deserves to be noted that Dr. Ordenstein, of Paris, suggests the possibility of a connection between leukæmia and hereditary syphilis, and in a case of his own exhibited Van Swieten's solution of bichloride of mercury for several months with surprising benefit.[8] And finally we feel constrained to mention the conclusions arrived at by Liègeois and Wilbouchewitch, as quoted by Keyes, and those of Dr. Keyes himself: (1) that syphilis diminishes the number of red corpuscles below the healthful standard; (2) that mercury in small doses, continued for a short or for a long period in syphilis, alone or with iodide of potassium, increases the number of red corpuscles in the blood, and maintains a high standard of the same; and (3) that mercury decreases the number of red cells when given in excess, especially in hospitals.[9]

Not less interesting, and certainly not less important, than the physiological, is the medico-legal application of what the microscope may teach. The law calls upon the expert to be thoroughly *au fait* in all that modern science has established with regard to the blood in its living and in its dead state, in its youth and adult periods, in its pathology, in its different and indifferent conditions of preservation, in dryness and in moisture, in heat and in cold; and the character, size, form, and number of the corpuscular elements in all attainable classes, orders, species, and families of the zoological series. Nor will his attainments be re-

[1] Kölliker, op. cit., p. 704. Note by Da Costa.
[2] E. L. Keyes, Am. Journ. Med. Sciences, January, 1876, p. 21.
[3] Ibid.
[4] Ibid.
[5] Gazette Hebdomadaire, Mai, 1875, p. 295.
[6] Kölliker, op. cit.
[7] Id. op.
[8] Medical Times and Gazette, November 13, 1875.
[9] Keyes, loc. cit.

garded as determinative, unless he be also master of micro-chemistry and familiar with absorption bands, and the expression of their wave length,[1] awakened in ghostly shapes in the spectroscope made an ally to the microscope. In short, accusing justice, and defensive crime or innocence, claim not only an absolute recognition of blood, as distinguished from all other matters or elements whatsoever, but the specific determination of the kind of blood, as well as a positive acquaintance with all the disturbing agencies of time, heat, dryness, and moisture, as affecting the *cruor* offered in condemnation or exculpation.

That microscopical science is so assured in her varied knowledge, even if its application be restricted within very narrow limits, cannot be asserted. From what has preceded it must appear that even for human red blood disks there is no fixed and established standard. The observations of Gulliver upon the variations in size of the red corpuscles in birds, under the different circumstances of excitement and repose, and the positive statement of Col. Woodward that "the blood disks of different persons, and even of the same persons at different times, vary more in size than the ordinary difference between human and dog's blood," are germane to this issue. But we would consider our task as glaringly incomplete if we failed to give becoming prominence to the averages of the most perfectly conducted work of our distinguished friend, Prof. Theo. G. Wormley, M.D., of Columbus, Ohio, and to state the expression of his variance with Col. Woodward's record, that "this is certainly contrary to *all* our own experience."[2] With Dr. Wormley's permission we offer a few of his measurements, which were furnished in the course of a recent correspondence:—

Human blood, average of all averages, including over 2000 measurements		$\frac{1}{3230}$	inch.
Dog,	average of averages	$\frac{1}{3576}$	"
Cat,	" "	$\frac{1}{4372}$	"
Rat,	" "	$\frac{1}{3652}$	"
Horse,	" "	$\frac{1}{4202}$	"
Jack,	" "	$\frac{1}{3507}$	"
Mule,	" "	$\frac{1}{3760}$	"
Ox,	" "	$\frac{1}{4119}$	"
Opossum,	" "	$\frac{1}{3142}$	"
Muskrat,	" "	$\frac{1}{3282}$	"

To these measurements the following is added as a note: "You are aware, I believe, that in getting our standard for micrometry every possible care was taken to have it correct. The ordinary scales are subject to considerable variation." In acknowledging the favor of the above, we beg to add that we have seen the whole record of the admirable averages of Prof. Wormley, the work of many years, which evidences the industry, the patience, the intelligence, and the scrupulous fidelity of the author.

It had been our purpose here to close this essay, but we pause to notice a paper by Col. Woodward, read at the Centennial meeting of the American Medical Association, which has for its title "The Application of Photography to Micrometry, with special reference to the Micrometry of Blood in Criminal Cases." By the plan proposed, which may be destined to supersede all others, and to abate the value of all former more laborious and less exact procedures, "the blood is placed on a glass stage

[1] Sorby, Monthly Microscopical Journal, May 1, 1875.
[2] Letter to Author, March 28, 1876.

micrometer, and photographed with any convenient power;" and in the elegant photographs offered in illustration "both blood and micrometer appear sharply defined. The measurements are then made on the negative." "For the purpose of soaking out blood stains" the able author prefers to "use a strong solution of caustic potash, as described by Virchow in 1857." The development of this mode of fixing the averages is insisted upon by the distinguished microscopist, who now regards the eye-piece micrometer as a "snare and a delusion," and claims that the negative on glass possesses an exactness which may be expressed in millionths of an inch, and may be tested at leisure by any number of persons. And another point of interest attaches to these light pictures, *i. e.*, the equal delineation, *under high powers*, of all corpuscles, whether large or small, of normal shape or deformed.

Remotely related to the subject are the "micro-photographs of the blood in disease," recently issued by Dr. E. Cutter, of Cambridge, and Dr. G. B. Harriman, of Boston.

In conclusion we beg to remark that the copious literature of this extensive subject, the Microscopy of the Blood, is nowhere complete in this country outside of its Capital; and that, in spite of the distinguished courtesy of the eminent scientist in charge of the Army Medical Museum, this treasury of knowledge cannot always be made available by the busy professional men who reside at a distance.

Discussion on Dr. Johnston's Paper.

After the reading of the preceding paper, Dr. J. G. Richardson, of Philadelphia, said:—I should like to offer an explanation in reference to my testimony at the trial in Herkimer County, New York. I did not state absolutely that the corpuscles were those of human blood, but that they *corresponded* to those of human blood. Microscopy has long been employed as an aid to the administration of justice; and serves easily, for example, to distinguish human blood from that of a fish, because the corpuscles in the former are round, while in the blood of the fish they are oval. At first the legal authorities demanded no more than this, but when the facilities for microscopic examination became more complete, we began to draw a still sharper line of distinction—that is, one between human blood and that of the musk-deer or goat. Now, with improved immersion lenses, we are, I contend, able to distinguish between the blood of a human being and that of an ox, horse, or other animal having corpuscles of similar size; but when circumstances do not thus narrow down the demands of justice in a trial, I would never attempt to discriminate the kind of blood with certainty. Our power to aid justice in this matter is due to the fact that when a man is arrested with blood-stains on him, and tries to explain how they got there, he generally alleges that he has been in a position to be stained by the blood of some animal; and then, by microscopic examination, we can distinguish between human blood and that of the ox, pig, or sheep, to which the suspicious stains are usually attributed.

Dr. Johnston said:—That would not be a positive recognition of human blood-disks, which is what the courts desire; for the blood might be that of a dog or guinea-pig, and not of a human being.

Dr. Henry D. Didama, of Syracuse, N. Y., said:—I know of a case in which the question presented was, whether the blood was that of an ox or human being; and though, in such a case, an expert might not be able to say that the blood was human, yet he could prove that the statement made by the criminal

was untrue. I wish to ask Dr. Johnston whether, if he should find a blood-disk $\frac{1}{3200}$ of an inch in diameter, he would be willing to swear that it was not the blood of an ox or pig? because, as I understand the matter, the blood-globules of the ox and pig never reach that size.

Dr. JOHNSTON said:—Where the variations were not exceedingly great, I would be unwilling to give an opinion.

Dr. RICHARDSON said:—I understand Prof. Johnston to say that, on examining some unlabelled slides of man's, dog's, and guinea-pig's blood, offered him by Dr. J. J. Woodward, he found himself unable to discriminate between them. This is what I should expect; but I would like to ask Prof. Johnston whether, if a person of unproved veracity should bring him a specimen of blood, covering the name, and telling him that it was from a sheep, and he should find, on measurement, that the corpuscles averaged $\frac{1}{3200}$ of an inch in diameter, he would not feel justified in contradicting the statement?

Dr. DIDAMA said:—Would not Prof. Johnston, if he found blood-disks of $\frac{1}{3200}$ of an inch, be certain that they could not have circulated in the capillaries of a sheep?

Dr. JOHNSTON said:—It is a very difficult question to answer, as both vessels and corpuscles are extremely elastic, and capillaries constantly allow corpuscles to pass through them, which are larger in diameter than the capillaries themselves.

Dr. JAMES TYSON, of Philadelphia, said:—It seems to me that this is an ambiguous mode of putting the question, because, as Dr. Johnston says, even if we could assert that a particular blood was not that of a sheep, we could not say that the corpuscles could not have circulated in the capillaries of a sheep, on account of the property of elasticity correctly attributed by Dr. Johnston to both corpuscles and capillaries, to say nothing of the varying size of the latter. But the fact is that the peculiar organization of the human mind has something to do with the drawing of conclusions. Some persons would feel at liberty to draw the same conclusions from the premises named by Drs. Didama and Richardson as they, while others would not. I, for example, would be willing to say that such blood was *more likely* to be that of an ox or sheep than that of a man, while I might not be willing to say absolutely "this is the blood of an ox or sheep," and not that of a man.

Dr. S. E. CHAILLÉ, of New Orleans, said:—I desire to ask two questions: I think we all agree with Prof. Johnston that human blood corpuscles cannot be positively distinguished from all others; but frequently, in court, some witnesses assert that a particular stain is human blood, while others say, no, it is the blood of an ox. The Court asks the microscopist, "Can you decide whether this is human blood or that of an ox—or, let us say, of a sheep?" Is it Prof. Johnston's opinion that such a decision could be made, and would he, under such restricted circumstances, feel justified in saying whether the blood was that of a sheep or of a man?

Dr. JOHNSTON said:—I could answer negatively, that the blood was not human.

Dr. CHAILLÉ said:—My second question is this: I have been told that certain specimens of blood have been offered to Dr. Richardson, and that he has been able to discriminate between them. I would ask of Dr. Richardson if such is the fact?

Dr. RICHARDSON said:—Such was the case, not in one series of specimens only, but in two.[1] The number of samples in each series was three; one being the blood of an ox, one that of a sheep, and the other human blood. I was able to distinguish between them by microscopical examination alone.

Dr. JOHNSTON said:—There is more than the mere diameter of the corpuscles to be considered; we must consider the *cachet*, or the stamp of individuality of the blood.

[1] See American Journal of the Medical Sciences, July, 1874; also, London Monthly Microscopical Journal, Sept. 1874.

Dr. L. S. Joynes, of Richmond, Va., said:—I would like to ask Dr. Richardson as to the relative ease of distinguishing between different bloods when dry and wet. Can we restore dried corpuscles so certainly as to distinguish whether they are human or otherwise? It is difficult to know how far blood corpuscles on the clothing are altered from their original condition.

Dr. Richardson said:—My experience has been that in dry blood-stains there is a shrinkage of from five to ten per cent. That is, that the fresh blood corpuscles of man average $\frac{1}{3200}$ of an inch, while in stains they measure from $\frac{1}{3400}$ to $\frac{1}{3700}$. The blood corpuscle of an ox, which averages $\frac{1}{4267}$ of an inch, when fresh, would measure $\frac{1}{4600}$ or $\frac{1}{4700}$ after re-moistening.

Upon the conclusion of the discussion, Dr. Richardson said:—As strong evidence of the presence of a true cell-wall in red blood corpuscles, upon the existence of which the value of my method for the diagnosis of blood-stains largely depends, I invite the members of the Section to inspect, beneath a Holmes's class microscope, a red disk of the Amphiuma tridactylum, in which the imperfectly crystallized cell-contents occupy the upper end, while the oval granular nucleus fills the inferior extremity, leaving the membranous capsule relaxed and wrinkled longitudinally, hanging like part of a half-flaccid balloon between them. (Fig. 1.) I also show, as corroborative testimony, a slide

Fig. 1 (× 400).

Fig. 2 (× 400).

bearing a blood corpuscle of the Menobranchus, which displays, within what I deem to be its limiting membrane, a single crystal made up of all the cell-contents except the nucleus.[1] (Fig. 2.)

[1] See Trans. Am. Med. Association, vol. xxi., 1870, p. 271.

THE EXCRETORY FUNCTION OF THE LIVER.

BY

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I HAVE selected as the subject which I shall have the honor to present to the Section on Biology, the Excretory Function of the Liver, for the reason that it seemed to me better to discuss a question concerning which I had made personal and original investigations, than to recite the observations of others, however interesting and important the latter might be. I have ventured to assume that the views which I have to offer are not without importance; and they are certainly not so familiar as many other topics which I might with propriety have selected. I shall, therefore, endeavor to bring to your notice, in the simplest manner possible, what I have myself learned with regard to the liver as an organ of excretion.

It is now well known that the liver has a variety of important functions with which physiologists are more or less completely acquainted. It produces a substance which is converted into sugar and is carried away in the torrent of the circulation. It secretes bile, which performs an important office in digestion. In addition to the digestive function of the bile, I think I have shown that this fluid serves as the vehicle for the elimination of at least one excrementitious principle, which is discharged in a modified form in the feces. If the liver serve as an organ of excretion, it is evidently of great importance, from a pathological as well as a physiological point of view, that we should have an accurate knowledge of the mechanism of this function. For a long time, many pathological conditions have been attributed to defective or perverted action of the liver; still, the terms, "liver complaint," "biliousness," etc., have failed to convey any definite pathological notion, and it is probably true that the liver has been accused of numerous sins of omission and commission without any positive scientific reason. Many medical writers have assumed, in an indefinite way, that the liver possesses an excretory function; but, so far as I know, no physiologist had ever described any definite excrementitious substance eliminated by this organ prior to my observations in 1862.

There are certain general laws applicable to secretions and to excretions, which it is important to consider in discussing the probable functions of the bile:—

I. Secretions have some useful purpose to serve in the economy, and, as a rule, they are not discharged from the body in health. Excretions have no function in the economy and are discharged from the body.

II. The flow of secretions from the glands is usually intermittent, occurring when their function is called into action. The flow of excretions is usually either constant or remittent.

III. The production of excretions depends upon the general process of disassimilation, which is constant. The production of secretions has no

relation to disassimilation, but is connected with processes which usually take place at intervals.

IV. The elements of secretion, which give to secreted fluids their characteristic physiological properties, are formed *de novo* in the glands themselves out of materials furnished by the blood, and they do not pre-exist ready-formed in the circulating fluid. The elements of excretion preexist in the blood, being taken up by the lymph or by the blood from the tissues, and they are separated from the blood by organs which have no part in their actual production; except that excrementitious substances may be changed one into another, as creatine into creatinine, or uric acid into urea.

V. When secreting organs are removed or destroyed, there is no vicarious production of the peculiar elements of the secretions; these elements do not accumulate in the blood; and the system suffers simply from the absence of the function of the special secretion. When excreting organs are removed or destroyed, there may be a vicarious elimination of the excrementitious principles by other organs, or the system may suffer toxic effects from the accumulation of excrementitious matters in the circulating fluid.

VI. The characteristic elements of true secretions are generally reabsorbed by the blood; but they are taken up in a modified form, so that they are not to be recognized in the circulating fluid. Elements of excretion are with difficulty reabsorbed by the blood after they have once been separated by the proper organs.

The applications of the foregoing general laws may be readily made to the pancreatic juice as contrasted with the urine, which two fluids we may take as types respectively of secretions and of excretions. Before we make an application of these laws to the bile, we may consider the simple question as to whether it can be shown that this fluid has a useful function to perform as a secretion. If the bile have no such function, an animal would live and maintain its normal condition if the bile were diverted from the intestine and discharged from the body. This question has been made the subject of experimental observation by simply cutting off the bile-duct, and making a fistula into the gall-bladder, by which the bile is discharged. The operative procedure involved is not difficult, but is very apt to be followed by fatal peritonitis, so that few experiments of this kind have succeeded. In the experiments which have succeeded, in the hands of Schwann, Bidder and Schmidt, Nasse, Bernard, and myself, the dogs have lived for thirty or forty days, dying with all the symptoms of inanition. In one remarkably successful experiment performed by myself, the dog lived for thirty-eight days, had a voracious appetite, and died at the end of that period after having lost about four-tenths of his weight. In this experiment, the bile-duct was ligatured in two places, and the intermediate portion was exsected. A fistula was then made into the fundus of the gall-bladder, which was kept open. The animal ate well the very day of the operation, and there was very little peritonitis. The only observation in which contrary results were obtained is one made by Blondlot.[1] In this case, a fistula was made into the gall-bladder after the bile-duct had been divided. The animal lived for five years, and, after fifteen days following the operation, was in good flesh and apparently suffered no inconvenience from the discharge of the bile

[1] Blondlot, Essai sur les fonctions du foie et de ses annexes, Paris, 1846, page 55, *et seq.*; and Inutilité de la bile dans la digestion, Paris, 1851.

from the fistula. During the first fifteen days the animal licked the bile from the fistula, but this was afterward prevented by a muzzle. After a time he made no attempt to lick the bile. Blondlot attributed the emaciation which occurred during the first fifteen days to this licking of the bile. When the animal died, more than five years after the operation, an examination of the parts was made in the presence of several physicians and students of medicine, and no communication could be found between the bile-duct and the intestine. From this observation, Blondlot concluded that the bile had no function in digestion, and that it was a purely excrementitious fluid; and he assumed that the cause of death in other experiments of a similar kind was the licking of the bile as it flowed from the fistula. In my own case of biliary fistula, in which the dog died after thirty-eight days, the animal was prevented by a muzzle from licking the bile.

The only point to consider, as it seems to me, in this single experiment of Blondlot, is whether or not a communication had been re-established between the bile-duct and the intestine. If such a communication existed, it would be easy to explain the survival of the animal. The following experiment, which I undertook for a different purpose, satisfied me upon this point:—

I attempted to estimate, in a dog, the entire quantity of bile discharged in the twenty-four hours. With this object in view, I cut down upon the bile-duct, emptied the gall-bladder, secured a canula in the duct, and attached a rubber-bag to the canula, for the purpose of collecting the bile. Twenty-three hours after the operation, the bag was in place and nearly full of bile. Just before the end of the twenty-four hours, however, the animal ruptured the bag, and the experiment, as far as its original object was concerned, was a failure. I then simply pulled the canula from the wound and set the animal at liberty. In about four weeks, after the wound had closed and the feces had become of normal color, the animal, being in a perfectly normal condition, was killed, and the parts were carefully examined in the presence of several assistants. It is well known that, in dogs, ducts that have been divided have a remarkable tendency to become re-established. In this case, inasmuch as no bile was discharged externally, and the feces were of normal color, it was certain that the bile was discharged into the intestine. Nevertheless, I searched for more than an hour for the communication before it was discovered. The only reasonable way, as it appears to me, to reconcile the single experiment of Blondlot with those of other observers, is to suppose that, in his observation, a communication between the bile-duct and the intestine had become established, which he failed to find. The difficulty which I experienced in finding the communication in my own observation led me to conclude that a communication existed in the case reported by Blondlot, which he did not discover.

It is in accordance with my own observations, as well as with those of other physiologists, to conclude that the bile is a secretion, and that it has a function to perform in connection with the digestive process, which function is essential to life.

Assuming that the bile has an important and an essential office in digestion, is it not possible that it may also serve the purpose of elimination, and contain elements of excretion? This is a view which has not been advanced by physiologists, who have regarded the bile either as a secretion or an excretion, and have not imagined that it could serve both functions. Before I take up the experimental facts bearing upon this

question, I propose to consider the arguments to be drawn from a study of the composition of the bile, and its discharge into the intestine. It was this idea which first led me to investigate the physiological relations of cholesterine.

The bile certainly has an important function as a secretion; and its flow, although not intermittent, is more abundant during the process of intestinal digestion. The peculiar biliary salts (the glycocholate and the taurocholate of soda) are formed in the liver and do not pre-exist in the blood. When the structure of the liver is invaded by disease so as to interfere with the production of bile, the biliary salts do not accumulate in the blood. The biliary salts are reabsorbed in a modified form in the intestine; for the quantity of one of their elements (sulphur) found in the feces is very much less than the amount discharged into the intestine.

On the other hand, with regard to one constant constituent of the bile (cholesterine), we do not know that it has any function in connection with digestion. The secretion of bile is continuous, although its flow is increased during digestion. Cholesterine, while it is an invariable constituent of the bile, exists in the blood and in certain of the tissues of the body.

The questions to determine experimentally with regard to cholesterine are the following:—

Is cholesterine produced in any of the tissues of the body?

Is cholesterine separated from the blood by the liver?

When the liver undergoes structural change in disease, does cholesterine accumulate in the blood?

Is cholesterine reabsorbed in the intestine or is it discharged, either unchanged or in a modified form, in the feces?

These are the questions which I endeavored to answer by a series of experimental investigations, made in the spring of 1862, and published in October of the same year, in the American Journal of the Medical Sciences.

Process for the Estimation of Cholesterine in the Blood.—The following is the process which I fixed upon, after a number of trials, for the quantitative analysis of the blood for cholesterine: The entire blood, serum and clot, is evaporated to dryness. The dry residue is then pulverized in an agate mortar, and treated for from twelve to twenty-four hours with ether, in the proportion of about one fluidounce of ether to one hundred grains of the original weight of blood. This is filtered, and the ethereal extract, which contains cholesterine and fats, is evaporated. The residue of this evaporation is then extracted with boiling alcohol, in the proportion of one fluidrachm for one hundred grains of the original weight of blood. This extract is filtered while hot, and the filtrate is evaporated, leaving the cholesterine and a certain quantity of saponifiable fats. To remove the saponifiable fats, add to the residue a weak solution of potash, and allow it to remain for about two hours; then dilute with water, filter, and wash the filter with water until the liquid which passes through becomes neutral. Dry the filter; wash it with ether; evaporate the ether; extract the residue with hot alcohol as before; evaporate the alcoholic extract, and the residue will consist of cholesterine, perfectly pure, as can be determined by means of the microscope. Using this process for the determination of cholesterine, a number

of observations were made upon dogs, from which I select the following as typical, the results having been repeatedly confirmed.

OBS. I. *Experiment showing an Increase in Cholesterine in the Blood passing through the Brain.* (The dog was not etherized.)

Blood from the carotid, 140.847 grains, contained 0.108 grain of cholesterine, or 0.768 part of cholesterine per 1000.

Blood from the internal jugular, 97.811 grains, contained 0.092 grain of cholesterine, or 0.947 part of cholesterine per 1000.

The increase in the proportion of cholesterine in the blood in passing through the brain was 23.309 per cent.

This observation, which was frequently repeated with the same general result, seems to show that the blood gains cholesterine in its passage through the brain. It is well known that cholesterine is always present in nervous substance—not in a crystalline form, but in a state of molecular union with nitrogenized and other matters. In order to verify this fact, I examined the brains of two subjects who had been killed instantly by accident while in perfect health, in one case finding a proportion of cholesterine of 7.729 parts per 1000, and, in the other, 11.456 parts per 1000.

The experiment just detailed was made with a view of determining whether or not the brain gives up cholesterine to the blood as it circulates through this organ; and the following experiment was made to determine whether the venous blood of other parts contains an excess of cholesterine. Theoretically, the blood of the femoral vein should contain a little more cholesterine than arterial blood, this excess being derived from the nerves of the extremity, although the increase would probably be not so great as in the blood of the internal jugular, which comes almost exclusively from the great nervous centre.

OBS. II. *Experiment showing an Excess of Cholesterine in the Blood of the Internal Jugular and Femoral Veins over the Arterial Blood.* (The dog was not etherized.)

Blood from the carotid, 143.625 grains, contained 0.679 grain of cholesterine, or 0.967 part of cholesterine per 1000.

Blood from the internal jugular, 29.956 grains, contained 0.046 grain of cholesterine, or 1.545 part per 1000.

Blood from the femoral vein, 45.035 grains, contained 0.046 grain of cholesterine, or 1.028 part per 1000.

The increase in the proportion of cholesterinc in the blood in passing through the brain was 59.772 per cent.

The increase in the proportion of cholesterine in the blood in passing through the lower extremity was 6.308 per cent.

This experiment confirms the previous observation upon the increase of cholesterine in the blood in passing through the brain, and it shows, in addition, that the blood gains cholesterine in other parts. Inasmuch as the nervous tissue is the only tissue in the extremities which contains cholesterine, it is probable that the excess contained in the blood of the femoral vein over the arterial blood was derived from the nerves.

It occurred to me that cases of old hemiplegia would present favorable conditions for verifying in the human subject the observations made on the lower animals. It has been ascertained that, when the function of nerves is permanently abolished, they soon become degenerated and their nutrition modified; and it seems probable that, if cholesterine be one

of their important products of disassimilation, the amount of cholesterine in the blood from paralyzed parts should be very small. Taking the blood, for example, from the paralyzed arm of a hemiplegic, this blood, coming from paralyzed parts, should contain less cholesterine than the blood from the sound arm. Of course, the blood from the arm contains no blood which has passed through the brain, which is assumed to be sound upon the paralyzed side. I examined, therefore, the blood from both arms in three cases of hemiplegia in the Charity Hospital on Blackwell's Island:—

CASE I. Sarah Rumsby, æt. 47, is affected with hemiplegia of the left side. Two years ago she was taken with apoplexy and was insensible for three days. When she recovered consciousness she found herself paralyzed on the left side. She says she had epilepsy four or five years before the attack of apoplexy. She has now complete paralysis of motion on the affected side, with the exception of some slight power over the fingers. Sensation is not affected. The speech is perfect and her general health is good.

CASE II. Anna Wilson, æt. 23, is affected with hemiplegia of the right side. Four months ago she became unconscious, and recovered in one day, with loss of motion and sensation on the right side. She is now improving and can use the right arm slightly. The leg is not so much improved because she will make no effort to use it.

CASE III. Honora Sullivan, æt. 40, is affected with hemiplegia of the right side. About six months ago she became unconscious, recovering the next day with paralysis. The leg was less affected than the arm from the first. Her condition is about stationary as regards the arm, but the leg has somewhat improved.

A small quantity of blood was drawn from either arm in these three cases. In each instance it was drawn from the paralyzed side with difficulty, and but a small quantity could be obtained.

The specimens were all examined for cholesterine, with the following results:—

OBS. III. *Quantities of Cholesterine in the Blood of the Paralyzed and the Sound Sides in three cases of Hemiplegia.*

CASES.	Blood.	Cholesterine.	Cholesterine per 1000 parts.
	Grains.	Grains.	
Case I. Paralyzed side . . .	55.458	....	The watch-glass contained 0.031 grain of substance, but the most careful examination with the microscope failed to show crystals of cholesterine.
Sound side	128.407	0.062	0.481.
Case II. Paralyzed side . .	18.381	....	Same as Case I.
Sound side	66.396	0.062	0.808.
Case III. Paralyzed side . .	21.842	....	Same as Case I.
Sound side . . .	52.261	0.031	0.579.

The conclusion from the experiments upon dogs and the three observations upon the human subject is inevitable, that cholesterine is produced in the substance of the brain and in the nervous tissue generally, as this substance is not contained in the muscular tissue or in any other parts except the crystalline lens, the liver, and the spleen. The question now to determine is the relation of cholesterine to the nervous system. Is it

one of the products of disassimilation of its tissue? If this be the fact, cholesterine is an excrementitious product, and it must be separated from the blood by some organ or organs and discharged from the body. Inasmuch as the bile always contains cholesterine, we naturally look to the liver as the organ for its elimination; for it is not found in the product of any other gland.

I employed essentially the same method in studying the question of the elimination of cholesterine as that used in determining the seat of its production, analyzing the blood going to and coming from the liver. Upon this point I made a number of observations, the general results of which were invariable. The following experiment is a type of these observations:—

OBS. IV. *Experiment showing that Cholesterine is separated from the Blood in its Passage through the Liver.* (The dog was etherized.)

Arterial blood, 159.537 grains, contained 0.200 grain of cholesterine, or 1.257 part of cholesterine per 1000.

Blood of portal vein, 168.257 grains, contained 0.170 grain of cholesterine, or 1.009 part of cholesterine per 1000.

Blood of hepatic vein, 79.848 grains, contained 0.077 grain of cholesterine, or 0.964 part of cholesterine per 1000.

The loss in the proportion of cholesterine in arterial blood in passing through the liver was 23.309 per cent.

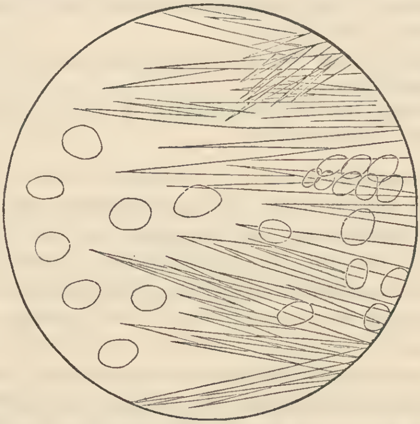
The loss in the proportion of cholesterine in the portal blood in passing through the liver was 4.460 per cent.

The bile always contains a certain proportion of cholesterine, which I found, in a specimen taken from the gall-bladder of a subject who had been killed instantly while in perfect health, to be 0.618 part per 1000. As I have demonstrated that the blood gains cholesterine in its passage through the brain and probably also from the general nervous tissue, that cholesterine is separated from the blood in its passage through the liver, and that cholesterine is invariably found in the bile and is discharged into the intestine, it seems to be proven that one of the functions of the liver is to eliminate this principle. If it can be shown that the cholesterine thus separated from the blood by the liver is discharged from the body, the fact that it is apparently produced in the nervous tissue and is taken up by the blood would point strongly to the conclusion that cholesterine is an excrementitious principle and is one of the products of disassimilation of the nervous matter.

Stercorine.—I made repeated examinations of the normal feces, with a view of determining the presence of cholesterine and its quantity. Although it is often stated by authors that cholesterine exists in the feces, I was unable to find it after the most careful examination, and I subsequently failed to discover any writer who had actually extracted it from the normal dejections. The process which I employed was essentially the same as that used in examinations of the blood, except that the extracts were decolorized by filtering through animal charcoal, and the alcoholic extract was treated with potash for one or two hours at nearly the boiling point. Treated in this way, the feces gave an extract which was non-saponifiable, but which did not crystallize for several days. It fused at a temperature of about 97° Fahr., while cholesterine fuses at 293°. After a few days, delicate, needle-shaped crystals began to appear, gradually increasing in number and breadth, and, as they became

broader, becoming split at the points and edges. These crystals presented all the characters of the crystals of a substance extracted from the serum of the blood by Boudet, in 1833 (Annales de Chimie et de Physique), which he called *séroline.* In some of my earlier observations upon the blood, I obtained these crystals; but I came to the conclusion that the so-called séroline was not a normal constituent of the blood, but was formed during the process used for the extraction of cholesterine. With this view, finding the so-called séroline a constant constituent of the normal feces, I called the substance stercorine, regarding it as one of the excrementitious principles of fecal matter. Crystals of stercorine are shown in the accompanying figure (Fig. 1). The rounded drops are probably composed of the same substance, as they disappear when the crystallization is complete. The idea that these crystals obtained from blood result from the transformation of cholesterine, is strengthened by the fact that the cholesterine of the bile is changed into stercorine in its passage through the alimentary canal. Stercorine, like cholesterine, is soluble in ether, very soluble in hot alcohol, and strikes a red color

Fig. 1.



Stercorine from normal human feces ($\frac{4}{10}$ inch objective).

with strong sulphuric acid. The points of distinction are the low fusing point and the form of the crystals.

I obtained from the feces of the twenty-four hours of a perfectly healthy man 10.417 grains of stercorine. It is estimated by Dalton that the total quantity of bile in twenty-four hours is 16,940.00 grains, and the total quantity of cholesterine, according to my estimate of 0.618 parts per 1000, is 10.469 grains, giving a difference between the estimated quantity of cholesterine and the actual quantity of stercorine extracted from the feces of only 0.052 of a grain. This sustains the idea of the change of the cholesterine of the bile into the stercorine of the feces.

Observations made by myself and others seem to show that the change of cholesterine into stercorine is incidental to the process of digestion. Cholesterine is found in quantity in the feces of hibernating animals and in the meconium, when, of course, there is no intestinal digestion, but when the bile is none the less discharged into the alimentary canal. I made an examination of human meconium and found cholesterine in the proportion of 6.245 parts per 1000, and no stercorine. I examined

the human feces in a case of simple jaundice from obstruction of the bile-duct, the feces being clay-colored, and found neither cholesterine nor stercorine. Nineteen days after, when the jaundice had disappeared and the color of the feces was normal, I found stercorine and no cholesterine. In the feces of a dog which had been deprived of food for forty-eight hours, I found stercorine and a small quantity of cholesterine. As far as can be learned from these facts and observations, then, it seems that the cholesterine of the bile is discharged in the feces unchanged when no digestion takes place, but that it is discharged in the form of stercorine under the ordinary and normal conditions of the digestive process.

Pathological Relations of Cholesterine; Cholesteræmia.—A knowledge of the relations of urea to nutrition bears so directly upon the pathology of renal diseases, that the pathological relations of any newly-discovered excrementitious principle assumes at once the greatest importance. If it be true that cholesterine, like urea, is a product of disassimilation, and that it is eliminated by the liver as urea is eliminated by the kidneys, we should expect to find, in cases of serious structural disease of the liver, an accumulation of cholesterine in the blood, or cholesteræmia, as we have uræmia in certain stages of extensive organic disease of the kidneys. It has long been observed, indeed, that, although simple jaundice due to resorption of the coloring matter of the bile is usually a trivial affection, there are cases of extensive change in the structure of the liver in which there is apparently a toxic condition dependent upon the presence of some excrementitious or poisonous substance in the blood. Pathologists have examined the blood in such cases with a view of ascertaining the nature of the supposed poisonous matter. Frerichs and others repeatedly examined the blood in cases of grave jaundice, expecting to discover the biliary salts or acids, but they never detected any substance which would react with Pettenkofer's test.[1] Becquerel and Rodier examined the blood in a case of jaundice and found "cholesterine excessively abundant," but they did not recognize the significance of this fact.[2] In such cases pathologists have looked for the biliary acids and their derivatives, and not for cholesterine. In order to throw some light upon the pathology of grave jaundice, Müller,[3] Kunde,[4] Lehmann,[5] and Moleschott[6] have extirpated the liver in frogs and kept the animals alive for several days, or even two or three weeks. On examining the blood, these physiologists failed to discover the biliary salts. They made no analyses of the blood for cholesterine. I hope to be able to show conclusively, by observations upon cases of disease of the liver in the human subject, that there may be an accumulation of cholesterine in the blood, or cholesteræmia, and that this occurs in certain cases of serious structural disease of the liver.

In cases of simple jaundice, there is resorption of the coloring matter of the bile from the excretory passages.

In cases of grave jaundice, which almost invariably terminate fatally, there is cholesteræmia, or accumulation of cholesterine in the blood.

1 Frerichs, Diseases of the Liver; New Sydenham Society, London, 1860, vol i. p. 95.
2 Becquerel et Rodier, Traité de chimie pathologique; Paris, 1854, p. 210.
3 Müller, Manuel de Physiologie; Paris, 1851, tome i. p. 122.
4 Kunde, De Hepatis Extirpatione; Berolini, 1850.
5 Lehmann, Physiological Chemistry; Philadelphia, 1855, vol. i. p. 476.
6 Moleschott, Comptes Rendus; Paris, 1855, tome xl. p. 1040.

There are cases of structural disease of the liver, in which there is no jaundice, but nevertheless there is cholesteræmia.

In the following cases, having first determined the proportion of cholesterine in normal blood, I examined the blood for cholesterine with reference to the points just stated:—

Obs. V. *Proportion of Cholesterine in Normal Blood.*

Male, æt. 35 . .	0.445	part of cholesterine	per	1000.
" " 22 . .	0.658	"	"	"
" " 24 . .	0.751	"	"	"

Obs. VI. *Case of Jaundice dependent probably upon Duodenitis.*—This case presented the symptoms of simple jaundice from temporary obstruction of the bile-duct. June 21, 1862, 212.428 grains of blood were taken from the arm. The proportion of cholesterine per 1000 was 0.508, which was within the limits of health, according to the results obtained in my examinations of normal blood. The feces, which were clay-colored, were examined, and I found neither cholesterine nor stercorine. July 11, the patient had entirely recovered; there was no jaundice; and the feces had become normal.

Obs. VII. *Case of Grave Jaundice with Cirrhosis.*—This case presented intense jaundice, ascites, great general prostration, and, toward the close of life, symptoms of blood-poisoning. The patient was admitted to the Charity Hospital on Blackwell's Island, June 16, 1862. On June 21, 50.776 grains of blood were taken from the arm. This blood contained a proportion of 1.850 part of cholesterine per 1000, an increase of 146.338 per cent. over the maximum quantity obtained from normal blood. The patient died June 27, 1862. There was double vision six days before death, and stupor for the last three or four days. The liver, examined after death, was in a condition of cirrhosis. The gall-bladder was contracted and contained but about two drachms of bile. The fibrous substance of the liver was increased in quantity, and the liver-cells were shrunken. The feces were taken a few days before death. The amount was small, only 272.1 grains in twenty-four hours, and contained 0.077 of a grain of stercorine. I found 10.417 grains of stercorine in the feces of the twenty-four hours in a healthy male.

Obs. VIII. *Case of Cirrhosis with Ascites and considerable affection of the General Health.*—In this case there was general prostration, confining the patient to the bed. After a tapping, the liver was explored and found to be considerably diminished in size. 117.193 grains of blood were taken from the arm, containing a proportion of 0.922 of a part of cholesterine per 1000, an increase of 22.769 per cent. over the maximum proportion obtained in my examinations of normal blood. In this case there were no nervous symptoms.

Obs. IX. *Case of Cirrhosis with Ascites and slight Constitutional Disturbance.*—This patient had suffered from ascites for eighteen months and had been tapped about thirty times. He is immediately relieved by tapping and goes out the next day. July 1, 1862, 251.567 grains of blood were taken from the arm, which gave a proportion of cholesterine of 0.246 of a part per 1000, or 44.719 per cent. less than the minimum obtained in my examinations of normal blood.

The cases just detailed, taken in connection with my observations upon animals, are certainly very striking. In the case of simple jaundice, which recovered, the proportion of cholesterine in the blood was within the limits of health. In the case of ascites, the patient not suffering much disturbance, the proportion of cholesterine in the blood was considerably below the normal standard. In the case of grave jaundice, which terminated fatally with symptoms of serious disturbance of the nervous system, the proportion of cholesterine in the blood was enor-

mously increased, being nearly three times greater than the maximum obtained in my examinations of normal blood. In the case of cirrhosis with considerable affection of the general health, the proportion of cholesterine in the blood was considerably above the maximum obtained in my examinations of normal blood.

Literature bearing upon the "New Excretory Function of the Liver," since the Publication of my Observations in 1862.

October, 1862.—My researches were published in the "American Journal of the Medical Sciences."

1868.—A translation of my memoir into French was published in Paris and presented to the Academy of Sciences for the Monthyon prize.

1869.—The commission from the French Academy of Sciences reported upon my observations and awarded an "honorable mention" with a "recompense" of fifteen hundred francs.

1869.—Grollemund (Thèse de Strasbourg) made observations upon the injection of the biliary salts into the blood in large quantity in dogs, and noted certain disturbances of the nervous system.

1869.—Tincelin (Thèse de Strasbourg) made observations in which he failed to obtain any marked nervous disturbances following the injection of the biliary salts into the blood in dogs.

1869.—Pagès (Thèse de Strasbourg) injected the bile-duct in dogs with a solution of sulphate of iron, which he thought destroyed the epithelium of the liver and interfered with its eliminative function, producing accumulation of cholesterine in the blood.

1870.—Feltz and Ritter (Journal de l'Anatomie, Paris, 1870) confirmed the results obtained by Pagès with the sulphate of iron. They found no marked effects following the injection of the biliary salts, taurine, or glycochol into the veins. They also injected cholesterine in soap and water. The cholesterine was not dissolved, and masses of cholesterine were found in the small pulmonary vessels, producing death by embolism.

1872.—Picot (Journal de l'Anatomie, Paris, 1872) noted an accumulation of cholesterine in the blood in a case of acute, yellow atrophy of the liver, which terminated fatally. He found a proportion of cholesterine in the blood in this case of 1.804 part per 1000, more than double the maximum which I obtained in examinations of normal blood.

1873.—Koloman Müller (Archiv für experimentelle Pathologie und Pharmakologie, Leipzig) made an elaborate series of experiments upon dogs. No serious or marked results followed the injection of the biliary salts or taurine into the blood. He rubbed cholesterine with glycerine and made a solution in soap and water. He injected 2.16 fluidounces of this solution, containing about 69 grains of cholesterine into the veins. In five experiments he produced "a complete picture of the symptoms of grave jaundice."

Conclusions of Koloman Müller.—"It appears to me to be certain that those cerebral symptoms which accompany severe jaundice and many diseases of the liver, the general manifestations of which have been called 'cholæmic intoxication,' are produced by an abnormal accumulation of cholesterine in the blood. This accumulation of cholesterine is contingent upon that alteration of the tissue of the liver, which, in such cases, it suffers more or less."

1875 and 1876.—Feltz and Ritter (Journal de l'Anatomie, Paris, 1875 and 1876) in opposition to their former experiments, conclude that the biliary salts injected into the blood produce grave changes, mainly in the blood corpuscles. The corpuscles become diffluent, change their form, the hæmaglobine transudes and crystallizes, and the power of absorption of oxygen progressively diminishes.

The general results of observations bearing upon the physiological relations of cholesterine, made since 1862, are confirmatory of my observa-

tions. As regards cholesteræmia, the experiments of Müller are the most important. Indeed, they supply the only missing link in my chain of experimental evidence; and they show conclusively that the symptoms of "grave jaundice," which I connected with cholesteræmia, may be produced by the artificial introduction of cholesterine into the circulation.

As an inevitable result of my observations, confirmed by others and extended by Koloman Müller, I can now confidently repeat the conclusions which I published in 1862.

Conclusions.—I. Cholesterine exists in the bile, the blood, the nervous matter, the crystalline lens, and the meconium, but does not exist in the feces in ordinary conditions. The quantity of cholesterine in the blood of the arm is from five to eight times more than the ordinary estimate.

II. Cholesterine is formed, in great part if not entirely, in the substance of the nervous matter, where it exists in great abundance, from which it is taken up by the blood, and constitutes one of the most important of the effete, or excrementitous products of the body. Its formation is constant, it always existing in the nervous matter and the circulating fluid.

III. Cholesterine is separated from the blood by the liver, appears as a constant element of the bile, and is discharged into the alimentary canal. The history of this substance, in the circulating fluid and in the bile, marks it as a product destined to be gotten rid of by the system, or an excretion. It pre-exists in the blood, subserves no useful purpose in the economy, is separated by the liver and not manufactured there, and, if this separation be interfered with, accumulates in the system, producing blood-poisoning.

IV. The bile has two separate and distinct functions dependent on the presence of two elements of an entirely different character. It has a function connected with nutrition. This is dependent on the presence of the glycocholate and taurocholate of soda, which do not pre-exist in the blood, subserve a useful purpose in the economy and are not discharged from it, are manufactured in the liver and peculiar to the bile, do not accumulate in the blood when the function of the liver is interfered with, and are, in short, products of secretion. But it has another function connected with depuration, which is dependent on the presence of cholesterine, which is an excretion. The flow of bile is remittent, being much increased during the digestive act, but produced during the intervals of digestion for the purpose of separating the cholesterine from the blood which is constantly receiving it.

V. The ordinary, normal feces do not contain cholesterine but contain stercorine (formerly called *séroline* from its being supposed to exist only in the serum of the blood), produced by a transformation of the cholesterine of the bile during the digestive act.

VI. The change of cholesterine into stercorine does not take place when digestion is arrested, or before this process commences; consequently, stercorine is not found in the meconium or in the feces of hibernating animals during their torpid condition. These matters contain cholesterine in large abundance, which also sometimes appears in the feces of animals after a prolonged fast. Stercorine is the form in which cholesterine is discharged from the body.

VII. The difference between the two forms of jaundice with which we are familiar, the one characterized only by yellowness of the skin and comparatively innocuous, while the other is attended with very grave symptoms and is almost invariably fatal, is dependent upon the obstruction

of the bile in the one case, and its suppression in the other. In the first instance, the bile is confined in the excretory passages and its coloring matter is absorbed, while, in the other, the cholesterine is retained in the blood and acts as a poison.

VIII. There is a condition of the blood dependent upon the accumulation of cholesterine, which I have called cholesteræmia. This occurs only when there is structural change in the liver, which incapacitates it from performing its excretory functions. It is characterized by symptoms of a grave character referable to the brain and dependent upon the poisonous effects of the retained cholesterine on this organ. It occurs with or without jaundice.

IX. Cholesteræmia does not occur in every instance of structural disease of the liver. Enough of the liver must be destroyed to prevent the due elimination of cholesterine. In cases in which the organ is but moderately affected, the sound portion is capable of performing the eliminative function of the whole.

X. In cases of simple jaundice, when the feces are decolorized and the bile is entirely shut off from the intestine, stercorine is not found in the evacuations; but in cases of jaundice with cholesteræmia, stercorine may be found, though always very much diminished in quantity, showing that there is an insufficiency in the separation of cholesterine from the blood, though its excretion is not entirely suspended. After death, but a small quantity of bile is found in the gall-bladder.

Discussion on Dr. Flint's Paper.

After the reading of the preceding paper, the President, Dr. John C. Dalton, of New York, said:—We have all listened with the greatest pleasure to the paper which has just been read. Though the subject is not entirely new, yet we are almost exclusively indebted to Dr. Flint for the latest light we have upon it. Those of us who are devoted to physiology are forced to select some particular department, and are therefore apt to be more interested in the development of that department than of others; but I am certain that if we make any distinction between the departments of physiology, physiological chemistry must be given the lead, for the study of the ingredients of animal forms lies at the foundation of all physiological study. We cannot claim that we are thoroughly familiar with physiology until we know the component substances of bodies—what they are, how they get into them, and what they do after getting there; what changes they undergo; and under what forms they disappear. The changes and operations of these substances constitute nutrition. Cholesterine, if I recollect aright, was discovered nearly a hundred years ago in gall-stones, was found to be extractible by hot alcohol, and was supposed to be an accidental deposit in an accidental, morbid form. But slow progress was made, and further discovery, until investigation proved that cholesterine existed in the liver. It was afterwards found in fresh bile, and then, after a considerable lapse of time, was shown to be an ingredient of the blood. Some years subsequently a curious discussion arose as to whether the nuclei of the red blood globules were not formed of cholesterine, and it was afterwards discovered that both the blood globules and the liquor sanguinis contained this substance, which was ultimately found in morbid exudations in all regions. Then came Dr. Flint's investigations in regard to the comparative amounts of cholesterine going to, and coming from, the organs. In this way the investigation has gone on for nearly a hundred years, and I suppose that Dr. Flint himself would not consider that the entire history of cholesterine has been yet exhausted. There is a great deal of work still to be done in this field to ascer-

tain the amount of cholesterine in the blood, and the different conditions and circumstances which determine its precipitation in morbid products. As we now know, it also forms an ingredient in some common articles of food. The study of cholesterine seems to me of the greatest importance in regard to excrementitious matters. I think we sometimes misrepresent to ourselves the character of the excrementitious process, in describing it as a process of destruction. But excrementation and disassimilation are not destruction; when food is taken into the stomach it undergoes a change there, and a further change in the blood, and so on, until it finally appears as carbonic acid or as cholesterine. Then comes the process of excrementation. The food has accomplished its duty in the nourishment of the body, and is wanted no longer. We could illustrate the process very well by all the steps upwards from the sowing of a cotton-seed to the manufacture of the cotton into cloth, and the final use of the rags in the manufacture of paper, and then the return of all the products back again to the soil. There are difficulties in the way of these investigations, and therefore we ought to feel ourselves very much indebted to those who help them on.

Dr. J. G. Richardson, of Philadelphia, said:—Dr. Flint's experiments establish three important facts in science, and complete a physiological chain in regard to the formation of cholesterine in the brain, its entering the circulation and being abstracted from the blood by the liver, and its being carried thence by the bile into the intestines where it is converted into stercorine. I would ask Dr. Flint whether he has made any experiments in regard to the formation of cholesterine in ovarian fluid?

Dr. Flint said:—I have not done so, but cholesterine is liable to be found in almost all the pathological formations of the body.

On motion, the conclusions of Dr. Flint's paper were adopted as expressing the opinion of the Section.

REPORT OF INVESTIGATIONS MADE UNDER THE DIRECTION OF PROFESSOR ZAVARIKINI, AT THE INSTITUTE OF NORMAL HISTOLOGY OF ST. PETERSBURG.

COMMUNICATED BY

PROFESSOR RUDNEW.

9

I. The Structure of the Sweat Glands in Man, in the Horse, and in the Sheep.

By Dr. Galani.

The epithelium of the sweat glands in man consists of very minute, oval cells, which are polygonal, and which are disposed in two layers, immediately under which lies the membrane consisting of fusiform elements with oval nuclei. By means of reagents we discover that these cells are simply muscular fibres. In the sweat glands of the scalp we find that these muscular fibre cells are intermixed with cells having the characters of connective tissue.

The epithelium of the sweat glands in the horse and sheep forms only one layer, while in all other animals there are two layers. The cells of this epithelium, viewed from the surface, have an hexagonal appearance, while from the side they appear of cylindrical form. Beneath the epithelium of the horse lie only the cells of connective tissue character, and these cells are so disposed one to another as to resemble in form a house tile. Beneath the epithelium of the sheep is found a membrane which consists of muscular fibres.

For the investigation of sweat glands we have employed the lens under which we have been able to separate the particles of the glands, and for the recognition of the muscular tissue we have used the following reagents, viz., a 36 per cent. solution of caustic potassa, chloride of palladium, carmine, and picric acid.

II. Upon the Termination of Nerves in the Pleura of the Rabbit, Dog, and Cat.

By Dr. Lebedeff.

The nerves, after separating into different arms and branches, terminate in a network in which it is sometimes possible to discover nervous cells of various sizes. Beneath the endothelium the network is composed of regular rhomboidal nooses, within which are found sometimes the cells of the endothelium, in numbers from one to five. Between the cells of the endothelium are disposed the fibres, which sometimes envelop the cell, and sometimes enter the cell and surround the nucleus. The greater part of the nerves possess nerve cells between their fibres, in number from two to fourteen, and this is especially observed in the rabbit. Some nerves do not terminate in the network, but in pear-shaped cells.

III. Upon the Microscopic Anatomy of the Nervous Apparatus of the Bronchi and Lungs in the Frog, the Rabbit, the Dog, the Cat, the Horse, and the Mouse.

By Dr. Santchich.

There are within the pleura many nerves which permeate the parenchyma of the lungs. The nerves form a double network, one being anterior and the other posterior. In each network are disposed from one to seventy cells, and the distributions are to the walls of the bronchi, the walls of the vessels, and the pulmonic pleuræ. The greater number of these cells are bipolar, and some few of them are even multipolar. All the nervous fibres emerging from the cells ramify to different parts of the lungs, and form networks. The nervous fibres found in the bronchi form two networks, one of these serving for two layers of the bronchi, and the other forming a surrounding network. The nervous fibres which go to the epithelium of the bronchi form a network exactly *under* the epithelium, and some of them at the same time penetrate the epithelium itself. If we isolate the epithelium cells, we observe that the nerve fibres are in intimate connection with the protoplasm of these cells. In the cartilage of the bronchi, viz., in the perichondrium, we find the minute fibres of the nerve, which sometimes penetrate into the upper part of the cartilage, but are more generally confined to the perichondrium. The glands of the bronchi are surrounded with a network of nerve fibres, which sometimes enter into the epithelium of the glands. In the bronchi and in the tissue of the lungs we meet with the terminations or ends of the nerve, in the form of either round or pear-shaped cells, and sometimes we find nerves which terminate in rhombic bodies, the nature of which is not at present known. The nervous fibres and networks are surrounded with the periaxial space, which space is covered with the epithelium cells. For examination the lung is stained with a solution of chloride of gold, one-half to twelve per cent., and sometimes with nitrate of silver and osmic acid.

IV. Upon the Termination of Nerves in the Human Skin.

By Dr. Santchich.

In the fibrous layer which lies immediately under the epithelium exists a nervous network of an extremely close and fine construction, the rhomboid meshes being very minute. This network does not contain nerve cells, except in very few cases, but within the fibrous meshes are to be found epithelium cells, in numbers from one to seven. The nerves, after permeating the Malpighian layer, terminate under the horny layer. If the epithelium cell with its nervous fibre be examined under the microscope in an isolated form, the fibre will be found in intimate connection with the protoplasm or body of the cell. Besides the network of nerves interlaced between the cells of the epithelium, there are seen minute fibres which do not take any part in the formation of the network, but which are also connected with the epithelium cells.

Within the outer sac of the root of a human hair is found a network of nervous fibres, some of which possess a pulp while some have none. Some of these fibres permeate the inner sac, and terminate in a minute network around the capillary vessels, while others pass through to the

epithelium, and here end between the epithelium cells. The papilla of the hair is supplied by the same network of nerves as the other parts of the hair follicle.

I may at this point describe the nerves appertaining to the fat glands and sweat glands of the skin. Around both, the nerves form a network. With the fat glands some of the nerve fibres lie either immediately beneath the epithelium or between the epithelium cells, and we have occasionally been able to distinguish how the nerve fibre, after passing into the fat gland, terminates in a body similar to a mulberry, with two or four nuclei. The networks of nerves which are disposed between the epithelium cells anastomose with each other, so that we can observe how the network of the papilla is combined with the nerves of the hair follicle.

We have worked under the microscope with chloride of gold solution, $\frac{1}{2}-\frac{1}{8}$ per cent., and for the isolation of epithelium cells we have used a 10 per cent. solution of common salt.

ON FUNGOUS GROWTHS IN SOLUTIONS FOR HYPODERMIC MEDICATION, AND THEIR PREVENTION BY SALICYLIC ACID.

BY

JOS. G. RICHARDSON, M.D.,

ATTENDING PHYSICIAN TO THE PRESBYTERIAN HOSPITAL, PHILADELPHIA.

Among the minor inconveniences of medical practice, few are more annoying to the careful physician than a failure to afford the hoped or prayed-for relief from pain, by the subcutaneous injection of morphia. I therefore venture to occupy a few minutes of the time of the Section with a brief account of a new method for preventing diminution in the strength of morphia solutions, in so far as it is due to the common cause of such deterioration, namely, the formation in the fluid of fungous growths, which necessarily develop at the expense of the contained solid ingredients.

If we examine under a microscope of high power one of the white flocculent masses which appear after a week or two of warm weather in our ordinary Magendie's solution of morphia, we will commonly find it made up of an immense number of branching threads, on an average about $\frac{1}{5000}$ of an inch in diameter, often distinctly septate, and probably recognizable as the mycelial condition of one of the genera of the Mucorini or siphonaceous plants.

Complete identification of the exact species is only possible, even for the professed mycologist, by investigation of its aerial fructification. We, however, often find, as it develops entirely beneath the fluid, an evident attempt towards the formation of some sort of reproductive body, and in many cases are to be seen in active motion, near the filaments, spore-like bodies, analogous to the antherozoids of the unmistakable algæ and fungi, which have doubtless escaped from broken fragments of the aquatic mycelial threads.

The atoxic quality of salicylic acid, as demonstrated by the moderate and even large doses administered in thousands of cases of acute rheumatism, etc., suggests at once its employment for the purpose of preventing the growth of these fungi; and I think it quite probable that it has already been so used by others of my fellow-practitioners as well as by myself. No exact observations upon this subject have, however, I believe, as yet been published, at least in our own country.

In order to determine the precise amount of this antiphytic agent required to produce the desired effect, I prepared on the first of August last, now five weeks since, one dozen two drachm vials, each containing one fluidrachm of a solution of acetate of morphia (gr. xvj to f℥j), such as is usually employed for hypodermic medication. These samples were numbered from 1 to 12, and after the acetate, except impurities, had been dissolved by the aid of a small quantity of acetic acid, I added to Nos. 2 and 4 each one-sixteenth of a grain of salicylic acid, to Nos. 6 and 8 each one-eighth of a grain of the same substance, and to Nos. 10 and 12 each one-

quarter of a grain of the like material. Nos. 1, 3, 5, 7, 9, and 11 were left unmixed with other ingredients, and all were corked tightly and equally exposed to the light, and to the unusually elevated temperature of our Centennial summer. At the end of one week, small whitish flocculi, evidently composed of filamentous tufts, were visible in the bottles the contents of which were unmixed with salicylic acid, and similar minute specks, in much less quantity, had made their appearance in Nos. 2 and 4, which contained each one-sixteenth of a grain of salicylic acid.

Under the microscope, specimens of these tufts were seen to be made up of mycelial threads, probably of some species of physomycetous fungi, such as I have already described. At the present time, after five weeks have elapsed, those vials which were not protected by salicylic acid present every one large masses of fungous growth, as each member of the Section may see for himself by inspecting them upon the table. Nos. 2 and 4, in each of which it will be remembered that one-sixteenth of a grain of the acid was dissolved, display small tufts of mycelial threads. No. 8 shows a mere trace of fungous growth, whilst Nos. 6, 10, and 12 exhibit to the naked eye (as they do also under a one-fifth objective) no indication of living fungi in the slight deposit of brownish impurities let fall at the bottom of the glass.

In specimens of similar morphia solution, containing one-eighth of a grain of salicylic acid to the fluidrachm, carried in the hypodermic syringe case, and used from time to time in my daily practice during three months of cooler spring weather, no flocculi of living fungi were discoverable with the naked eye or under the microscope. Further, no unusual effects of any kind were observable in patients, some of whom received as much as ♏viij at a dose. Injections made into my own arms for the purpose of testing the fluids in Nos. 10 and 12, produced but little smarting sensation, and afforded the ordinary anodyne, with no uncommon nauseating, depressing, or disagreeable effects of the drug.

I therefore recommend the preparation of a fluid for hypodermic injections according to the following formula: Morphiæ acet. vel sulph. gr. xvj; acid. acetici (No. 8), gtt. ij; acid. salicylic. gr. iss; aq. destillat. f℥j. If a sediment is left undissolved, this liquid should be filtered, or, after standing a few days in a vial, or preferably a conical glass, the clear fluid may be drawn off by means of the hypodermic syringe itself, and preserved for use. By employing such a preparation, the practitioner is almost certain to avoid the disappointment in the relief of pain liable to arise from injecting a solution, the strength of which has deteriorated, and he will also escape that danger of producing abscess by the insertion of fungous elements beneath the integument, against which we have been cautioned by some authorities, although perhaps without sufficient cause for the warning.

In conclusion, I would suggest that a similar application of salicylic acid to the preservation of watery preparations of quinia, atropia, etc., to solutions of bromide of potassium, citrate of potassium, etc., and to vegetable infusions, when it is desirable to avoid the employment of even small quantities of alcohol, may prove of great value to the pharmaceutist as well as to the physician.

Discussion on Dr. Richardson's Paper.

After the reading of the preceding paper, the President, Dr. John C. Dalton, of New York, said:—Dr. Richardson has referred to two disadvantages that might result in the hypodermic use of solutions of morphia without this precaution, but in language which leads us to suspect that perhaps he does not think much of the danger of producing an abscess. I would like to inquire whether there is any definite information in regard to the danger, or whether in his opinion the danger exists at all?

Dr. Richardson said:—Theoretically I have felt inclined to suppose that danger would exist, but in order to test the theory I have tried several experiments, injecting my own arm with a solution containing fungi, and in no case has an abscess been produced.

Dr. Dalton said:—Of course it would be more agreeable to inject a fluid not containing fungous growths; at the same time ought we to anticipate that these growths would continue their development in a subcutaneous tissue? My attention was once very directly called to the difficulty that might arise from such development. I made a series of observations in order to determine how many definite kinds of microscopic fungi might be derived from ordinary atmosphere. Taking fresh slices of boiled potato, I placed them on a plate that had been immersed in boiling water, and covered them with a bell-glass, under which also a moist sponge was placed. A considerable variety of these microscopic growths showed themselves from time to time, of various colors; and notwithstanding their minute size and apparent want of specific character, they had differences in form and size and structure. A few weeks after finishing the experiment, I found one day on the back of my hand a spot which was irritable and itching; this spread, two or three other spots appearing, until finally I was compelled to give attention to the matter, and on examination I found that my hand was the seat of a tricophyton, which was finally removed after treatment for four or five weeks. It then occurred to me that perhaps I had inoculated myself while engaged in the experiments. I had been raising microscopic fungi, and I had unknown to myself been infected with them. Further, upon the same subject, Dr. Vandoren told me that in a series of experiments he had demonstrated that the ovum of the mosquito is not deposited directly in the water, but floats in the atmosphere, and is brought down by the rain. So it would seem that in reality there are a larger number of germs, both vegetable and animal, that may arrive at their destinations through the elements. My impression is that this fact in regard to the ovum of the mosquito is new.

Dr. Richardson said:—My recollection is that our natural histories gave us pictures of the mosquito in the act of laying eggs in the form of a boat which floated upon the surface of the water for some days or weeks before hatching. I think the experiments mentioned are entirely novel.

Dr. Dalton said:—In regard to the boat in which the eggs are contained, is it barely possible that Dr. Richardson may be thinking of the mosquito itself coming out from the integument of the larva?

Dr. Richardson said:—No, sir; that is a subsequent performance. But I wish to ask Prof. Dalton in respect to the infection of which he spoke, whether his health had been deteriorated in any way? I make this inquiry because one of the most difficult questions in regard to the development of these entophytic and epiphytic diseases is whether the health is first deteriorated, so that a suitable *nidus* for the fungi to grow in is afforded, or whether, on the contrary, the fungi are the primarily active agents which deteriorate the health.

Dr. Dalton said:—I can testify, with the greatest confidence, that I was in perfect health.

Dr. Richardson said:—Some years ago I had an interesting case bearing upon this point in my own family. On the chin of my little daughter appeared

a small pimple, which became inflamed, and seemed to have a yellow head upon it. In examining it I was struck with its remarkably dry appearance, and its sulphur-yellow rather than brownish hue. I placed a portion of it under the microscope, recognized the presence of the fungus of Favus, and soon cured the disease with corrosive sublimate solution. The child was in perfect health, and I could only account for the attack by supposing that she had leaned her chin on the sill of a car-window, where some person infected with achorion Schonleinii had accidentally left a few spores of the disease. I would like to ask another question, whether there has been any corroboration or disproof of the experiments described by Simon in regard to the dependence of ocular diphtheria upon micrococci developing in the cornea of rabbits; where two punctures were made, in the one side with a diptherized needle, and in the other side with a clean needle, in every case the result having been that the cornea infected was marked with brownish streaks, which under the microscope displayed minute fungus spores. I look upon it as exceedingly important to know whether the fungi can develop in an internal portion of the system, secluded from the air, and that in a previously healthy part of the organism.

Dr. Dalton said:—I am not familiar with any corroboration of those experiments. I tried some experiments of a similar kind, two or three years ago, with results which convinced me that it did not require for such parasitic growths the previous existence of a morbid condition of the recipient. The experiments were with regard to the ordinary decomposition or rotting of fruit. It can no longer be maintained that rotting is a continuation of the ripening process. An external wound is absolutely necessary to the rotting of any fruit with a hard integnment, such as the apple and the pear, though it is not absolutely necessary with other fruit, like the orange. I tried the experiments with apples, and proved that they could be kept for any length of time, if uninjured, and also that rotting was contagious. The rot could plainly be seen to spread away from the point of contamination. I have exposed a dozen apples, half sound and half broken, to the ordinary atmosphere, and the rot always began at the point of injury. The spores came in from the atmosphere. Indeed, an injured apple can be kept from rotting by being protected from the atmosphere. I have taken a sound apple, and made a little cut in it, and then put it under a bell-glass, and kept it an indefinite time. The experiments can be easily tried, and are most convincing.

Dr. Richardson said:—You speak about the rot starting from the wound in the integnments of the apple in a way that resembled the spread of erysipelas from the edges of a wound in the human body. Did it ever occur to you to cover the wound in the apple with carbolized putty before exposing it to the infection of the fungi which cause decay, and thus throw a ray of light upon one of the great surgical problems of the day?

Dr. Dalton said:—I never did so, but I think that it might be an interesting experiment.

Dr. Richardson said:—I would ask Prof. Rudnew whether these experiments of Oertel and Nassiloff (the latter, I believe, a Russian observer), narrated in Prof. Simon's report, have been confirmed or disproved by others of his countrymen?

Prof. Rudnew, of St. Petersburg, said:—I have studied these investigations, but have made no similar researches myself, and believe that the views of those gentlemen are still considered *sub judice*.

THE MECHANISM OF JOINTS.

BY

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The[1] limb being subservient to both support and motion, it is reasonable to expect that in certain joints the former function should be observed, and in others the latter. When the apparatus for support is conspicuous, the joint may be said to be of static value. But when this purpose is subordinate to flexion (*i. e.* deviation from the axial line of the limb), the joint may be said to be of dynamic value.

The most striking distinctions between the static and dynamic articulations lie in the relations of the opposed surfaces. To explain this portion of my remarks a few words of a general character are necessary. I premise that the typical "ball and socket" joints consist of well-defined balls embraced by perfect sockets. No such joints are found outside of the vertebral column. The articulations between the bodies of the vertebræ, although spoken of as amphiarthroses, are in truth "balls and sockets." The central intervertebral mass is the "ball;" the opposed vertebral surfaces and the peripheral interlacing fibrous bands of the disk make up the "socket." I also premise that the simplest forms, or at least the first forms, of joints appear in the vertebral column, since this structure answers to the axial line of the body. The movable union between any pair of segments or bodies of this axis may be taken as typical of what is possible under more complex conditions elsewhere. If it is remembered that a rod or axis cannot be projected far without segmenting, the best basis is secured upon which to rest any consideration of arthrosis. Obviously *motion* between a pair of bony segments is the main fact to consider in every problem. The questions which I believe can be asked with reference to further development of this theme are, in what way is *support* secured through a series of such mobile joints, and how can the apparatuses of *motion* be varied?

The limb is a special structure appended to the body. It moves chiefly at the joint between its first and second bones. This joint, in both the anterior and posterior extremities, is a ball and socket, as usually described. But it evidently in each instance is a portion of a ball opposed to a partial socket; since only small portions relatively of the head of the humerus or femur can be brought in relation to the socket at one time. So generally throughout the limbs (although in less marked degree), it is found that segments of spheroids play within concave surfaces. I propose to call all such surfaces modified balls and sockets (the type of which is exhibited only in the vertebral column), and proceed to define the two main varieties, named above, to wit: (1) Where the rela-

[1] [This paper is an abstract of a more elaborate study which the author reserves the right of publishing hereafter in a separate form.—EDITOR.]

tions between the two surfaces are such that the ball lies in its socket, and suggests *rest*. (2) Where the ball is suspended from the socket, and suggests *motion*. The occipito-atloid articulation is an example of the first, and the temporo-maxillary of the second variety.

The dynamic joints are apt to be crossed by powerful muscles, if indeed it may not be said of them that the muscles aid to a great degree in maintaining the efficacy of the articulations. Thus if the muscles about the shoulder-joint are divided, the humerus breaks contact with the scapula, and the joint is destroyed. This is also true of some other articulations, as the temporo-maxillary, in part. In some other instances of the same variety, the relation while not maintained by muscular action is strengthened by the presence of muscles crossing the joint, as at the ankle and wrist-joints.

As in all other studies in biological science, typical forms are united by intermediate ones. Some joints, as the knee and elbow, do not naturally fall within either static or dynamic kinds. The elbow-joint is especially difficult to study, it being varied from its type to a remarkable degree. The knee-joint, although exceedingly intricate, is resolvable in its several portions to static and dynamic functions, the structural modifications from typical points being pronounced. In the same way the first or swinging joints in the respective limbs, while of the same variety, since the ball is suspended from the socket in both, differ from each other in that the hip-joint is more fixed than the shoulder-joint. In a word the hip-joint is a dynamic joint, modified or specialized to do static work. In order that the work can be done effectively, stout bands cross its capsule, and the head of the femur is received within the acetabulum in such wise that the weight of the body is borne, not upon the centre of the head of the bone, but upon its upper surface.

If these general statements be remembered in studying the etiology of fracture and dislocation, more especially in examples of indirect violence, it will be recognized that a static joint will not be apt to be dislocated as long as the weight of the superincumbent mass presses directly within the socket. Thus a dislocation at the occipito-atloid articulation is rare, as is dislocation of the astragalus. But if the force be obliquely directed, causing the weight to be received violently against the side of the socket, then the joint may easily be luxated.

In the many positions assumed by the body, it often happens that an injury may be sustained from a reversal of the normal or typical relation. Thus while the extremities are described from the shoulder down, with the hands free (the main idea of the entire limb being *motion*), it becomes a different subject when studied with the limb suddenly assuming a *supporting* use—as for example when the body falls forward, and the hand strikes the ground while the limb is extended. Here the head of the humerus is driven up against its socket by the downward weight of the body, and the idea of swinging is lost. In a word, a dynamic joint is abruptly called upon to be a static one. A natural result is luxation of the shoulder, or, if the muscles about the joint be particularly rigid, fracture of the clavicle. In less degrees of violence, the force being expended nearer the hand, the wrist is either luxated or the radius broken. In medium degrees of violence, particularly seen in young subjects, the outer condyle of the humerus will break by the weight of the body being thrown through the convex radial head of the humerus downward upon the fixed concavity of the head of the radius, provided that the arm be forcibly extended. If the arm be partially flexed, the

weight of the body being transmitted to the forearm at an angle, the humerus will be apt to break transversely a little above the elbow-joint.

I do not speak here of the frequency of such fractures induced by indirect violence, but only of the way in which the articular surfaces as described would in my judgment behave under the circumstances detailed.

The lower limb, however, is always prepared for the conduction of lines of force so exceptional in the upper. The whole limb, while expressing motion, is also designed to sustain the weight of the body. Hence the greater strength of the joint surfaces, as well as the more prop-like appearance of the limb. In walking, the weight of the body is sustained for one moment at the astragalo-scaphoid joint in a nearly straight line, embracing, in the limb, the femur, tibia, and astragalus. The scaphoid bone and the inferior calcaneo-scaphoid ligament constitute a socket which receives and sustains this enormous weight. In falls from a height, which terminate by the foot striking, the weight not being received directly within the centre of the socket may be deflected, and luxation of the astragalus may occur. But from the rarity of this accident I infer that the mere exaggerations of the lines of force acting in its accustomed directions, are less injurious than the same kind of force acting upon the superior limb which is taken at a disadvantage.

The remainder of my remarks will be in application of the above data to the study of joints, including new observations of some special forms of articulation.

In the knee-joint, for example, I think that it can be shown, from the shapes of the articular surfaces, that the outer femoral condyle is the static or axial half, and the inner is the dynamic or swinging half. This study involves many details which would occupy too much time to elaborate.[1] Suffice it to say, however, that when a person stands erect, the femur rests upon the tibia as a ball in its socket, chiefly through its outer condyle. In flexion, this condyle is "switched off" (for the most part through the action of the popliteus muscle upon the outer semilunar cartilage), while the swinging of the inner condyle of the tibia upon the inner femoral condyle is initiated—not, it is true, by a ball suspended from its socket, as much as by a gliding of a shallow concave tibial surface upon a slightly convex femoral surface.

The term "switching off," as applied to the outer femoral condyle in passing from extension to flexion, needs explanation. The outer tibial condyle is observed to be convex toward the tibial spine. It is evident that, if any portion of the convex surface of the femoral condyle comes in contact with this convexity, no support is possible. This is what takes place when the knee is flexed. But when it is extended, the femoral condyle is placed firmly in a tibial concavity, the inner boundary of which is now formed by this eminence. As above stated, the outer semilunar cartilage is the factor producing the change. The outer tibial condyle is a true saddle surface, modified by the presence of the semilunar cartilage. The best example of a pure saddle surface is seen on the proximal surface of the trapezium. Here the convexity increases also toward the inner border. The first metacarpal bone of the thumb may be described as lodged upon this convexity in flexion of the thumb, and

[1] The details embrace the study of the cancelli of both femur and tibia, the shapes of the femoral, tibial, and patellar facets, the shapes and motions of the semilunar cartilages, as well as a refreshened account of the so-called crucial ligaments.

as relaxed or "switched off" in extension. It is evident that this lodgment and relaxation must be limited by appropriate ligaments. Inordinate or uncorrelated motions determine dislocation.

In the limbs of some lower animals, special adaptations in joints are recognized, which result in fixing or locking some of the facets. The immediate result of such locking is to conserve muscular power. Applications of this principle to some joints in the human frame may prove useful. Thus the knee-joint is nearly immobile at forced flexion or forced extension, but is freely movable at points between. It is probable that a careful study of other joint-surfaces will show similar peculiarities.

The facets upon the ends of bones are arranged in the order of the succession of the bones themselves in a given limb. The most evident arrangement is to have a facet upon the proximal and distal surfaces of each bone, in such wise as to allow the longitudinal axis to answer to the centre of each facet. These may be termed primary facets, since they are the most constant in any series of studies, and are least subject to change in special apparatuses. Assuming that the generalization of Goodsir is correct, that a process sent from the main shaft of a bone, as from the ribs of fishes and birds, may be termed a rayed process or actinopophysis, I will suggest that the same term may be given to the analogous process in a forked rib. If this be conceded, I think it not improbable that, in the event of the shaft of a bone and its actinopophysis becoming facet-bearing, such ray-borne articular surface should receive the name of actinic, or secondary surface. I believe that the inner femoral condyle is an instance of such a surface. I also believe that the surfaces upon the sides of certain bones, as those between tarsal and metatarsal bones, and between the tibia and fibula, or between the radius and ulna, are of subordinate value to the primary facets, since they appear to be caused by mutual compression of the bones themselves, and hence may be termed lateral or tertiary facets.

A joint may be said to be at rest when the least pressure exists between its opposed facets. The supine position will tend to rest static joints, since it diminishes the pressure of the weight. It will not, necessarily, so rest completely dynamic joints, since the muscles may be more or less active in the supine position. The lower jaw, for example, is not so rested. It is evident that to place in absolute rest a joint of this kind, its muscles must be kept from contracting. Any apparatus, therefore, to keep a joint at rest, must include all the muscles influencing the joint. When it is recalled that in the lower extremity the muscles are so disposed that the contraction of those arising from the hip effects changes in portions beyond the knee, and even down to the foot, it may be concluded that to make quiet any one, or part of one, of these surfaces, the entire limb should be kept quiet.[1]

It is at the same time true that, in a joint in which the articular surface on one side is larger than that on the other, those portions at any time not in use, may be said to be at rest. Thus, in the knee, some portions of the condyles are at rest in extension, while others are at rest in flexion. It follows from this that, in diseased action, products of pathogenesis will be absent from surfaces removed from pressure or friction.

[1] Rest is perhaps best secured at semi-flexion, *i. e.*, at the point at which extension ceases, and flexion has but just begun. All parts are then relaxed. This, while a theoretical point in some joints, has fixed value at the knee.

Now pressure long continued will destroy tissue, while friction will induce hypernutrition. It can be seen when a diseased joint is examined, with this fact in mind, at which points the pressure, and at which the friction has been. Thus in a knee-joint long the seat of chronic rheumatic arthritis, the outer condyle of the tibia will be found reduced in size, while the inner will be exaggerated. Also knee-joints flexed in diseased conditions will have points of pain localized at the inner side, while knee-joints extended in similar conditions will have like points developed upon the outer side.

If the outer condyle is axial, it will so remain, no matter what portion of the axis is reserved for articulation. So that in motion, after excision of the knee-joint, the line of axial support is preserved, and a good limb for *standing* is secured. But in consequence of the special apparatus for *flexion* having been removed, the limb below the knee is a mere wabbling appendage during attempts at bending.

Discussion on Dr. Allen's Paper.

After the reading of the preceding paper, the President, Dr. John C. Dalton, of New York, said:—The paper contains much that is new to me, while many things previously unexplained are now made clear. It has, however, occurred to me, as it undoubtedly has to the author, that he might have extended his principle also to the ligaments. I recall that while dissecting joints, as a student of medicine, after removing the external ligaments and retaining the capsule only, on bending the members making up the joint, it was impossible to say when certain parts were on the stretch and when not. It appears to me that this could also be explained by supposing the internal concave surface of the capsular ligament to be made up of a large number of facets operating in succession.

Dr. Allen said:—The occasion demanded that a single topic only should be discussed. Had time permitted, much could have been said upon the relation that articular surfaces bear to ligaments, both intrinsic and extrinsic. The fasciæ and muscles of every limb are also interested in every joint motion, and of the latter more particularly those that cross more than one joint. One series of these problems blends so easily into others that the line drawn between any two of them must necessarily be arbitrary. As Emerson has said, the answer to one riddle is another riddle. All our conclusions are relative, and invite renewed investigation. With reference to the ligaments of the knee-joint, it may be remarked that the great variety of function assigned to them by different writers is proof that their actions are not thoroughly understood.

Dr. Burt G. Wilder, of Ithaca, N. Y., having asked permission, as a visitor, to comment upon Dr. Allen's paper, said:—I understood Dr. Allen to say that the primary or fundamental movement of the limbs, among mammals, was forward and backward as with most quadrupeds, but that man had the power of raising the arms away from the trunk by means of the deltoid muscle, and that the head of the humerus presented a peculiar and characteristic facet, indicative of this movement of the limb. I would remind Dr. Allen of certain facts in embryology and comparative anatomy, well known to him of course, but apparently not present in his mind at the time of preparing his paper.

At a certain period of development, the forearm of man is in a state of pronation, as it is permanently in most quadrupeds, and afterwards acquires the power of complete supination, which seems to be correlated with the peculiar action of the entire limb alluded to by Dr. Allen. But, at a still earlier period, when the limbs have first appeared as little buds from the sides of the body, their narrow borders are forward and backward, while their flat surfaces

look outward and inward. Were any movement possible at this period, it is evident that to be effective it would have to be not forward and backward, but upward and downward, that is, *dorso-ventral*, and thus similar to that of which the human arm is capable in its greatest perfection in man's adult condition. In like manner, as far as we know, are developed the limbs of all Vertebrates; and the dorso-ventral or primitive motion is retained with most fishes and with the marine turtles. It is likewise persistent throughout life with the aquatic mammals, as the whales and porpoises, the manatee and dugong, the seal and beaver, and the curious duck-bill, or *Ornithorhynchus*, of Australia. It may be worth while to examine the shoulder-joint of these forms for the peculiar facet which distinguishes man from the ordinary quadrupeds.

At any rate, it seems somewhat significant that the dorso-ventral movement which, as Dr. Allen has stated, distinguishes the upper and nobler arm from the leg, and is likewise peculiar to man in his adult and perfect condition, in contrast with the monkeys and ordinary quadrupeds, should be also the primordial movement of both the arms and legs of all mammals, as indicated by the earliest condition of the limbs, and that it should likewise be retained in the case of certain low forms. In other words, the highest estate of man recalls his earliest condition, and suggests his relationship to the lowest members of his class.

Dr. Allen said:—While the act of walking expresses the swing of the anterior extremity, a yet better example of the characteristic motion of this part is swimming. This act, as performed by a man, is essentially different from the same act as essayed by a quadruped. In man, the arm is at one time forcibly abducted by a powerful deltoid. This muscle, in its distinctive shape, is not developed in the lower forms of animals, and in consequence the spinal play of facets between the scapula and humerus is, with them, absent.

SECTION ON SURGERY.

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OF PHILADELPHIA.

On taking the chair, Professor LISTER said:—

GENTLEMEN:—

Although I am well aware that the time of this Section is exceedingly precious, I cannot refrain from expressing my deep sense of the honor I have received, an honor as great as it was entirely unexpected, in being called upon to preside over the Surgical Department of this great International Congress, sitting as it does in the chief centre of medical instruction on this vast continent. American surgeons are renowned throughout the world for their inventive genius, and boldness and skill in execution. It is to America that we owe anæsthesia, the greatest boon ever conferred upon suffering humanity by human means; from America came the ligature of the common iliac artery for aneurism; the ligature of the internal iliac for the same disease; the "extension treatment" by the weight and pulley for fractures of the thigh, and other injuries and diseases; the reduction of dislocation of the hip-joint by manipulation; and that model of ingenuity, which I cannot mention without alluding to the name of its inventor, Sayre's splint for morbus coxarius. These are but samples of what surgery owes to this country, and it might, therefore, well have been that some American surgeon should have been called upon to preside over this Section of the Congress. Yet I assure you, gentlemen, that highly as I esteem this honor, it is the more gratifying to me, because I am persuaded that it has not been conferred on account of any special merit of my own, but in consequence of the interest felt by the profession in Antiseptic Surgery, with which my name happens to be connected. It was the circumstance of my observing in the programme of the business of the Congress that the subject of Antiseptic Surgery was to come on first for discussion, that led me to cross the Atlantic; and I should be pleased, indeed, if the discussion which is about to take place should have the effect of strengthening the belief of the profession in the truth, the value, and the practical application of the principles of Antiseptic Surgery.

ANTISEPTIC SURGERY.

BY

JOHN T. HODGEN, M.D.,

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Putrefaction is a decomposition experienced by animal substances when deprived of life and placed under special circumstances. The products of putrefaction are the results of the retrograde metamorphosis of nitrogenized substances. The process bears a striking resemblance to similar retrograde changes that occur in vegetable matters, resulting in the production of carbonic acid, alcohol, acetic and butyric acids, etc., while the products of the decomposition of animal substances are represented by carbonic acid, ammonia, butyric acid, leucine, sulphuretted hydrogen,[1] and many other matters whose chemical characters are not well established, and yet which cannot but be recognized as active agents in the production of septicæmia. Pasteur has shown that neither in vegetable nor in animal substances can putrefaction occur rapidly without the presence of living germs.[2]

Tyndall has, by the aid of electric light, and by almost innumerable experiments in reference to the development of living organisms, shown that the germs, without which putrefaction cannot be induced, and without which living organisms cannot be made to appear in vegetable and animal infusions, are found in unnumbered multitudes in the atmosphere.[3] The observations of Rindfleisch make it appear that these putrefactive and infusorial germs are abundant in water (terrestrial water), and less abundant,[4] and perhaps not even present, under ordinary circumstances, a few hundred feet above the earth's surface.[5] Tyndall has,

[1] The Antiseptic System, by A. E. Sansom, p. 31. Nitrogen, hydrogen, ammonia; sulphuretted, carburetted, and phosphuretted hydrogen; carbonic acid; carbonic oxide; acetic, butyric, and valerianic acids, and various other phosphorus and nitrogen bases

[2] Quarterly German Magazine, 1872; Mildew and Fermentation, by A. De Bary. Living yeast cells capable of growing and budding are absolutely necessary to the introduction of fermentation. . . . In dead substances in which we find bacteria, they are undoubtedly the vital promoters of decomposition.

[3] British Medical Journal; abstract of paper read before the Royal Society, January 18, 1876, by Prof. Tyndall.

[4] Virchow's Archiv, 1872, Bd. liv. S. 402. Examination of Lower Organisms, by Prof. Rindfleisch, of Bonn. Inaugural Thesis by Lothar Jasper, of Berlin, 1873. Treatment of Wounds by the Open Method and by Antiseptics. . . . . Atmospheric air carries the organisms which produce suppuration. . . . The air does not contain the organisms which produce decomposition, these are contained in terrestrial water and on all objects moistened by it.

[5] See The Antiseptic System, by A. E. Sansom, in which Schrœder and Dusch (in 1854 and 1859) testify that air passed through cotton-wool, which acted as a filter, was free from organisms. (p. 36.) "In 1860 M. Pasteur . . . filtering the air through a quantity of gun-cotton, dissolved the latter in ether, and thus obtained in a small space a number of bodies whose appearance announced them to be organized." (p. 35.) . . . He sowed the germs and produced organisms. "In some places the air was more fruitful than in others; especially its fertility diminished in proportion to the altitude from which it was obtained." (p. 36.) "In the air of sick rooms, hospitals, crowded halls, etc., many fungoid organisms have been noted by Trautman, Brittan, and Swayne, Dundas Thomson, Rainey, Lund, Lemaire, and

moreover, shown that, in the absence of currents of air, the germs do not reach infusions contained in vessels terminating in bent tubes, the ends of which are directed downwards, showing that germs must fall, or be carried by currents of air, into infusions, if they reach them at all, and establishing beyond a doubt that the specific gravity of the germs[1] is greater than that of the atmosphere.

Pasteur has determined that the ferments present different physical peculiarities,[2] so that the one may be distinguished from the other, and that they feed upon albuminoid substances.[3] Prof. Tyndall filled 100 tubes with infusions, and from the irregular manner in which the contents of the individual tubes were smitten, concludes that the germinal matter of bacterial life is not uniformly distributed in the air,[4] and thus accounts for the difference in the behavior of different wounds similarly circumstanced.[5]

The atmosphere of hospitals seems to be pervaded by living germs, and the walls and ceilings seem to be encrusted with them. Chalvet[6] found putrescible matters in the atmosphere and on the curtains, the windows, the walls and the ceilings of the Hospital Saint Louis. He also states that the vapor of water condensed near a suppurating wound is charged with irregular corpuscles resembling dried pus. "The experiments of Chauveau and Burdon Sanderson demonstrate that if a septic fluid capable of producing toxæmia when injected into the veins of a living animal be strained through a porcelain filter, the liquor so filtered may be injected with impunity, whereas the solid residue remaining on the filter retains in full force all the septic properties of the original fluid matter."[7] Mr. Lund found bacteria in the air of the Manchester Royal Infirmary.[8]

Septicæmia, in the surgical sense, expresses that condition of the blood which is induced by the presence of the products of putrefaction, a condition of the circulating fluid that unfits it for the proper performance of its function. Putrid matters may enter the circulation through any of the mucous, serous, or cutaneous surfaces, or from any part of the body in which they may be present.[9]

In surgery, the meaning is limited to the presence of putrid matters

Ransome. More recently Dr. Maddox has carefully examined the particles derived from the atmosphere," . . . and "found pollen grains, minute germs of fungi or protophytes, and excessively minute bodies, 'molecules,' 'globules,' etc." (p. 59.) "Dr. Percy found the dust of the British Museum to contain 50 per cent. of inorganic [Qu. organic?] matter. The latest investigations of this subject have been made by Mr. Tichborne, F.C.S. The following were the results of analyses: Street dust in Dublin contained organic matter 31 per cent. Dust from places of Public Concourse contained organic matter 32 to 53 per cent. Dust from Nelson's Pillar, 134 feet in height, contained organic matter 29 per cent." (p. 60.)

1 See The Antiseptic System, by A. E. Sansom, p. 58. Pasteur and Lister find that putrefactive germs are arrested in bent tubes.

2 See Sansom, p. 36. "By further researches Pasteur concluded that the different kinds of organisms met with in putrefying solutions differed widely in the conditions of their nutrition and their vitality. Some absorb oxygen and excrete carbonic acid; others the converse."

3 Pasteur, Annales de Chimie et de Physique. Burdon Sanderson, Transactions of the Pathological Society of London, vol. xxiii., 1872. Cohn, Quarterly Journal of Microscopical Science, 1873, p. 156.

4 Sansom, p. 36; Pasteur's experiments.

5 Prof. Tyndall's recent researches; Abstract of paper read before the Royal Society, January 18, 1876. From the British Medical Journal.

6 Chalvet, Mémoires de l'Académie Impériale de Médecine; Paris, 1863, tome xxvi.

7 See A Clinical Lecture on Septicæmia, delivered January 26, 1876, by S. Messenger Bradley, F.R.C.S., Surgeon to the Manchester Royal Infirmary.

8 Ibid.

9 Pathology and Morbid Anatomy, by T. Henry Green, London, 1876.

derived from the decomposition of the solids and liquids of the body itself; and these putrid matters may be formed either in parts in direct contact with the air, or in those with which the air does not come directly in contact.[1] Common observation indicates that absorption of these noxious matters takes place more rapidly in recent wounds, and in the early stages of inflammations, while the experiments of Billroth attest that after healthy granulation is fairly established, absorption of putrid matters goes on so slowly (if at all) that the phenomena of septicæmia are not presented. He says,[2] "Certain circumstances are necessary for putrid matters to be taken into the blood of man. Such substances are taken through the healthy skin and mucous membranes when the putrid substances have a destructive or cauterant action, or an active power of penetrating, like fungi and infusoria." "Diseased skin or wound-surfaces take up such putrid matters more readily, but even they only do so under certain circumstances; for instance, germs do not readily pass through well organized, uninjured granulations." "If we dress a nicely granulating wound on a dog with charpie dipped in the filthiest putrid matter, if the latter contains no cauterant substance that may destroy the granulating surface, the animal will not sicken; nothing will be absorbed. . . . If the septic poison be introduced into fresh tissue, it not only excites severe local inflammation, but quickly induces general fever."

Putrid pus is found in abscesses in many parts of the body; near the alimentary canal or respiratory passages, and in the soft tissues of the extremities, as well as in and about the bones, and, indeed, in every part of the body. A destructive inflammation may be set up about these collections, and the protecting walls may melt away and furnish more matter to undergo putrefaction, while the conditions left by their destruction will favor the absorption of septic matter. And it is probable that the septicæmia that so frequently follows the discharge of large collections of putrid pus is due, not so much to the admission of air and fresh agents active in the production of putrefaction, as to the rekindling of a destructive inflammation in the walls of such abscesses. This renewing of inflammatory changes may be due to the increased flow of blood to their walls, permitted by suddenly removing the pressure that may have, for an indefinite period, acted as a barrier to its entrance. The renewal of inflammatory processes from any cause in tissues surrounding collections of putrid matters is apt to be followed by septicæmia.

Certain conditions of the solids and liquids of the body are essential to the ready action of septic substances. In the physiological processes of the body, disintegrations and retrograde metamorphoses are constantly occurring as a consequence of vital actions, and the elements of the tissues assume a condition of fitness for putrefaction; all of the excretions are examples of these products. Febrile states, inanition, over-exercise, all induce changes in the organism that favor putrefaction in the solids and liquids of the body.

The impregnation of the tissues with certain substances tends to prevent putrefaction. Prof. Polli, of Milan, found that the bodies of animals that had been fed on the sulphites resisted putrefaction longer than those of similar animals not so fed, and that urine passed after taking sulphites remained "fresh, acid, and clear, and did not undergo ammoniacal fermentation for eight days during the hottest Italian summer." According

[1] Joseph Bell, M.D., Edinburgh Medical Journal, 1869, p. 982, *et seq.*
[2] Billroth, Surgical Pathology, American Edition, 1871, p. 334.

to Polli, these salts do not destroy the ferments, do not directly kill living germs, but modify the tissues so that they are incapable of being acted upon by catalytic germs, and thus putrefaction does not occur.[1] Prof. Gamgee's process of preserving meat consists in impregnating it with carbonic oxide and sulphurous acid.[2]

Any condition or any substance that opposes putrefaction is antiseptic, and, in its widest sense, this term includes many methods of action. In the surgical sense any agent which prevents those changes in the blood which ordinarily result from the rapid absorption of large quantities of putrid matters, must be regarded as antiseptic. Filters, used to prevent the contact of living germs with the putrescible discharges from wounds and the tissues, furnish a most admirable method of preventing septicæmia. A. Guérin, in 1870, used, at St. Martin's Hospital, cotton-wool as a dressing after amputations.[3] The great practical difficulty in the successful use of cotton lies in the fact that we cannot be sure that the cotton-wool itself does not contain germs which may become active in the production of putrefaction.[4] Attempts have been made to correct this possible condition by exposing the cotton-wool to air of an elevated temperature, and also by impregnating it with various substances, with the hope that germs already existing in the cotton may be destroyed. Prof. Lister, in 1871, prepared cotton-wool by diffusing through it chlorine gas, sulphurous acid gas, carbolic acid vapor, and vapor of benzine.[5] This end is attained by Prof. Lister in the use of his antiseptic gauze, with the advantage that it is a material which may be much more neatly and easily applied, and more readily rendered antiseptic, being in thin sheets, the fibres of which are all easily reached by the germ destroyer. Charcoal, animal and vegetable, is one of the best of protective dressings, acting (if a sufficiently thick layer be used) as a filter, and also as an absorbent by which the discharges are held until they may be removed; and, if putrefaction has actually taken place, the fetid emanations are held, and do not contaminate the surrounding air. Chalk, plaster of Paris, and clay, serve the same purpose as charcoal, but less efficiently. Powdered madder root[6] and Peruvian bark not only act as filters and condensers of fetid gases, but actually prevent putrefaction by destroying the activity of living germs, and, perhaps, by altering the escaping liquids so that they do not readily putrefy. Among the most valuable antiseptics are the metallic salts, as those of iron, copper, lead, zinc, mercury, and silver.[7] All of these used in a sufficiently concentrated state act as caustics, and less concentrated they simply condense the tissues, and alter the solids and liquids so that they do not readily putrefy; in addition to this action they destroy the vitality of the living organisms upon which rapid putrefaction depends, and also destroy noxious products.[8]

As a first application to a recent wound, perhaps, no substance of this

[1] Observations on the treatment of zymotic diseases by the administration of sulphites. Read in the medical section before the annual meeting of the British Medical Association, August, 1867, by Prof. John Polli, M.D., of Milan.

[2] Scientific American.

[3] Extracts from Thesis of R. Hervey; Paris, 1873.

[4] Prof. Tyndall, paper read before the Royal Society, January, 1876.

[5] British and Foreign Medico-Chirurgical Review, October, 1875.

[6] London Medical Record, May 15, 1876; Antiseptic Properties of Madder Root, by M. de Restaing.

[7] Experimental investigations of the Action of Medicines, by T. Lauder Brunton, 1875.

[8] Therapeutics, Materia Medica, and Toxicology, by H. C. Wood, M.D.; Philadelphia, 1874.

class is better than chloride of zinc, which coagulates at once the superficial layer of the albuminoid substances with which it comes in contact, and, coagulating the blood and lymph while yet within the mouths of the minute vessels, seems not only to prevent putrefaction, but also to render rapid absorption improbable.

Since the prevention of absorption is as efficient as the prevention of putrefaction[1] in avoiding septicæmia, this and a similar class of substances are among the very best antiseptics. Chlorine,[2] bromine,[3] iodine,[3] and the alkaline sulphites are active destroyers of germs and preservers of flesh, while the permanganates and manganates are chiefly valuable as scavengers,[2] as they hasten oxidation, and thus rapidly alter organic compounds to a degree that renders them incapable of putrefaction. Quinine,[3] salicylic acid,[4] salicine, benzoic acid, and boracic acid,[5] all destroy the vitality of bacteria,[6] and prevent and check putrefaction. Carbolic acid, creasote, and thymic acid, not only devitalize the germs, but alter the tissues so that they do not so readily undergo putrefactive changes, and alter recently wounded tissues so that absorption does not readily occur, but they do not destroy the products of decomposition.[7]

Drainage tubes[8] cannot be neglected in enumerating the valuable methods of getting rid of putrescible matters;[9] so, also, the water-bath, in which the water is continually changed, dilutes and carries away the putrid matters, and so prevents their absorption;[10] so, also, the contact of currents of dry air rapidly desiccating the putrid discharges, prevents both putrefaction and absorption. Alcohol and alcoholic tinctures[11] have attained considerable eminence as antiseptics, and no doubt owe their efficiency to the power possessed by alcohol of coagulating the albuminoid products of wounded surfaces, as well as of the tissues with which it comes in contact,[12] and also to the fact that in the manner in which they are employed, they seem by repeated changes to remove the putrescible matters before putrefaction can occur; they are, no doubt, farther beneficial in preventing absorption, since the change they effect in the tissues would render these less liable to the entrance of matters from without.

[1] The Action and Use of Antiseptics in Surgical Practice, by Charles Roberts, F.R.C.S., Assist Surg. for the Victoria Hospital for Children; Lancet, vol. i., 1872.

[2] H. C. Wood, op. cit. [3] Brunton, op. cit.

[4] New York Medical Journal, January, 1876; Gangrene, by Dr. N. G. McMasters. Polytechnisches Journal, July, 1876; Antiseptic Properties of Salicylic Acid, by Prof. Kolbe, of Leipsic.

[5] Lancet, May 20, 1876; Boracic Acid as an ordinary Dressing for Wounds, by Dr. Leonard Cane.

[6] Brunton, op. cit.

[7] Sansom, op. cit.; Mould and Mildew are rapidly destroyed by carbolic acid. . . . It prevents the germination of the spores of the yeast plant. . . . Albumen precipitated by carbolic acid does not putrefy. (p. 24.) An infinitesimal quantity of carbolic acid instantly kills bacteria, vibriones, etc. (p. 25.) When carbolic acid is added to a putrefying solution, it is observed that all infusoria present in that solution instantly die, and the cessation of putrefactive change is coincident with their death. And when carbolic acid is added to a solution susceptible of putrefaction, but before the commencement of any putrefactive decomposition, no organisms are developed. (p. 29.)

[8] British and Foreign Medico-Chirurgical Review, vol. liv., 1874, p. 1; Review by Mr. Callender.

[9] Half-Yearly Abstract; Isolation and Treatment of Wounds, by G. W. Callender, F.R.S.

[10] Memoir read before the French Academy of Medicine, 1870, by Dr. Leon de Fort.

[11] Pansement des plaies Chirurgicales; par le Dr. Benjamin Anger, Paris, 1872.

[12] Alcohol and Alcoholic Preparations in Surgery, etc.; by F. J. Batailhé and Ad. Guillet, Paris, 1860. See Amputations of the Thigh, by John Green, M.D., Boston Medical and Surgical Journal, June, 1863.

The value of the antiseptic treatment of surgical diseases is attested by many of the most eminent surgeons of Europe.[1] The practice is infinite in variety, extending from the simple protection of wounds from contact of catalytic germs, to the purification of hospital wards, water-closets, and grounds. So general is the practice of antiseptic surgery, and so thorough the faith of the profession in the value of its observance, that we find none so bold as to deny its value, or so reckless of the lives of his patients as to dare neglect its teachings.

One practises isolation as the surest method of preventing septicæmia, another has unlimited faith in ventilation and personal cleanliness;[2] one moves with deadly intent on the hosts of bacteria that infest his wards, another keeps watch over his wounds with a fidelity that challenges admiration; one places a barrier to the contact of the dreadful cause of putrefaction, another seals the absorbent surfaces and denies admission to the products of putrefaction; one leaves the wound open for the escape of the deadly enemy, another bars the paths of entrance so that none but the pure shall enter; one washes and scrubs, another plasters and daubs—all seek the same goal but by a variety of paths.

When we consider the fierce contest that for years has been waged in reference to the theory of the spontaneous generation of infusoria, and learn how difficult it is to prevent the entrance of germs into infusions; when we remember how difficult it is to prepare an infusion, and protect it, so that living matter shall not enter it from without, we cannot but realize the utter impossibility of protecting wounds from the contact of catalytic germs. Accepting as true the doctrine now so thoroughly established by Pasteur and others that fermentation and putrefaction do not occur except in the presence of living germs, and recognizing the presence of these germs in the blood of living human beings,[3] and their action as attested by the presence of collections of putrid matter in every part of the human body, we can readily see the utter hopelessness of a struggle to prevent by filters, plasters, or powders, their contact with open wounds.[4] Dr. E. Fischer found bacteria septica under Lister's dressings in every case, notwithstanding that all the cases, except one, had a favorable course.[5] Ranke and Von Birch-Hirschfeld found the same.

It should not be forgotten that through the various channels of excretion, septic matter may be eliminated from the body, and that if absorption is not more rapid than elimination, no poisoning occurs. If, therefore, we can keep the absorption of septic matters within bounds, we virtually prevent septicæmia. The practice of washing out the cavity of the uterus after the removal of fibroids affords a good illustration of the value of preventing a too abundant absorption of putrid matter,[6] as

[1] Volkmann, Thiersch, Sigmund, Holmes, Syme, Lister, Nankivell, Kolbe, Bennett, Rivington, Cane, Polli, Green, Hervey, Cadge, Bradley, Guérin, Jasper, Esmarch, Bell, Dougall, Macnamara, Leisrink, Koenig, Holderness, Manson, Ross, Sansom, Hamilton, McCormac, Bickersteth, Hulke, Baker, Bartlett, Goodall, Sydney Jones, Saxtorph, Lund, Walton, Wells, Bastleto, Jessup, etc.

[2] British Medical Journal, April 25, 1876; Prevention of Pyæmia in Hospital Practice, by Wm. Cadge, Surgeon to the Norfolk and Norwich Hospital.

[3] Clinical Results of Lister's Antiseptic Treatment, and the Substitution of Salicylic for Carbolic Acid; Prof. C. Thiersch, Leipsic.

[4] Medical Times and Gazette, vol. ii., 1875; The Antiseptic System in Surgery, by George Thomson, M.D., Surgeon to Oldham Infirmary.

[5] Deutsche Zeitschrift für Chirurgie, Feb. 1876, Bd. vi., No. 4, S. 319; Lister's Dressing and the Organisms found beneath it, by Dr. E. Fischer, Strasbourg.

[6] On Intra-Uterine Fibroids; by J. Marion Sims, Surgeon to the Woman's Hospital of the State of New York, 1874. Remarks upon the Enucleation of Uterine Fibroids, with illustrative cases; by T. Gaillard Thomas, M.D.

does also the washing out of the uterus after giving vent to accumulations of old menstrual fluid (Emmett).

Some of the eminent German surgeons affirm that, during the Franco-Prussian war, a large percentage of the fatal results in gunshot wounds of the peritoneum was due to septicæmia, and that good results were obtained by repeatedly washing out the cavity.

Hervieux[1] urges the value of washing out the uterus in cases of puerperal metritis, and Fordyce Barker recommends the injection of carbolic acid solution four, five, or six times a day in the same disease. The washing out of the cavity of the chest after evacuation of pus, in empyema, the cleansing of putrid matters from abscesses and sinuses, all afford examples of the benefit of preventing the too rapid absorption of septic matter.

Good results are said to be obtained also by favoring the elimination of septic matters through the various emunctories of the body. Sulphite of magnesia has been used not only with the intent of protecting the tissues and destroying the septic germs, but also as an aid to the elimination of septic matters through the kidneys and intestines.

In connection with antiseptic surgery, the antiseptic ligature[2] cannot be ignored; most favorable results have been reported after its use. The advantages claimed are: (1) That it does not cause suppuration; (2) That it becomes absorbed;[3] and (3) That the ligature becomes organized,

[1] The Puerperal Diseases, by Fordyce Barker, 1874, p. 322.

[2] On the Employment of Carbolized Catgut as a Ligature in Amputations and other Major Operations; by A. W. Nankivell, Resident Surgeon to Saint Bartholomew's Hospital, Chatham; Lancet, Feb. 19, 1876.

[3] I. On Ligatures made with Catgut; by D. Murinoff. Inaugural Discourse, St. Petersburg, 1875; from Centralbl. f. Chirurgie, 1875, No. 43.

II. On the different kinds of Ligatures employed in the treatment of deep-seated Cuts; by Eliaschewitsch, Russian Military Medical Journal, June, 1875; Centralb. f. Chir., 1875, No. 43.

I. The experiments, the conclusions of which we report here, have been made upon dogs and rabbits. Simple threads of catgut, soaked in chloral, and "catgut phenique" were employed in making the ligatures. All the ligatures on two or three dogs were soaked with juices; on the seventh day they were swollen, soft, and covered with a thin transparent layer of connective tissue; the knots were adherent to the surrounding tissue. The finest threads were re-sorbed in ten days, the catgut No. 4, of Lister, in from twenty to thirty days. This disappearance is effected by the division of the thread into fibrils which gradually disappear upon the contact of granulations. It requires the same time to be accomplished, whatever thread may be employed, provided that it is of the same dimensions. The simple thread of gut does not irritate in the least the neighboring parts; the thread soaked in chloral irritates them a little; the catgut a little more. As to consecutive hemorrhages after section of a large arterial vessel between two ligatures, Murinoff gives the following statistics: catgut, 10 per cent.; thread of gut soaked in chloral, 25 per cent.; simple thread of gut, 87 per cent.

II. Eliaschewitsch has sought to establish by experiments upon animals the different influence exercised by the threads of the suture according to their nature. Relating to inflammation, as to the influence which they exert upon the point of suture itself, he arranges them in the following decreasing order: Cotton, linen, silk (waxed silk being less irritating than raw silk), metal (whatever it may be), Florence thread, crin végétal, catgut phénique. The thread in a cord of non-carbolized gut irritates less than that covered with phenic acid, but it is less resistant. The presence of Florence thread or catgut in the depth of a wound does not hinder its reunion by first intention. The greater or less rapidity with which sutures cut, depends upon the irritation produced by the thread itself, and its degree of tension, and finally upon the condition of the lips of the wound. In the second series of experiments, Eliaschewitsch has endeavored to ascertain what becomes of catgut threads left in the wound. He has discovered that as soon as they come in contact with granulations they split up over their whole surface, and fibrils are detached which in their turn are divided into bits finally constituting only a granular detritus. The process began upon the fifth day in threads which had been used for ligatures of large arteries on a dog; at the end of a month no trace of a ligature was found.

and becomes a ring of living tissue surrounding and occluding the vessel.[1]

[1] Lancet, April 3, 1869. On Ligature of Arteries on the Antiseptic System; by Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow. . . . . In order to put the antiseptic animal ligature fairly to the test, I made the following experiment: Ligature of the carotid artery in the calf on the antiseptic system with threads composed of animal tissues.—On the 31st of Dec. 1868, I tied the right carotid about the middle of the neck in a healthy calf a few days old; the animal being under chloroform. Ligatures of two different kinds were employed at an interval of about an inch and a half, the sheath of the vessel being left undisturbed in the intervening part. The cardiac ligature was of home manufacture, composed of three strips of peritoneum from the small intestine of an ox, firmly twisted together into a threefold cord. The distal thread was of fine catgut, called "minikin-gut" by the London makers. Both had been soaked for four hours in a saturated watery solution of carbolic acid, which swelled and softened them so that the thread of my own making was too large to enter the eye of the aneurism needle, except near the ends, where it was thinner than elsewhere. This substantial ligature bore the strain of tying well, but the fine catgut broke as I tightened the noose; I did not, however, remove it, but, having a second piece at my disposal, passed it round at the same place, and with gentle traction completed the knot. There were thus two ligatures of the gut at the distal side. All were cut short except one end of the catgut, which I purposely left about three-quarters of an inch long to give a better opportunity of ascertaining what would become of the foreign material. . . . . A month (thirty days) after the operation, the animal, which had continued in perfect health, was killed, and the soft parts of the neck, below to the spine, were removed for examination. On dissection, I was struck with the entire absence of inflammatory thickening in the vicinity of the vessel, the cellular tissue being of perfectly normal softness and laxity. On exposing the artery itself, however, I was at first much disappointed to see the ligatures still there, to all appearance as large as ever. But had I borne in mind what I had observed in some of my earlier cases of compound fracture treated antiseptically, I should have been prepared to find these threads present in appearance, though absent in reality. It may be well for me to quote from the account I have before given of one of these cases. It was a compound fracture of the leg produced by direct violence, with a wound of considerable size, and a great deal of extravasation of blood into the limb. In accordance with the practice which I then followed, a piece of lint soaked with undiluted carbolic acid had been placed over the wound, and had formed with the blood a firm crust. Nearly three weeks after the accident, I was detaching a portion of the adherent crust from the surface of the vascular structure into which the extravasated blood beneath had been converted by the process of organization, when I exposed a little spherical cavity, about as big as a pea, containing brown serum, forming a sort of pocket in the living tissues which, when scraped with the edge of a knife, bled even at the very margin of the cavity. This appearance showed that the deeper portions of the crust itself had been converted into living tissue. For cavities formed during the process of aggregation, like those with clear liquid contents in a Gruyère cheese, occur in the grumous mass which results from the action of carbolic acid upon blood; and that which I had exposed had evidently been one of these, though its walls were now alive and vascular. Thus the dead but nutritious mass had served as a mould for the formation of new tissue, the growing elements of which had replaced the materials absorbed, so as to constitute a living solid of the same form. Hence it might have been anticipated that the ligatures of peritoneum and catgut placed on the calf's carotid would, after the expiration of a month, be found transformed into bands of living tissue. Such was, in truth, the case, as was apparent on closer examination. They had, indeed, a deceptive resemblance to their former condition from the persistence in their substance of the impurities of the original materials, the dark adventitious particles being of mineral nature incapable of absorption, so that they had remained as a sort of tatooing of the new structure. Nevertheless a marked alteration in color had taken place, especially in the distal ligature, where the dirty gray of the softened catgut had changed to a dirty pink tint.

The two pieces of catgut which had been tied round the vessel at that part had become, as it were, fused together into a single fleshy band, inseparably blended with the external coat of the artery. The knots were nowhere discoverable, and the only indication of the end which had been left long at the time of the operation was a black speck here and there upon a delicate thread of cellular tissue in connection with the vessel. The cardiac ligature was in like manner continuous in structure with the arterial wall. The short ends had disappeared; but the massive knot was represented by a soft, smooth lump, which appeared first entirely homogeneous, except that it was speckled with dark particles, as before referred to. On section, however, I discovered in the interior of the mass, and lying close

Prof. Lister says:—"Ample as was the evidence afforded to the naked eye of the organization of these ligatures, it was satisfactory to find it

to the wall of the artery, a small residual portion of the original knot, of comparatively firm consistence, and with the threefold twisted character of the cord plainly visible. It was quite distinct from the living tissues that surrounded it, so that it could be readily picked out from its bed with a pair of needles. A slender and irregular remnant of the noose was also found lying in a sort of tubular cavity, extending about half way round the vessel. Thus the process of organization had not yet quite invaded the entire thickness of the foreign solid, and it was a happy circumstance that the cord had been so constructed that the distinction between the old structure and the new could be plainly recognized. Ample as was the evidence afforded to the naked eye of the organization of these ligatures, it was satisfactory to find it confirmed in the clearest manner by the microscope. A bit of the knot having been teased out with the needles in a drop of water, presented, like a fresh piece of peritoneum, the wavy bundles of parallel fibres, characteristic of perfectly developed fibrous tissue. Adhering to the surface of the remnant of the ligature was some soft, opaque material, readily washed off with water, consisting of corpuscles of different forms, most of them caudate or fibroplastic, but some spherical, though not resembling pus; and here and there fragments of the original peritoneal tissue, affected more or less with interstitial cell development. At a short distance from the remains of the old thread, the fleshy material which had been formed at its expense proved to be a most beautiful example of fibroplastic structure, the coarse fibres which mainly constituted it being composed of very large elongated cells, often containing several nuclei, and presenting in their course branchings and thickenings of various forms. Here and there were some fibres more perfectly formed, and also cells of a more rudimentary character. Again, the band which had resulted from the organization of the two fine threads of catgut, which from the smallness of their bulk had no doubt vanished early, having had longer time to perfect its structure, was a comparatively well-developed form of fibrous tissue, consisting of coarse fibres rather than elongated cells, being thus intermediate between the merely fibroplastic condition of more recent growth and the completed texture of the original thread. For it is to be remarked that a piece of catgut exhibits, under the microscope, abundance of perfect fibrous tissue.

A more favorable period for the investigation, with a view to establishing the nature of the change which ligatures of animal tissue experience under antiseptic management, could hardly have been selected. Between the parts tied, the calibre of the artery was occupied by adherent coagulum, which was for the most part decolorized, and exhibited, under the microscope, fibroplastic cells of irregular forms. A similar clot was present between the distal ligature and a small branch that arose about a quarter of an inch beyond it. But between the proximal ligature and the heart the formation of a coagulum had been entirely prevented by a large vessel taking origin immediately above the part tied, which had thus borne for a month the full brunt of the cardiac impulses. Yet the vessel, so far from showing any sign of giving way, as it would inevitably have done had it been tied in such a situation without antiseptic precautions, appeared to have derived additional strength from the operation. The encircling ring of new tissue incorporated with the arterial wall must have had a corroborative effect, and within its grasp the inner coats which seemed to have been but imperfectly ruptured by the soft and substantial ligature were considerably thickened and had coalesced, so as to form a strong cul-de-sac, the irregularities of which had been smoothed over by a little fibrinous deposit, which had assumed the characters of a firm fibrous tissue, and presented a free surface, undistinguishable from that of the lining membrane of the artery. At the situation of the distal ligature, the structure of the vessel seemed entirely unaffected. The middle coat was seen in longitudinal section as a pink streak between two white lines, representing the external and internal tunics, neither thicker nor thinner than in the neighboring parts. The catgut threads had been tied too gently to produce rupture of the internal and middle layers, and their presence, and the constriction which they occasioned, whatever may have been their effect in the first instance, had left no permanent marks of the disturbance; while the fleshy band which had replaced them, though in time it would doubtless have dwindled to an insignificant filament, was at least a temporary addition to the strength of the artery. These appearances at the distal ligature are calculated to revive under a new aspect the old question whether it would not be better always to avoid rupture of the internal and middle coats, which could easily be done by using a pretty thick piece of catgut, softened by steeping it in a watery solution of carbolic acid. In this way the wall of the vessel would be left from first to last entirely intact. This, however, is probably a matter of indifference. Indeed, judging from the condition of the artery at the cardiac ligature, the injury done to the vessel at the outset by tight tying seems to lead to changes which increase its power of resistance, which was certainly severely tested in the present instance. It appears, then, that by applying a

confirmed in the clearest manner by the microscope;" and he adds, "It appears, then, that by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue and strengthen the vessel where we obstruct it."[1]

Mr. Fleming, who made a series of experiments to determine the changes accomplished in antiseptic ligatures, says:—"The results[2] of these experiments show that a gradual softening takes place from without in, the catgut breaking down and becoming infiltrated with cells, probably leucocytes; . . . next, the pultaceous mass into which it has been converted begins to metamorphose, and is soon permeated with blood-channels and ultimately may be described as a cast of the catgut in a kind of granulation tissue freely supplied with bloodvessels which, in many of my sections, were freely injected." It is a well-established fact that epithelial cells, far removed from the circulation and actually dead, placed in contact with a granulating surface, live again and multiply; and there seems no reason why the cells of the fibrous tissue of the sheep should not also. Ivory pegs driven into bone become absorbed; metals, glass, wood, and silk ligatures may remain in the tissues for an unlimited time without exciting irritation, inflammation, or suppuration.

The question as to the special value of carbolized catgut remains open since the facts above stated of wood, metal, silk, glass, etc., are well established. Prof. Paul F. Eve, of Nashville, Tenn., informs me that for

ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue, and strengthen the vessel where we obstruct it. The surgeon, therefore, may now tie an arterial trunk in its continuity close to a large branch, secure alike against secondary hemorrhage and deep-seated suppuration, provided always that he has so studied the principles of the antiseptic system, and so carefully considered the details of the mode of dressing best adapted to the particular case in hand, that he can feel certain of avoiding putrefaction in the wound. For my own part, I should now, without hesitation, undertake ligature of the innominate, believing that it would prove a safe procedure.

[1] Prof. Lister, in the Discussion which followed the reading of this paper, objected to the position in which he had been placed in reference to the organization of the ligature antiseptically applied, and the following explanation was subsequently made by the author: "It is due to Prof. Lister, due to the Surgical Section, and due to myself that I should correct a misstatement of the position of Prof. Lister in regard to ligatures antiseptically applied. In the Report on Antiseptic Surgery, in the part relating to the use of the antiseptic ligature, the following passage occurs: Prof. Lister says, 'Ample as was the evidence afforded to the naked eye of the organization of these ligatures, it was satisfactory to find it confirmed in the clearest manner by the microscope,' . . . and again, 'It appears, then, that by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue and strengthen the vessel where we obstruct it.' These are the precise words of the report, and the precise words of an article On Ligature of Arteries on the Antiseptic System, by Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow, as published in the London Lancet for April 3, 1869, page 451, and copied into Braithwaite's Retrospect, vol. lviii., 1869, pp. 139, 140. So far, it would appear that in the Report on Antiseptic Surgery I was entirely correct in my quotation of Prof. Lister's language, as it appeared in print; but Prof. Lister having stated in the Surgical Section that he had not advocated the theory of the revitalization of the antiseptic ligature, I must accept his statement as correct, and modify my report accordingly. The quotation embraces but two sentences of an elaborate article, a careful reading of which, prompted by Prof. Lister's disclaimer, permits me to conclude that the expression, 'the organization of these ligatures,' is not intended to convey the thought that the tissues of the ligature are revitalized, but that a living tissue takes the place of the absorbed ligature; and that the expression, 'a ring of living tissue,' does not refer to a revitalization of the tissues of the ligature, but to a new tissue occupying the former site of the catgut, as if cast in its bed."

[2] The Behavior of Carbolized Catgut inserted among Living Tissues; by Wm. J. Fleming; Lancet, May, 1876.

forty years he has been in the habit of using the sinews of the deer for ligating vessels. He says: "I have never used carbolized spray or ligatures. The tendons of the deer, dried and torn into shreds and rolled into ligatures, are what I employ. They are absorbed, and I have never seen a knot made in tying a vessel. I have occasionally used them as sutures, but generally apply silk for this purpose." Dr. Eve's experience with the tendons of the deer leaves us free to conclude that the carbolized oil and water with which Prof. Lister's catgut is prepared has no special value in preventing suppuration or hastening absorption.

The following propositions are sustained by our present knowledge of the subject of Antiseptic Surgery:—

I. Putrefaction may and does occur in the solids and liquids of the body both with and without the direct contact of germs borne in the air or water. In sustaining this proposition, I need only call attention to the innumerable abscesses containing fetid pus that may occupy any site within the human body, sites so remote from all the mucous and cutaneous surfaces that neither air nor water as such can reach them.

II. Putrefaction of the solids and liquids of an open wound may in many cases be prevented by not permitting the contact of living germs with the surface, or by destroying their vitality after contact with it. The experiments made by Prof. Tyndall are so conclusive that to deny their force and completeness seems irrational. If they were negative, or if they were few, a question might arise, but being positive and almost innumerable, there seems no reasonable doubt of the correctness of his conclusions. In his admirable series of experiments he allowed the floating particles to settle from the air until it was optically pure, and found that infusions placed in this pure air did not undergo putrefaction, and that living organisms were not produced. There are many agents by which the vitality of septic germs may be destroyed; and their contact with the cut surfaces is sufficient to destroy the vitality of such germs as may be present at the moment. While it is exceedingly difficult in the case of an open wound to prevent the contact of septic germs that pervade the atmosphere, it may generally be accomplished for a brief period. And while there are many substances that efficiently destroy the vitality of septic germs, the profusion in which they exist renders it necessary that the applications for their destruction shall be so precisely made, and so frequently renewed, that it will in practice rarely be accomplished.

III. It is possible that the living solids and liquids of the body may be so altered that they shall not furnish the conditions necessary to putrefaction. Polli's experiments with the sulphites afford evidence of the truth of this proposition. For he found that the bodies of dogs fed with the sulphites did not decompose, and that urine passed during the administration of sulphites did not putrefy.

IV. Practically, the conditions to be met in preventing putrefaction are so difficult, that in many cases it is impossible to comply with them; yet even partial success is eminently worthy of our best efforts. In the experiments made by Prof. Tyndall, in which he determined the presence of germinal protoplasm in the air, it appears that several days of absolute quiet are necessary for the complete settling of the germs so that the air becomes free from them. These particles of living matter are so infinitely minute[1] that they are utterly beyond the reach of the

[1] Spontaneous Generation Controversy, by Rev. W. H. Dallinger; Monthly Microscopical Journal, vols. x. and xiii. See Popular Science Monthly.

microscope, and can only be made visible by a condensed beam of light. Dallinger and Drysdale have shown that, while the life of the monad will not resist a temperature of 140°, the germs that Prof. Tyndall recognized by his electric beam are capable of germinating after having been subjected to a temperature of 300°. In view, then, of these facts, and the one established by Pasteur, that putrefaction occurs only in the presence of living germs, and that Prof. Fischer has uniformly found bacteria under antiseptic dressings so ingeniously devised as those of Prof. Lister, we must conclude that germs cannot be prevented from coming in contact with wounds. While this is true, the rapidity of putrefactive changes may be diminished, the absorption of septic matters limited, and the eliminative functions stimulated so that septicæmia may not occur.

In conclusion: Putrefaction does not occur in the absence of living organisms; this fact was established by Pasteur and is supported by Tyndall.

The germinal matter active in putrefactive changes is found abundantly in air and water; most conclusive evidence of this fact is furnished by the experiments of Tyndall and Rindfleisch.

The germinal matter resists exposure to a temperature of 300°, while monads cease to manifest vital phenomena after exposure to 140°; the experiments of Dallinger and Drysdale are presented to sustain this position.

Septicæmia is induced by the presence of the products of putrefaction in the blood; the precise element which causes by its presence septicæmia is not determined.

Professional experience teaches, and the experience of Billroth demonstrates, that absorption of putrefactive products does not occur with sufficient rapidity from a granulating surface to produce blood-poisoning.

Hence, in the early stages of the healing of injuries, septicæmia is more to be feared than after granulations have been formed; and the danger in this period is heightened by the presence of dead and decomposing fragments of original tissue.

Antiseptics may simply strain out the catalytic germs, as cotton-wool; or destroy the germs, as carbolic acid, salicylic acid, the salts of copper, zinc, mercury, etc.

Antiseptics may destroy the products of putrefaction, as iodine, bromine, chlorine, and permanganate of potassium, etc.

Antiseptics may alter the putrescible matters so that putrefaction cannot occur, as sulphurous acid, etc.; or antiseptics may alter the tissues of a wounded surface so that, though septic matters are present, they may not be absorbed, as perchloride of iron, etc.

The rapid removal of septic matter from contact with an absorbing surface will prevent absorption, as is done by the water-bath.

The difficulty encountered by experimenters in preventing the development of infusoria in vegetable and animal infusions, teaches, and the experience of surgeons confirms, the impossibility of invariably preventing the admission of germs to putrescible substances, either in the most carefully constructed reservoirs or on the free surface of the human body.[1]

[1] I am indebted to Drs. Evers and Boutwell for translations from the German, and to Drs. Boisliniere and Lebeau for translations from the French. I must also acknowledge my obligations for the use of the Army Medical Library, at Washington, and of the valuable collection of the College of Physicians of Philadelphia.

Discussion on Dr. Hodgen's Paper.

After the reading of the preceding paper, Dr. Addinell Hewson, of Philadelphia, said:—I have been for many years interested in the subject of Antiseptic Surgery, and have tried almost every substance which has been introduced for the purpose of destroying germs. I have relied principally of late upon *earth*, as I am satisfied that *dryness* is the most essential element in any dressing. As Prof. Hodgen has said, water produces germs rapidly, and it is impossible to prevent this even with powerful antiseptic agents. I have therefore, during the past year, at the Pennsylvania Hospital, absolutely prohibited the washing of any wound during its entire treatment. In addition to dry dressings, it is important to remember that there are other necessary conditions for rapid healing, and I have not used a ligature in the Pennsylvania Hospital for ten years, employing always either acupressure or torsion, and thereby avoiding foreign bodies in the wound. I also enjoin quietude of the parts, with infrequent changes of dressings; I have also tried the effect of color, by covering wounds with blue paper, to exclude all but the actinic rays. For six months I have tried salicylic acid, sprinkling it freely upon the wound, and covering the latter with cotton-wool and then with blue paper. I never wash wounds nor disturb the dressing save to remove the outer layers when they are soiled. The salicylic acid produces no pain, but acts as a sedative, so much so that at first I thought it was narcotic, but I subsequently found this to be an error. I am seldom obliged to give morphia, but, to satisfy the patient, have tried hypodermic injections of a fluidrachm of cold water, a remedy which I have also found successful in relieving sciatica. I have partially superseded earth by salicylic acid, but have not abandoned the former, save when the prejudices of patients or nurses have thrown obstacles in the way of its successful employment.

Dr. J. H. Pooley, of Columbus, Ohio, said:—I have come to consider carbolic acid as important in surgery as instruments themselves. I have known large ragged wounds to heal under its application without one drop of pus. I have found the most efficacious form to be one part of carbolic acid to seven of olive oil, which is less offensive than linseed oil. I always make the application with a brush, carrying the oil well down into every portion of the wound, then applying lint saturated with the same mixture, and retaining all with a bandage. This should be repeated every twenty-four hours, brushing the oil in thoroughly, and removing all sloughs. I have recently seen an extensive crush of the soft tissues of the foot heal, without discharge, in three weeks under this dressing, while under ordinary circumstances the cure would have occupied two or three months. It must be added that, while carbolic acid for a time assists the healing process, yet after a certain period it seems positively to retard cicatrization, pointing to the possibility that this acid has the same repressing power upon granulations that it has upon germs. I have had but little experience with salicylic acid, but in one case in which I used it upon an immense mass of granulations situated over the region of the liver, and from which had been discharged a large number of gall stones, the dusting of the surface occasioned severe pain, an opposite effect to that described by Dr. Hewson, who has stated that the application is painless.

Dr. Hewson said:—In amputations, when I close the wound with a covering of powdered salicylic acid and cotton-wool, there is no pain; but the same dressing in open wounds does give pain. I should add that I always close the wound, not with adhesive plaster, but with strips of gauze and collodion, which is less irritating and favors primary union, besides giving the advantage of a visible wound, and preventing the direct contact of the salicylic acid or earth.

Dr. William Canniff, of Toronto, Canada, said:—It is, I believe, very generally acknowledged that the air is inhabited by various forms of germs—

organisms possessing different degrees of vitality; but it remains a question whether putrefaction is due to the operation of these microscopic organisms. Dr. Hodgen's first proposition states that "Putrefaction may and does occur in the solids and liquids of the body, both with and without the direct contact of germs found in the air or water." Well, if that is the case, I submit that the doctrine urged by Prof. Lister is undermined.

Dr. Hodgen said:—I did not intend to convey the meaning that germs are not present in all cases of putrefaction. The organisms may be introduced to the part through the blood.

Dr. Canniff said:—Then what is the use of applying to a wound or open abscess means to form a barrier to the ingress of germs? If organisms can enter the system through the lungs, or any other mucous surface, and find their way to a certain part to work mischief, it is entirely futile to adopt any procedure to prevent their action. I should like to ask the advocates of the germ theory if they hold that organic matter can never putrefy without the operation of air germs? Because if it ever does, it would be difficult in any given case of putrefaction to show that this was not a simple chemical change in the elements composing the dead animal matter. It is a received doctrine that the body is made up of an infinite number of cells, or molecules, each of which possesses a distinct individual life. They have a moment of birth, and a period of growth and development, of maturity, of decline, and of death; and during their period of life they, like the whole body, are subject to disease, and to degeneration; they may be feeble, or possessed of vigorous vitality. They are also individually and in number liable to injury. Take a contused wound; in it may be found a number of cells crushed to death; others are injured in various degrees, some of which will or may under favorable circumstances recover and be completely or partially restored to their normal vitality. Which must perish and which shall be restored, has to be determined by nature. The line of demarcation has to be established; and the cells deprived of life sequestrated. This is a work of time, and is accomplished by natural laws. I am unable to see in what way the presence of germs can affect this procedure. If a portion of tissue is crushed to death, beneath it is carried out the work of sequestration; and this is attended with chemical changes in the dead organic matter. It is asking a good deal to expect us to believe that this is due to air germs.

My own experience is such that I find it impossible to entertain the doctrine of germ putrefaction. I will mention three cases in my own practice: A man of over sixty had his leg crushed by a wagon heavily laden. There was a compound dislocation of the knee-joint, with fracture of the upper extremity of the tibia. The limb was placed in a proper position, the parts adjusted, and cold water dressing alone applied. The wound was freely and constantly exposed to the air; and, although the weather was hot, the wound healed, and the man recovered with perfect use of the joint and with no suppuration whatever. Two men, injured at the same time by a railway accident, were brought into the Toronto General Hospital. One had contused wounds of the leg. Cold water dressing was employed; and he made a rapid recovery. The other man, in a bed beside the first, had severe contusions of the leg, but no wound. The skin was unbroken. In a number of days it was found that suppuration had taken place. The abscess was opened, and forth came putrefying pus. Now here the putrefaction must have taken place without the presence of germs, or else they had been exceedingly active in finding their way through a mucous surface and by the blood to the part. I may say that the practice recommended by Prof. Lister has not been adopted in the Toronto Hospital; his doctrine is not as yet acted upon; and I feel safe in saying that the success attending the treatment of wounds in that Institution is not exceeded by that in any other. Of course, in hospitals and other places, the organisms which exist in the air are often depraved; they take a low form of degraded life, and the result is an impure air which will more or less affect open tissue;

but I am referring to ordinary air. I have in hundreds of cases seen incised wounds, many in connection with plastic operations, in which healing by primary adhesion has taken place, and yet in which no care has been taken to keep away supposed germs. Often the wound is allowed to remain open for some time, until all oozing of blood has ceased. Now how are we to account for these results, if the air is everywhere peopled by organisms bent on working destruction whenever a solution of continuity takes place on the surface of the body?

In speaking of the treatment pursued in the Toronto Hospital, I do not intend to convey the impression that the agents called antiseptics are not used. They are constantly employed for purifying tissue, not with the view of destroying or preventing the action of air germs, but to arrest or prevent putrefying decomposition due to chemical changes. In conclusion, I would say that I believe that everything, almost, depends upon *cleanliness* in the management of wounds; and I believe that the success which has been obtained by those who practise according to Prof. Lister's theory, is greatly due to the cleanliness which that practice secures. Next to cleanliness, I think that the maintenance of *rest* is valuable; physical and physiological rest. Rest after the evacuation of an abscess will often secure an early adhesion of the walls of the collapsed sac. *Pressure* again is an important element in the treatment of wounds so as to press away fluid in the part, and to prevent feeble circulation or stagnation of blood in the tissues which have to supply the reparative material. And lastly, attention must be given to the constitution and to the surroundings of the patient. Is it not to be feared that the particular treatment advised by Prof. Lister tends to divert the attention of the surgeon from these essential points?

Dr. F. H. Hamilton, of New York, said:—In common with all the other members of the surgical section, I feel a great interest in what you, Mr. President, will have to say on the subject of antiseptics in surgery. The medical men of this country are familiar with your labors in this direction. They know with how much zeal and intelligence you have prosecuted your inquiries, and I may add for myself, personally, that I have never seen a medical gentleman who had enjoyed the privilege of witnessing your practice in your own wards, who was not delighted with the results. Nevertheless, a large proportion of American surgeons seem not to have adopted the practice; whether from a lack of confidence or for other reasons, I cannot say. We are happy, therefore, that you have this opportunity to address us at this time, and we hope sincerely that you will be able to remove such doubts and practical difficulties as remain in our way. Indeed we would be glad to have you convince us that your method is the best.

It seems necessary to remind the Section, however, that there are in this country and abroad other methods of treating open wounds, for which their advocates claim exceptional and extraordinary results. Recently our attention has been called by a paper read before the American Medical Association, I think at St. Louis, to what is called the "open-air treatment," in which no dressings whatever are employed, but the wound is left open to the air, the discharges being permitted to drop into proper receptacles, or to dry upon the surface. My friend, Prof. Moore, of Rochester, called my attention to a case of amputation at the shoulder-joint treated in this manner in one of his surgical wards, which was closing rapidly and with very little suppuration. At the Bellevue Hospital this method has been employed recently, in quite a number of major amputations, and with remarkable success. It must be noticed that the open-air treatment is the exact opposite of Prof. Lister's antiseptic method, yet there are not a few who claim for it equally good results with those obtained by Prof. Lister. A few years since I was appointed operating surgeon to the St. Francis's Hospital, a hospital of several hundred beds, in the city of New York, where I found Dr. Rose and the other gentlemen (all of whom were German, and thoroughly educated) employing warm water in

the treatment of nearly all surgical accidents. The same plan was pursued by these gentlemen, under my observation, after I became connected with the Hospital, and, I must say, with most extraordinary results. No ointments nor antiseptic washes were used to my knowledge, unless as a rare exception, but the warm water dressings were applied to all open sores, whether simply traumatic or specific, in the form of either baths or fomentations. Under this treatment, pyæmia, septicæmia, erysipelas and gangrene were almost unknown, except as these originated outside of the hospital and before admission to the wards. That is to say, under the warm-water treatment the origin or development of these conditions was almost unknown.

The same plan has been adopted to a certain extent by myself and some of my colleagues in the Bellevue Hospital, and several of the gentlemen, including Prof. Gouley, who I believe is present, have attested its excellence, if not its superiority. My largest and most satisfactory experience, however, has been obtained while occupying the position of Surgeon-in-Chief of the New York Reception Hospitals. The results have been given to the profession in a couple of papers, one of which was published in the Richmond and Louisville Medical Journal, in 1874, and the other in the Medical Record of New York, during the same year; but the most complete essay upon the subject was published, in 1875, by my former pupil, Dr. Frederick E. Hyde, of New York, in the Buffalo Medical Journal, and subsequently in pamphlet form.

There are yet other plans for the treatment of wounds and other surgical accidents, for which their advocates have made special claims, all of which may be considered as contending with the method advocated by Prof. Lister, for respectful consideration, if not for preference.

It may not be possible for this body to decide now between the merits of these several methods, but we shall listen with great interest to what the distinguished gentleman from Edinburgh shall say upon this subject.

Dr. J. A. Grant, of Ottawa, said:—For many years we used cold-water dressings at the Ottawa Hospital, with great success, yet failed to obtain results equal to those secured by Prof. Lister. I therefore made a visit to Edinburgh, and was fully convinced of the superiority of the antiseptic method. I was particularly attracted by the extreme care and cleanliness which characterized all of Prof. Lister's operations.

Dr. R. A. Kinloch, of Charleston, said:—While I am willing to accord to the antiseptic system, carried out in the various ways which have been suggested, all or much that has been claimed for it, I think that *rest* is an important factor in the treatment of wounds, which is too much overlooked, and to which may perhaps be attributed much of the good claimed for certain kinds of dressing. The value of immobility is recognized in the treatment of fractures and joint diseases, but it is equally important in dealing with lesions of the soft parts. The benefits said to have been derived from the use of warm water, may be in part due to the quietude of the limb within the box in which it is placed during the course of treatment. The cotton dressing serves admirably the purpose of securing apposition and immobility of the wounded tissue, and many of the forms of antiseptic dressing are similarly effective, because the dressing is but seldom disturbed. The dirt dressing of Dr. Hewson may do good in the same way; it moderately compresses, supports, and keeps at rest the injured structures. Even the plan now carried out at Bellevue Hospital, of exposing wounds to the air, may act in the same manner. The parts are secured in their proper situation, and not handled; a crust forms on the surface, and serves still further to protect and insure rest to the parts.

Dr. John T. Carpenter, of Pottsville, said:—I enjoyed the opportunity of practising antiseptic surgery on a somewhat extended scale, many years before the origination of the germ theory or of the use of carbolic acid. It was my fortune, in 1862, as Medical Director of the U. S. Army Hospitals in and around Cincinnati, to supervise the treatment of a large number of cases of Hospital Gangrene. Recognizing the extraordinary virulence of this septic

disease, and yet believing its constitutional symptoms to be exclusively the result of local infection, I directed the employment of an antiseptic local treatment, which met with most gratifying success. The hospital surgeons were directed to remove, as thoroughly as possible, the whole of the diseased tissues with the knife; to bathe the wound with *freshly generated* chlorine gas (used as chlorine water); to pack the then purified raw surfaces with lint saturated in creasote; and to trust to free ventilation, good food, and tonic treatment, for the rest. Of more than a hundred cases of hospital gangrene thus faithfully treated, I recall now but two which proved fatal, one from erosion of a large branch of the femoral artery, the other from thrombosis with enormous heart-clot. Here then was made a decided attempt, in treating a putrefactive disease: (1) to remove the poisoned tissues; (2) to disinfect those which could not be removed, and (3) to occupy the wound with a protective dressing as a means of preventing any re-inoculation by a poison which we all recognized as existing in the atmosphere of the wards. Here were recognized at that early date the principles of causation and of treatment which have only recently been thoroughly expanded and perfectly carried out. I may add that creasote was the only chemical agent known at that time which could substitute or replace our present carbolic acid, and which would produce results at all similar.

Dr. Frederic Hyde, of Cortland Village, N. Y., said:—In according value to any form of topical treatment, whether antiseptic or not, the true condition of the part, both at the time of and often anterior to the occurrence of the injury, should be taken into account. Whether the conditions existing before the wound or injury was received were strictly physiological, or more or less pathological, will materially influence the results of any and all forms of local treatment, and whatever importance and value may belong to one mode or another, we shall observe in the results that the rapidity or tardiness of repair, and the degree of recovery, will depend much upon the state of the patient at the time the injury was received.

Dr. R. F. Weir, of New York, said:—Perfect cleanliness is important in the treatment of wounds. By using cotton waste instead of sponges, by the employment of the douche for washing wounds, and by paying great attention to the cleansing of bedding, etc., we have been able to almost banish septicæmic diseases from the Roosevelt Hospital. We use both carbolic and salicylic acid, but not for the purpose of excluding germs from wounds.

On motion, the discussion of Dr. Hodgen's paper was adjourned, and the President, Professor Lister, was requested to address the Section the next day upon the subject of Antiseptic Surgery.

ANTISEPTIC SURGERY.

REPORT OF REMARKS MADE BEFORE THE SURGICAL SECTION, DURING THE ADJOURNED DISCUSSION ON DR. HODGEN'S PAPER.

BY

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(PRESIDENT OF THE SECTION.)

Having been requested to address the Section on the subject of Antiseptic Surgery, I will reply to some of the objections which have from time to time been raised against this plan of treatment.

The first objection is, that it is "too much trouble." I grant that it is somewhat troublesome, and yet I believe that the trouble is more imaginary than real, and is one of the necessary results of a want of the proper apparatus and appliances, together with inexperience as to their use. There is always trouble with any operation until one has learned the minutiæ of the various steps. But even supposing that it does require a little more attention at the time of the operation, I think that when the infrequency of dressing, and the shortening of the period of cure, are taken into consideration, it will be seen that the labor performed is far less than when ordinary means are employed. Again, it is worth some trouble to be able to seal up an amputation, an exsection, or a large wound, with the absolute certainty that no evil effects will follow.

The second objection is, that "other modes of treatment give good results." I grant that they give good results, but not the best attainable. Without strict antiseptic treatment, it is impossible to freely lay open a knee-joint, with the certainty that no danger will follow; or to take out a wedge-shaped piece from either femur, as I have recently done, and yet to feel that such a produced compound fracture is perfectly safe, and without risk. And then we must consider the progress of the wound. The blood becomes organized under the dressings, and cicatrization occurs without one drop of pus, and almost without granulations, so that, when we take away the superficial blood-crust, a clean surface is found beneath. This I venture to say never could be done without antiseptic treatment. Again, to be able to open a chronic abscess of the spine, without fear of irritative fever, or pyæmia, requires the strictly antiseptic plan. And in acute abscess, it is pleasant to feel that after the procedure of opening and pressing out the accumulated material, not one drop more will form. Again, in the ligature of a large artery near a great branch, it is a great relief to know that rapid healing will occur, and that the danger of hemorrhage will be avoided. None of these things can be done without risk, unless putrefactive changes are strictly prevented.

I must object to the definition of Antiseptic Surgery, given by Prof. Hodgen, as that includes the disinfection of water-closets, etc. I pre-

fer to use the term "antiseptic system," and construe this as meaning "the dealing with surgical cases in such a way as to prevent the introduction of putrefactive influences into wounds."

Another objection which has been made, is that "putrefaction may be due to germs introduced from within;" but this is an objection easily met. Every simple fracture disproves it, for in every fracture we have a most serious wound, even although it is not visible to the eye—bones and soft tissues are all mutilated to a greater or less degree—and if partial destruction and mere feebleness of tissue would cause putrefaction, then we should have it occurring in every injury of this kind.

Another point raised by Dr. Hodgen, is that, when serious results occur after the opening of an abscess, they are caused not so much by putrefactive changes as by injury to the pyogenic membrane and consequent absorption. With all due deference, I would say that a pyogenic membrane is a much better absorbing surface than a mass of granulations. It is well known that large granulating surfaces, which are discharging so profusely as to taint with their horrible odor a whole hospital, yet are not sufficiently absorbent to injure the system to any perceptible degree.

Opening of such abscesses antiseptically, will certainly prevent putrefactive changes and a relaxed condition of the pyogenic membrane, and thus prevent constitutional disturbance. As to the cause of this disturbance, when the antiseptic system is not used, I would only say that in a chronic abscess the pyogenic membrane is not in the condition of a granulating surface. In acute abscesses it may be, but in chronic cavities we sometimes have no secretion of pus until after the abscess is opened. This is sometimes seen in cases of diseased bone, in which clear liquid is all that can be drawn off, and pus is not secreted until subsequently. In proportion to the chronicity of an abscess does its pyogenic membrane partake of the characteristics, not of a granulating surface, but of cicatricial tissue with the epidermis stripped off, and in this latter state it is vastly more likely to absorb. The bad symptoms usually appear within the first few days, while the membrane is in this condition, and patients sometimes die within twenty-four hours after the opening of the abscess, in which time the membrane certainly cannot have assumed the condition of a granulating surface. Pus may not form until the third day, while the irritative fever is usually observed earlier than this. The danger at first is from this irritative fever; afterwards, when the pyogenic membrane is in the condition of an open sore, from hectic.

[Prof. Lister then gave a practical demonstration of the application of the antiseptic system upon the person of a patient, and spoke of the different forms of carbolic acid.]

At first I used the German creasote, which is very offensive from the krealic acid which it contains. I now used the purest acid I can procure, which is absolute phenol, and I have found that its solubility and want of offensiveness are in direct proportion to its purity. Calvert's carbolic acid requires twenty parts of water to dissolve it, this only sixteen. Another advantage is that a solution in which there are no undissolved particles is far less irritating than one in which the little portions of pure acid are brought into direct contact either with the wound or the surgeon's hand. Pure carbolic acid is used most satisfactorily in water, and for detergent purposes it has no equal. It is exceedingly penetrating, as evidenced by its action on India-rubber, and when used in the proportion of

1 to 20, will most effectually purify every portion of the epidermis, while instruments soaked in the same solution are rendered thoroughly antiseptic. For the purposes of atomization, its volatility is an advantage, and although I most heartily wish that I could dispense with the spray, yet I do not see how it can be accomplished. Thiersch, of Leipsic, substituted salicylic acid for a time, but he has now returned to carbolic acid.

[Prof. Lister then showed a Spray Producer, manufactured by J. Gardner, of Edinburgh, which, by a modification of the French plan of having one tube and wick within another, was so arranged that alcohol was volatilized, and then this vapor burned, thus securing a very large flame and insuring a rapid stream from the boiler.]

When the pressure is sufficiently great, the atomization will be so fine that there will be scarcely any perceptible wetting of the surgeon's linen, and when thus minutely divided the carbolic acid is much less irritating to the wound. The large size of the stream is also of great advantage in giving a larger area for work. The surgeon should always see the spray, and then he will be certain that it is in the right place. When assistants are scarce, the atomizer can be placed upon a table and made to play upon the part.

The skin being throughly cleansed, the instruments soaked, the hands washed, finger-nails brushed, and sponges prepared, the incision is commenced; and from this time until the completion of the dressing, if the surgeon's hands or an instrument are passed beyond the area of spray, they must not be again brought near the wound until they have been dipped in the antiseptic solution. It is the close attention to these minute details that renders this system so absolutely certain and safe in its results. Carbolized catgut ligatures are always used, both ends being cut-off. Before closing the wound, a purified Chassaignac's drainage-tube is introduced, if needed, when the dressing can be applied with the perfect assurance that, every organism having been thus far excluded, none can enter the wound, and healing without suppuration will occur.

The gauze employed is simply loose, open, cheap, cotton-cloth, impregnated with carbolic acid, one part; paraffine, five parts; and common resin, seven parts, thoroughly spread over the surface when hot, and put under heavy pressure in a stout wooden box. The paraffine prevents stickiness, and the resin makes an excellent vehicle. In dressing a fresh wound it is best to protect the raw edges by a small piece of thin oiled silk, covered with copal varnish, in order to prevent any injurious effect from the direct contact of the carbolic acid, and to allow the blood clot more easily to organize. If this is done, a wound need not be opened for a week, but it will otherwise require earlier dressing. The next substance to be applied is a layer of gauze, soaked in fresh carbolic solution, since the dry gauze might contain organisms from accumulated dust, and in its dry state not be able to destroy them. Over this is placed half a dozen thicknesses of the same material, loosely wrapped around, the spray meanwhile playing continuously upon the part. Then is applied a layer of Mackintosh cloth, and over all is wrapped a loose bandage made of the same carbolized gauze torn into strips. The wound may now be left with a feeling of absolute security, and without any anxiety, for a week, or until the discharge begins to soak through, when the outer dressings may be raised under the spray, and fresh ones applied. The deep dressings need not be disturbed for weeks, if necessary. For changing the dressings, in private practice, I carry a small atomizer which can be packed in a box in the pocket. In wounds too large to be covered by the spray

from this instrument, one side can be dressed at a time. One or two dressings are all that are usually required.

I have found resin a most excellent vehicle for the carbolic acid, as it holds the latter with extreme tenacity, while at the same time it has no affinity for the discharges, allowing them to pass through for a long time without extracting the virtues of the acid. I have found also that the energy of a drug depends not alone upon its quantity, but upon the affinity of its vehicle, so that 1 to 5 of resin is no more pungent than 1 to 20 of oil, or 1 to 100 of water.

It may be thought that all this caution in dressing is unnecessary and troublesome, and it would be so if one had to do all this and then failed to secure a good result, but when these minutiæ are heeded bad results will not occur; wounds will heal so rapidly and kindly that one will soon come to find that it is not the most but the least troublesome mode of dressing, when he takes the course of the case into account. Many doubtless fall short of these good results simply because they neglect the precautions mentioned, and then they blame the system for their failure.

I have been making strenuous efforts to improve the catgut ligature, and believe that I have at last obtained the desired result. Old ligature is far preferable to new, and yet the knots will sometimes loosen. I have seen a case of Cæsarean section progress admirably until the stitches in the uterus gave way, and then death speedily followed. The old ligature is harder and does not absorb as readily as the new, but I have found that gut which is reliable when tested with warm water, will yet loosen under the action of the liquor sanguinis. The ligature must not be made too hard, or it will be too stiff for tying, and will even act as a foreign body as much as silk does, and yet it must be so hard that after soaking in serum for weeks it will still hold firmly. I have tried many substances; chromic acid will harden the gut, but when the latter is soaked in serum it is as unsatisfactory as before. Glycerine gives a ligature which will knot well, but is still too hard. I have at last made a mixture of carbolic acid, glycerine, chromic acid, spirit of wine, and water, which I think will prove the very thing required. The ligature which I show has been soaked a month in serum, and yet the knots are perfectly firm.

The germ theory of putrefaction is the foundation of the whole system of antiseptic surgery, and, if this theory is a fact, it is a fact of facts that the antiseptic system means the exclusion of all putrefactive organisms.

Burdon Sanderson a few years ago established a fact when he showed that water developed bacteria. When Pasteur's solution, with sugar and ammoniacal solutions, was boiled and exposed to the air, various forms of fungi were found, but no bacteria; but if one drop of undistilled water was added then these latter appeared. Again, when a wineglass was filled with cooled Pasteur's solution, bacteria were found in the contents; but these were attributed not to air dust but to adhering water upon the glass. If it were true that water alone was the carrier of these germs, it would be of no use to employ the spray; it would only be necessary to cleanse the hands and instruments. Formerly, when I used the carbolized putty, I have seen a joint which had been laid open do admirably until the blood clot at the wound was pulled away, when upon the next day the whole articulation would be in a state of putrefaction. I should like to know how this could occur save from air dust.

[Prof. Lister then showed a glass stand, cup, and cover, which he had used for his experiments. The cast-iron box in which they were heated to 300° F., in order to destroy all living organisms, was fitted with a cover, in the groove in the lid of which was placed cotton, in order to filter the air which would otherwise pass uninterrupted through the crevice during the cooling process.]

If a liquid free from organisms is introduced into such a prepared glass, it will remain without fermentation until it dries up. This proves that if a liquid be freed from living organisms, putrefaction will not occur. The lid may be raised for a moment, and some of the liquid secured for experimentation, yet no dust will enter during the short space of time required for this manœuvre. If instead of Pasteur's solution we use boiled milk, and remove the cover for fifteen minutes, then bacteria will appear. Milk is more favorable for the development of different kinds of bacteria than Pasteur's solution. We may have these organisms without putrefactive changes, and I have found them in milk treated as above described for days without any smell or putrefaction; but if we let in air or water, at once the liquid will putrefy. This would indicate that there are bacteria of several kinds, just as there are great numbers of torulæ which cause alcoholic fermentation. Boiled milk is not prone to sour, but if we introduce one drop of sweet milk, we almost immediately find bacteria.

Again, suppose that we try how such organisms behave in boiled urine. It may be objected that the boiling will prevent decomposition of the urine, but it is possible to obtain pure urine which has never been exposed to the influences of the air, and in which there is therefore no need of boiling to destroy any germs, unless those germs existed in the bladder. It may be set down as a rule that living tissues prevent the development of bacteria in their immediate vicinity. This is instanced in the puncture of an œdematous limb, where the liquor sanguinis which does not ooze away never putrefies, while if it were situated in meshes of cotton instead of connective tissue it would certainly do so, showing that the living structures prevent this process. Again, take the instance of healing under a scab; here is an underlying layer of liquor sanguinis, yet fermentation never occurs. Hence we might infer that the mucous membrane of the urethra would also exercise a forbidding effect upon the development of these germs, since, although the acts of micturition are infrequent, no organisms are ever found beyond the meatus, even when the glans and prepuce are abundantly supplied with them.

For experimentation, let the penis be thoroughly washed with a solution of carbolic acid, 1 to 40, which will produce no irritation; then, taking a flask which has been removed from the "heating box" before described, gently lift the cover, and let the patient micturate into the flask. One will then have pure, uncontaminated, unboiled urine, which will keep for a year, even although it contains vesical mucus. When it is wished to obtain some of the contents of this flask, it is only necessary to remove the cap, slip over the mouth one-half of a rubber ball, which has been steeped in carbolic acid, and through a hole in this any amount can be poured, the little drop remaining at the orifice acting as a valve.

Into one of these glasses of unboiled, pure urine, put one drop of boiled milk. The first day, nothing; the second, a dimness and milkiness; and now, if we look, we will find long, spiral forms of bacteria. Inoculate a second glass of urine with one drop of this liquid; and now the bacteria, having become accustomed to the liquid, will cause it to become filled with germs within a few hours. Transfer a drop from the second

glass into Pasteur's solution; again we have bacteria, but of different forms. The former were motionless; these are quite active. Now inoculate a glass of boiled milk, and large, moving germs are soon seen. One drop of this last will produce lactic acid fermentation almost immediately. If we believe that torulæ are the cause of alcoholic fermentation, *à fortiori* we must believe that the bacteria lactis are the cause of lactic acid fermentation.

In regard to the inferences to be drawn from these experiments in regard to the healing of wounds, I would ask why the effused lymph, in the operation for harelip, does not putrefy? I believe that the explanation is to be found in the fact that this lymph lies between two healthy living surfaces, which prevent the development of organisms just as does the urethral mucous membrane. I believe that we should even go further, and say that blood-clot should also be regarded as tissue. I made an experiment several years ago, which consisted in laying bare the jugular vein of an ox, just struck down, applying two ligatures, and cutting away the vein beyond each, thus obtaining a receptacle of living tissue containing blood. This vein could be kept for hours without any change taking place in its contents. I then drew out the blood into a flask, and covered it with gutta-percha to prevent evaporation and dust; for twenty-four hours the blood still remained fluid, save where it was in contact with the glass, but the instant that the glass was stirred, coagulation took place.

The formation of clots in aneurismal sacs may be explained by this experiment; as long as the wall remains smooth and uninjured, clotting does not occur; but when the sac is injured, or free circulation interfered with, then coagulation takes place. If the blood-clot in wounds does not receive injury, it will organize, and should be accounted as tissue. In this blood-clot are many consituents—fibrin, white corpuscles, etc. These leucocytes act just as the connective-tissue corpuscles, and prevent fermentation, and the explanation of the organization of blood-clots, when not disturbed, can thus be explained. Putrefactive changes may be introduced into the clot, and yet putrefaction not occur—a circumstance which can be best accounted for by the fact that the clot acts as a living tissue, and thus prevents putrefaction, just as does the urethral mucous membrane before mentioned. There are undoubtedly often deep blood-clots in the tissues when acupressure is used after amputations; and yet these fail to putrefy, but become organized. This may occur under any form of treatment, but by employing the antiseptic system such a result is rendered exceedingly probable.

As regards the question of constitutional treatment which has been raised, I would say that, with strict antiseptic precautions, the wound is placed in a condition similar to that of a simple fracture, in which the injured parts are free from the dangers of atmospheric exposure, and the surgeon has far less need to attend so closely to the patient's constitutional condition, as the cases will almost always progress favorably.

The statement that cell-forms have been found beneath antiseptic dressings, must be received with caution. I have recently met a gentleman who was with Raneke, in Halle, when he found, as he supposed, these organisms beneath antiseptic coverings; and when this gentleman pointed out to me the bacteria which he called putrefactive, I at once recognized them as of the non-putrefactive variety; and the gentleman was forced to admit that they differed from those found in decomposing masses.

As regards bacteria found in abscesses when the skin is not broken, and in which Dr. Hodgen stated that the forms came from within, I think the instances are very rare, although I acknowledge that such cases do occur. Again, it does not follow that every foul-smelling abscess is putrefactive. In ischio-rectal abscesses there is usually a stinking odor, even although no communication has been established with the bowel. My treatment for this class of cases has been not to poultice them, for that would surely make a true fistula, but, after thoroughly disinfecting the skin, to lay them freely open under the spray. I then cover them with lint soaked in carbolized oil, and over the mass place a T-bandage, which can be drawn aside at each act of defecation. I never inject them, nor use a drainage tube. Under this treatment they soon heal without either suppuration or odor; and I have never failed to secure this result save in one case, in which the house-surgeon, on the first day, failed to change the dressings under the spray.

In regard to salicylic acid, I think its action is chemical, and that it is especially useful in destroying those odors which are not the result of putrefactive change. That it has this chemical property is proved by the fact that, when mixed with amygdaline and emulsine, it prevents the formation of hydrocyanic acid. Mixed with mustard, also, it prevents the evolution of the volatile oil. I have thought that the action of salicylic acid is very similar upon living tissues. Other classes of cases for which this agent is especially suitable, are compound fractures, which do better if kept closed for weeks, and yet are liable to become offensive before the expiration of that time, and chronic abscesses connected with diseased vertebræ, when the skin is excoriated and ulcerated. In such cases I simply smear the under surface of the carbolized gauze with the powder of salicylic acid, and then have allowed a single covering to remain for six weeks without either odor or irritation.

Discussion on Mr. Lister's Address.

Upon the conclusion of Prof. Lister's remarks, Dr. W. H. Van Buren, of New York, said:—I would ask how far the antiseptic treatment is applicable to a certain class of operations, as, for instance, those for the relief of strangulated hernia; and how far it is safe to throw the carbolized spray upon the peritoneum in ovariotomy and laparotomy? Absorption has occurred in such cases, as evidenced by the production of hæmaturia, and other symptoms of carbolic acid poisoning.

Mr. Lister said:—Upon this point I can speak from experience, as I have repeatedly operated for strangulated hernia under the spray, and, in one case of abdominal wound, a cloth soaked in the carbolic solution lay upon the exposed intestines for several hours without producing any irritation. As for ovariotomy, I am informed that Mr. Keith now uses the spray in all his operations, and also that Nüssbaum attributes his great success in this class of cases to the use of the antiseptic system. I myself think that the danger to be apprehended is not so much from peritonitis as from the toxic effects of the acid. In one case, in which a large ovarian tumor had been removed and hemorrhage occurred, I did not open the wound, but simply applied a large carbolized sponge, with pressure, and although a large amount of clotted blood remained in the peritoneal cavity, yet no inflammation resulted. I have seen a few cases of danger from the toxic effects of carbolic acid, the symptoms being weakness, failure of appetite, sometimes vomiting, and the urine very much darkened. These effects have seldom occurred since I have ceased the

use of the antiseptic putty, and I believe that they will only be found where there is a peculiar idiosyncrasy.

Salicylic acid I consider inferior to carbolic acid as a detergent, yet, in those cases in which a deep dressing is to be left on for weeks, and in which the odor is apt to become very offensive, I have used it with great advantage. This odor is particularly disagreeable in dressings applied to the feet, axilla, or perineum, and is not due to putrefaction or to the presence of organisms.

Dr. T. E. SATTERTHWAITE, of New York, said:—The vegetable germ theory seems to be regarded by some as an essential foundation for the antiseptic system of surgery.

The hour being late, I merely desire to point out a few facts which, in my opinion, militate against the theory, as far as it claims that a certain class of minute living organisms of a vegetable character is essential to disease-processes. Pasteur has, of late, considerably modified his ideas, so far as to acknowledge that alcoholic fermentation may be initiated without the presence of certain minute living vegetable organisms, in other words, torulæ or allied forms, whatever their name. A statement to this effect was made at the recent discussion on the Germ Theory in London. Le Chartier and Bellamy have observed that, in some modified forms of alcoholic fermentation, such minute organisms may at first be absent, though later they make their appearance. Again, Hiller has inoculated eggs with bacteria, and no putrefaction has ensued. Some French observers also have found no organisms present in putrefaction.

These facts are stated to show merely that it is questionable whether alcoholic fermentation and putrefaction are due solely to certain minute living vegetable organisms, to the growth and multiplication of which these processes are due.

The early views of Pasteur have met with opposition, notably from Liebig, Stahl, and others of the German school, and views of a somewhat similar nature are still maintained now by those who oppose the "germ theory." As for the changes which it has been stated take place in one and the same bacterial body, under differing circumstances, I think that this is an important and interesting fact, and one that has not as yet received sufficient recognition. But it is an argument that is exceedingly valuable to those who do not believe in the vegetable germ theory. It helps to explain a fact that has led to a great deal of confusion, viz., that the different organisms alleged to be found in different diseases may owe their differing form not to the disease, necessarily, but rather to the peculiar menstruum in which they happen to be.

With regard to the experiment recited by Mr. Lister, in which urine was passed into a glass vessel that contained air, but no floating particles, where the fluid continued to resist ammoniacal decomposition until, the air being admitted, it was seen to contain particles, I would say that the presence of these particles does not necessarily indicate that they are bacteria; in fact, the experiments of Blackley, in England, and of Wyman and others, have shown that the air contains numerous particles beside bacteria, such as the pollen of grasses, epithelial scales, and what is known as granular matter, whatever that may happen to be in any given case; and we have a right, in view of certain other experiments, such as the production of putrefaction without bacteria, to believe that some of these particles—not bacteria—may have occasioned the change.

The same statement applies to Prof. Tyndall's experiments, in which he passed the beam of the electric light through a box fitted with windows, where the box contained matter easily susceptible of putrefactive change. When the electric beam showed that the inclosed air contained floating motes, then the fluids soon commenced to putrefy, but when the beam showed that the particles were no longer in the air, the tubes might be introduced and exposed to the air, and yet would remain unchanged indefinitely. Now it is implied by Prof. Tyndall that the presence of particles is synonymous with the presence

of bacteria, which is certainly an assumption, as the electric beam does not show that these particles are bacteria in distinction from other particles.

Experiments with vacuum tubes in relation to putrid matters, if performed with delicacy, tend to show that the fluid of such infusions, although virulent in so far as inoculation is concerned, may still remain clear or transparent for an indefinite time, without the formation of bacteria.

In some experiments made with salicylic acid and putrid matter intimately mixed, infection took place in animals I inoculated, when the quantity of salicylic acid was sufficient to prevent the development of bacteria in organic infusions. These are simply a few of the points maintained by those who oppose the vegetable germ theory as generally accepted. In my paper read in the Section on Sanitary Science, these and other facts are stated at more length.

Mr. LISTER said:—I am of the opinion that Pasteur has not materially modified his views. The penicilium glaucum, without air, produces a degree of alcoholic fermentation; there are also certain forms, which when grown under water have thick filaments, with loose joints, and which closely resemble torulæ; but, if in oxygen, the growth is much more rapid, and the similitude is less marked. Pasteur made an experiment with grapes kept in an atmosphere of carbonic acid, and although they showed evident signs of fermentation, yet Pasteur attributed this process to the natural change in the vegetable cells of the fruit itself, and not to organisms.

I have made numerous experiments to determine the question as to whether the cause of fermentation is a chemical or a vital one; also whether this is in solution in water, or in suspension. I can hardly believe that living things are soluble, and yet if they are only in suspension, by taking minute enough drops, we ought to avoid them. I have tried as small a quantity as $\frac{1}{200}$ of a drop of putrefying liquid, introduced by means of a syringe into different solutions, and have found that no matter how small the quantity used, yet under favoring circumstances, bacteria are developed. I would mention as a point worthy of note, that I have discovered that bacteria are killed by the reduction of temperature to a degree still considerably above the freezing point.

Dr. SATTERTHWAITE said:—In regard to the modification of Pasteur's views which I stated had taken place, the instances referred to by Prof. Lister illustrate the point which I am desirous of making.

In certain of these instances, as in the well-known experiment where grapes were suspended in an atmosphere of carbonic acid, Pasteur admits[1] that fermentation took place without the intervention of certain minute organisms, by which is doubtless meant the yeast plant or other allied organisms. The ferment was, in this case, Pasteur said, the tissue elements of the fruit, the vegetable cells, rather than the minute organisms known as fungi, torulæ, etc. I am aware that Pasteur claims that this admission does not necessarily affect his doctrine, but I must still maintain that Pasteur's view, as generally understood, is considerably modified, to use no stronger expression. The ferment is in this case stated to be the substance of the fruit, rather than a minute microscopic organism ranked among the cryptogams. This is the only point I am desirous of making in this matter, and the object of introducing it is not so much because fermentation and putrefaction have to do with the discussion, as because the connection between minute living organisms and these processes is brought forward as good *à priori* evidence of a similar relation existing between other minute organisms, such as bacteria and disease-processes.

In reference to Liebig and others of the German school who have opposed Pasteur, I did not allude to them as expressing distinct statements as to the nature of the fermentative agent, for these views varied as the discussion on

[1] Bull. de l'Acad. de Médecine, Mar. 2, 1875, No. 9, p. 255; Lancet, April 10, 1875, p. 508.

these topics went on, but rather as showing that there have been and still are[1] those who believe that these changes are not dependent on the growth and multiplication of such living organisms as have been described.

In reference to the nature of disease-poisons, it does not necessarily follow that because they may not be bacteria, they may not be molecular; indeed, it is my present conviction that this is the case in a certain number of instances, as filtration experiments appear to show. The most virulent fluids with which I have experimented have never produced lesions when they have been passed through porous clay.

Mr. Lister said:—Pasteur has shown that if the grape is bruised, and then placed in the atmosphere of carbolic acid, fermentation no longer takes place; implying that only living tissues undergo this change. I have tried the experiment of passing solutions through pores so fine as to arrest bacteria, but I have found that albumen is also stopped, and it may be that chemical substances in solution are also prevented from passing; consequently the experiment proves nothing save that the cause of fermentation has been prevented from passing.

[1] Handworterb. d. r. und a. Chimie, 1848, S. 232; Annalen der Chim. u. Pharm., 153, S. 1, 6, 146. Bastian, Researches illustrative of the Physico-Chemical Theory of Fermentation; Proceedings of the Royal Society, No. 172, 1876.

THE TREATMENT OF ANEURISM.

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In seeking for information concerning the medical and surgical treatment of aneurism, we find by far the most ample experience in the records of English surgery. Breschet, in his well-known memoir on aneurism, published in Paris in 1832, frankly admits that the pathology of this disease had received more attention in England than in France, because of its greater frequency in that country.[1] The greater frequency of aneurism in the British Islands than elsewhere still exists; and, unless we accept the suggestion that it is the result of the more general use of beer and spirits in those countries, I must follow Breschet in ignoring its cause. Although our own country has been singularly prominent in the boldness and success of operations upon the great arteries for aneurism, yet the disease is not so frequent here as in England, and at the same time spirit-drinking is very common. On the continent of Europe, where the consumption of spirits is less general, aneurism is comparatively rare. In connection with the etiology of the disease it is to be remarked that the weight of opinion in favor of syphilis as a cause of aneurism is noticeably less than formerly.

In the very able lectures on the treatment of aneurism delivered in 1872, 1873, and 1874, by Mr. Timothy Holmes, as Professor of Surgical Pathology in the Royal College of Surgeons of London, he states that, in collecting his materials, he addressed letters to more than thirty hospital surgeons throughout Great Britain and Ireland, and that he received notes of no less than three hundred and thirty-seven cases of aneurism, in reply, covering ten years of practice. Mr. Holmes complains, moreover, that the pathological museums of London alone are so overstocked with material that it is difficult to master their contents, whilst the bulk of published matter outruns the best efforts of any one inquirer to keep up with it. Such recent and ample supply of material bearing on our subject leaves little scope for further research, and the evident ability shown in its collation and digestion gives these lectures great value. Since Guthrie lectured on this subject from the same chair more than forty years ago, we have had nothing more judicial and authoritative. I shall, of necessity, draw largely from them in this report.[2]

The purely medical treatment of aneurism, more especially of internal aneurism, has risen into greater importance within the past ten years. Mr. Jolliffe Tufnell, of Dublin, has fairly and ably demonstrated the posi-

[1] Mémoires de l'Académie de Médecine de Paris, t. iii.

[2] It is to be regretted that Mr. Holmes's eighteen lectures "on the medical and surgical treatment of aneurism in its various forms" are only accessible in the pages of the journals in which they were reported at the time, without any copies of the drawings, or tables, or pathological preparations by which they were illustrated. They are too valuable to the profession not to be reproduced in a more permanent form.

tive value of absolute repose in the horizontal position, conjoined with a greatly restricted diet, omitting entirely the frequent bleedings which had brought the so-called treatment of Valsalva into disfavor. Mr. Tufnell's successful results certainly justify the systematic enforcement of his method, especially in cases of thoracic and abdominal aneurism, which we are too much in the habit of regarding as incurable. "The method I advocate," says Mr. Tufnell, "is the imitation of nature's cure; the production artificially of that consolidation which in isolated cases has from time to time occurred spontaneously." The recumbent position is the secret of cure, *but it must be steadily maintained.* It places the same check upon the circulation in internal aneurism which in external cases is produced by mechanical appliances. Three months of horizontal position, with eight ounces of solid food and six ounces of fluid as daily allowance, were the measures with which he cured Doyle, a hackney coachman of thirty-five, with an abdominal aneurism as large as an orange.[1] Tufnell's method is legitimately derived from the principle so ably illustrated by the Irish surgeons when they established the value of compression as a remedy, the principle originally demonstrated by Hunter, that to produce coagulation in an aneurismal sac it is only necessary to slow its current of blood, not to arrest it entirely; and, in the judgment of the writer it is destined hereafter to play a prominent part in the therapeutics of aneurism.

The aid of drugs has been also invoked in the medical treatment of aneurism for the purpose of slowing the action of the heart and favoring coagulation; digitaline and ergotine have been used by Albertini; and Langenbeck has made trial of ergot in subcutaneous injection. Acetate of lead has also been used internally. I have met with little evidence in their favor. A case of orbital aneurism is reported by Dr. Holmes, of Chicago, which got well after six weeks' use of tincture of veratrum viride in doses of five minims, with a fluidrachm of extract of ergot, suggested by the late Professor Brainard.[2]

In a recently published work Dr. Balfour brings forward further evidences of the value of potassium iodide in causing solidification of internal aneurism.[3] This remedy has been tested by the profession, and there are favorable reports of it.[4] In many instances, however, the recumbent position and restricted diet have been employed at the same time with the iodide. In aneurism in a syphilitic subject its use would be indicated. Pretty certainly the iodide in such a case would increase the number of the red globules of the blood, as Keyes has observed to be the case under the use of small doses of mercury, and, possibly, also, its fibrinating power.[5] Until we gain more accurate knowledge of the causes

[1] The Successful Treatment of Internal Aneurism by Consolidation of the Contents of the Sac. By Jolliffe Tufnell, F.R.C.S.I., etc., President of the Royal College of Surgeons of Ireland, etc.; 2d edition, London, 1875.

[2] Amer. Journ. of Med. Sciences, July, 1864.

[3] Clinical Lectures on Diseases of Heart and Aorta; London, 1876.

[4] See case of cure of aortic aneurism in a woman of thirty-five, attributed to doses of the iodide of twelve to fifteen grains three times a day for several months, by Dr. T. M. Matthews, of Texas, in Amer. Journ. Med. Sci. for January, 1875, and others. Mr. Annandale, of Edinburgh, from "repeated observation," bears testimony "to the value of the iodide in relieving the symptoms and promoting coagulation in this disease." Brit. Med. Journ., Oct. 30, 1875, p. 550.

[5] On the Effect of Small Doses of Mercury in modifying the number of the Red Blood Corpuscles in Syphilis, a study of Blood-counting with the Hématimètre; by E. L. Keyes, M.D., etc.; New York, 1876.

which modify the quality or quantity of its fibrin, the mode of action of drugs in favoring coagulation of the blood must remain obscure.

The *ligature*, as applied by Hunter, maintains its character in a general way as the most prompt and certain of the surgical remedies for aneurism, and, when not employed in the first instance, it is always held in reserve as a final resort in all cases to which it is applicable; but safer methods of cure are constantly being sought for in order to escape the dangers which are still inseparable from the use of the ligature, viz., hemorrhage, gangrene, and inflammation of the aneurismal sac.

The ligature of carbolized catgut prepared by Professor Lister's method offers the best promise, at this time, as a preventive of secondary hemorrhage. Bickersteth, of Liverpool,[1] Pemberton, of Birmingham, Heath, Maunder and Holmes, of London, besides Professor Lister himself, certain of his colleagues of the Royal Infirmary of Edinburgh, and others, have recorded evidence that the carbolized catgut ligature is competent to constrict an artery for a sufficient time to bring about the consolidation of an aneurism without causing ulceration of its coats. After this duty has been accomplished the substance of the ligature then softens into a mass which is, as it seems, capable of serving as a nidus for cell-growth, and, having excited no suppuration whatever, the catgut becomes finally identified with the surrounding tissues.[2] Thus, after such a ligature has been applied, both of its ends may be cut off and the wound closed as for quick union. This latter result is more likely to be attained, according to Professor Lister, when the operation is done under carbolized spray, and the wound dressed according to the antiseptic method. The Edinburgh surgeon expresses his readiness to ligate the innominate artery by this method, believing confidently that the vessel will be obliterated with greatly diminished risk of suppuration and ulceration, and with increased chances of rapid consolidation of the wound.[3] A double surety would be thus obtained against secondary hemorrhage.

It is proper to state, however, that some instances have been recorded

[1] An Introductory Address on Recent Progress in Surgery, delivered at the Liverpool Medical Institution, Liverpool, 1871, p. 20.

[2] Observations on the Ligature of Arteries on the Antiseptic System, by Joseph Lister, F.R.S., etc., Lancet, April 3, 1869, p. 451. More recently, Professor Lister's statements have been confirmed by Mr. Fleming, Lecturer on Physiology in the Glasgow School of Medicine, in a very conclusive series of experiments on dogs and rabbits, reported in the London Lancet, May 27, 1876, p. 771. Mr. F. says: "The results of these experiments show that a gradual softening takes place from without in, the catgut breaking down and becoming infiltrated with cells, probably leucocytes. This part of the process takes from five days to about twenty, varying with the specimen of catgut, the tissue amongst which it is situated, and the age and vitality of the animal. Next the pultaceous mass into which it has been converted, begins to metamorphose, and is soon permeated with blood channels, and ultimately may be described as a cast of the catgut, in a kind of granulation tissue freely supplied with bloodvessels, which in many of my sections are very fully injected.

"If, then, we admit these conclusions, we can easily account for the different results obtained by the use of catgut in different hands. We see that it is in reality merely a temporary ligature, because when in the softened stage we cannot consider it to have any constricting effect.

"Whether, then, this temporary condition lasts long enough to produce embolic occlusion of the vessels, depends upon the sample of catgut and the vitality of the patient.

"In conclusion, my experiments seem to demonstrate that an aseptic, dead, foreign, animal body may under appropriate conditions become, by a process of softening, absorption, and re-deposition, changed into or replaced by a living, vascular, and comparatively highly organized, animal structure."

[3] Holmes's Lectures on the Treatment of Aneurism, ut supra, Lancet, June, 1872.

in which the catgut ligature has become prematurely soft, and pulsation has returned in the aneurism ; and in which, when the wound has not united, secondary hemorrhage has occurred.[1] The answer has been made that in these cases of failure the catgut may not have been well prepared, and that the necessary details of the antiseptic method were possibly neglected.[2]

Meanwhile, those who, like Mr. Maunder, lack confidence in the catgut ligature, have reverted to the use of carbolized silk, which was employed by Professor Lister before he adopted the catgut; and it has been found in several reported cases to answer the same purpose. There is no danger that the carbolized silk ligature will give way, but Lister had reason to suspect that it might provoke suppuration, and therefore preferred the catgut.[3]

The problem is to discover a mode of preparing catgut ligatures so that they shall possess first the right amount of strength and durability when exposed to soakage in the tissues, and at the same time that their very desirable quality of disappearing by blending with the tissues shall not be impaired. Professor Lister has just announced to us that he has reason to believe that he has succeeded in solving this problem, which has cost him so much labor. By this event surgery will have secured a great triumph over the danger of secondary hemorrhage after ligature of the great arteries for aneurism.

The employment of pressure upon an artery previous to its ligature has been heretofore regarded as a means of averting the danger of gangrene, which, according to Norris, causes 12.2 per cent. of all the deaths after ligature of large arteries for aneurism.[4] As early as 1820, Todd, of Dublin, had practised compression as a preparatory course prior to operation "in order that mortification of the limb might be prevented by allowing some progress to be made in establishing the collateral circu-

[1] My late colleague, Dr. Krackowizer, used a carbolized catgut ligature upon the primitive iliac artery for aneurism at the German Hospital, New York, and pulsation returned in the tumor within twenty-four hours, rendering it necessary to reopen the wound and apply another ligature. Similar cases are recorded by Dr. E. Watson, of Glasgow, in the Glasgow Med. Journ., May, 1870, p. 340 ; by Professor Spence, of Edinburgh, in the Lancet, June 5, 1869 ; by Mr. Holden, in St. Bartholomew's Hosp. Reports, vol. viii. p. 187 ; by Mr. H. L. Browne, Lancet, May 6, 1876 ; and by Mr. Maunder, in Surgery of the Arteries, London, 1875, p. 147, where the results of ten cases of "antiseptic" ligature are detailed, some of which are favorable, and others the reverse.

[2] It is only fair and just to Professor Lister that when the remedy he has proposed is under judgment, the conditions under which he employs it, and on the strength of which he recommends it, should be strictly complied with.

[3] Careful microscopic examination of the tissues around a loop of prepared silk which had successfully obliterated an artery, after rapid healing of the wound, in one of the lower animals, has shown as a rule the persistent presence of portions of the substance of the silk, which have been found in several instances surrounded by granulation tissue in process of organization along with some pus corpuscles, proving that silk does not become entirely blended with the tissues, and does not so readily lose its identity as the properly prepared catgut. Lister, ut supra.

In a recently published clinical lecture at the Royal Infirmary, Glasgow, Dr. Eben Watson describes a patient in whom he had tied both external iliacs for inguinal aneurism at an interval of nine months. Both operations were done under carbolic spray, carbolized silk ligatures applied, and then cut off. The wounds healed well, and the ligatures were never heard of. Dr. W. gives evidence for his belief that they became encapsulated. He prefers them to catgut, which, for reasons derived from his own experience, he considers less safe than silk. Lancet, Aug. 12, 1876, p. 213.

[4] Contributions to Practical Surgery, Philadelphia, 1873. In one hundred and eighty-eight cases of ligature of large arteries for aneurism, gangrene occurred in thirty-one, and was the cause of death in twenty-three.

lation."[1] This idea has been effectively disputed recently, by Mr. Holmes, who, controverting certain conclusions arrived at by Mr. Jonathan Hutchinson, asserts that the larger experience contained in his (Holmes's) collected table of hospital cases leads to precisely the opposite conclusion, viz., *that the mortality from ligature, after compression has failed, is* 10 *per cent. greater than when the Hunterian operation has been performed at once.*[2]

Mr. Clement Walter tied the external iliac, at St. George's Hospital, for inguinal aneurism, after six weeks' trial of pressure, both distal and proximal, as well as flexion, without avail, and the patient died in four days with incipient gangrene.[3] It is well known, also, that pressure, especially if intermittent, or irregularly applied, is liable to provoke inflammation of the sac; and that it is possible, in rare instances, for pressure itself, alone, to occasion gangrene, as in a case recently reported by Dr. Oscar H. Allis, of Philadelphia.[4] These facts should teach us, when it is decided to try compression, to use all possible means to command prompt success, and, these failing, *not to prolong unavailing efforts.*

The prestige acquired for the Hunterian operation by the brilliant successes of Astley Cooper, Abernethy, Travers, Mott, and others, caused the ligature to be regarded, during the first half of this century, as the remedy above all others for all forms of aneurism, wherever its application was possible; and it continued to be so regarded, in spite of its really high mortality, until the feasibility of a safer cure by compression was established by the Dublin surgeons. After it became an established fact that the disease was safely curable by compression, numerous other devices were proposed to attain the same end to the exclusion of the ligature, and of compression also; some of greater, others of less promise, but mainly of a bloodless and safer character. These methods, after a few remarks on compression, I propose to enumerate.

Meanwhile, it seems to me profitable to ask ourselves how far English and American surgeons may have carried a partiality for Hunter's operation to excess, and tied arteries for aneurism in cases in which other methods might have offered a better chance for life? That this has been the case in some degree we have the authority of Syme, of Edinburgh, undoubtedly one of the greatest operators of his day. Towards the close of his life, this surgeon formally raised the question as to the applicability of Hunter's operation to axillary and iliac aneurisms, after having advocated it, as he states, for thirty years. After a series of operations upon the greater arteries, unequalled in brilliancy and success, he felt himself justified in asserting that "aneurisms of the popliteal, femoral, and carotid arteries alone are proper subjects for ligature by the Hunterian method," and that all other aneurisms are more safely treated

[1] Practical Remarks on the Treatment of Aneurism by Compression, with plates of instruments, etc., by Jolliffe Tufnell, M.R.I.A., etc. etc., Dublin, 1851, p. 24. Tufnell here quotes Todd, who adds, "at the same time *not altogether without hope* that by diminishing the current of blood in the trunk of the artery so as to favor coagulation of the blood in the sac, a cure without operation might be effected." This is interesting as showing that the idea of cure by compression was still fairly in view at this early period.

[2] Lecture iv., part i., 3d series, Lancet, July, 1874. The probability of this conclusion had been already demonstrated by Dr. J. Ashhurst, Jr., in his System of Surgery, Phila., 1871, p. 543.

[3] St. George's Hospital Reports, vol. vi., 1871–72.

[4] In a robust colored man of 50, with a recent popliteal aneurism, a cure was effected on a second attempt at compression in thirteen hours. Gangrene appeared in the foot and leg on the next day, and the patient died in eight days. Dr. Allis states that he has found one other case of gangrene after pressure. Trans. Path. Soc., Phila., 1874, vol. iv. p. 117.

by the old operation of laying open the sac.[1] After careful study of his very interesting cases, it is difficult to refuse a large share of assent to Syme's conclusions. Even if we cannot admit his premises, which have been shown to be untenable by Holmes and Henry Lee, and refuse to return formally to the "old operation," preferring to trust to the medical treatment of Tufnell, or to the promises held out by galvano-puncture, coagulating injections, the manipulation of Fergusson, distal ligature in one of its new applications, or even to some form of compression—which Syme, through what Holmes calls a "strange prejudice," so singularly undervalued—nevertheless, the terrible mortality of the Hunterian operation in subclavian, axillary, and iliac aneurisms demonstrates clearly that the Edinburgh surgeon was right in raising the question of its applicability to these cases. "The Hunterian operation on the inominata," says Holmes, "and upon the first part of the subclavian, gives twenty-eight cases, in only one of which did the patient survive, and then after repeated attacks of hemorrhage—a ghastly record of death and blood."[2] The ligature of the second and third parts of the subclavian for aneurism is also very unpromising; Mr. Poland gives us twenty-one cases, of which only nine recovered.[3] In consequence of the pushing up of the clavicle by the growing tumor, ligature of the subclavian in its third portion for aneurism is often difficult, sometimes impossible, and always dangerous. Dupuytren abandoned it on one occasion, after persevering for an hour and a half, asserting that it was the most difficult operation he had ever undertaken; and Liston punctured the aneurismal sac in carrying his ligature around the vessel, and lost his patient.[4] On the other hand, in two cases of rapidly advancing axillary aneurism, Syme limited himself to dividing the tissues at the root of the neck so as to permit a finger to be introduced to compress the artery against the first rib, and then he laid open the tumor in the axilla and tied the vessel as it entered and again as it left the sac, and in both instances saved his patient. In two cases of axillary aneurism of a still more desperate character, he succeeded in saving the patients by amputation at the shoulder-joint after the ligature had failed; and Mr. Spence adopted the same mode of procedure, at least with temporary advantage, in a case of subclavian aneurism.[5]

Now, granting that Syme has demonstrated that laying open an aneurism is a safer and more certain mode of cure than the Hunterian ligature in these localites and others similarly situated, where pressure can be momentarily effected between the tumor and the heart, does not this success also prove that it is feasible to attempt a cure, perhaps at an earlier period, by galvano-puncture, or coagulating injections?—for it is in cases in which the circulation can be thus suspended that these novel and ingenious remedies offer the best prospect of successful application.

[1] On the Treatment of Axillary Aneurism, Med.-Chir. Trans., vol. xliii. p. 142. See also another paper in vol. xlv. of the same Transactions.

[2] This includes thirteen cases of ligation of the innominata, and fifteen of the subclavian. Holmes, ut supra, Lect. iv., part ii., 1st series, 1872.

[3] Statistics of Subclavian Aneurism, by Alfred Poland, Guy's Hospital Reports, vols. xv., xvi., xvii.

[4] Surgeon Otis, of the U. S. Army (in Medical and Surgical History of the Rebellion), gives 76 per cent. of deaths for ligature of the subclavian outside of the scaleni, mainly for hemorrhage or traumatic aneurisms after gunshot wounds, however. In my table of eighty-six cases of the same operation for all causes, the deaths were nearly 40 per cent. Contributions to Practical Surgery, Phila. 1865, p. 194.

[5] See Mr. Poland's paper in Med.-Chir. Trans., vol. lii. p. 277.

In thirty-two cases of ligature of the primitive iliac, tabulated by Dr. Stephen Smith, of New York, the operation was done for aneurism in fifteen, in which ten died, and one only, Mott's patient,[1] was permanently cured.[2]

Whatever value, therefore, may be ultimately assigned to the operation of Hunter elsewhere in the arterial system, it seems clear that, in these localities at least, it has proved neither safe nor reliable, and cannot be so regarded in future.

For gluteal aneurism, the Hunterian operation is equally unpromising. Holmes, who studied this subject thoroughly two years ago, succeeded in collecting but twelve cases in which the disease was considered spontaneous, and for which the internal or common iliac had been ligated; of these five, or nearly half, died.[3] On the other hand, there were twenty-one cases in which the affection was pretty certainly traumatic; in two of these the Hunterian operation was done—one of them the case of Dr. Bigelow, of Boston—and both ended fatally. To these I can add an unpublished case I witnessed in the Bellevue Hospital, New York, some years ago, in which ligature of the internal iliac was undertaken, but the patient died within a few days, as was supposed, from wound of the iliac vein. There are several other cases on record in which the same cause of death is noted, showing the difficulties which attend the ligature of this vessel.

In summing up his conclusions concerning gluteal aneurism, Holmes hardly shows his usual surgical acumen. He says: "The old operation by laying open the sac and tying the vessel, is a desperate business, and in spite of the really considerable amount of success which has attended it, no prudent surgeon could contemplate it without repugnance." Now, if the success of the old operation in gluteal aneurism has been "really considerable," as could be readily shown if time permitted, is not this conclusion rather sentimental than surgical? The affection is undoubtedly, in the great majority of cases, traumatic in its origin, and the treatment demanded is that of a wounded artery, whenever practicable. Syme succeeded by laying open the sac, following the lead of John Bell in his famous case of the leech catcher. Mr. Bickersteth, of Liverpool, and others, have been also successful in a similar mode of procedure. The source of fatal delay in quite a number of cases has been the fear of uncontrollable hemorrhage, and lack of power to command it. This risk, in the hands of a less confident and skilful operator than Syme, is a great drawback, and it is certainly very desirable to find some safe mode of commanding the circulation in this locality. Lister's aortic clamp, as used by Syme in the case in which he successfully attacked an iliac aneurism by the old operation, is not free from danger of crushing the intestines and causing congestion of the kidneys, as proved by Mr. Bryant's fatal case at St. Bartholomew's.[4]

I have a suggestion to offer which seems to me feasible and effective,

[1] Phila. Journ. Med. and Phys. Sciences, vol. xiv. p. 176.
[2] Amer. Journ. Med. Sciences, July, 1860.
[3] Lectures, ut supra, June, 1874.
[4] This was an attempt to cure an aneurism of the aorta very near the diaphragm by means of distal pressure. Holmes's Lectures, ut supra.

Mr. Geo. Pollock, in trying the rapid pressure treatment with Lister's compressor upon the aorta with chloroform for two hours and ten minutes, and again with ether for an hour and a quarter, was obliged to desist on account of faintness on each occasion. The attempt was also followed by hæmaturia which lasted for several days. He did not succeed. Trans. Clin. Soc. London, vol. vii. 1874.

and which I shall put in practice if I encounter one of these rare cases. In explorations of the rectum, after stretching the sphincter ani, where several fingers or the whole hand, after the suggestion of Simon, of Heidelberg, have been introduced into the bowel, I have been struck with the facility with which the great arteries of the pelvic cavity could be felt by the fingers, and compressed against its bony walls; and I am confident that either the external or internal iliac or their common trunk could be thus easily commanded long enough to render it a safe operation to lay open a gluteal aneurism and secure the artery involved, if external to the pelvis. All that is necessary is a reliable assistant with a small hand, who is somewhat familiar with the manœuvre.[1]

If, possibly, a gluteal aneurism should take its origin within the pelvis, emerging through the sciatic opening, this fact might be also ascertained by rectal examination, as already advised by Holmes; and at the same time, carrying the exploration a little further, the feasibility of commanding the circulation through the tumor by digital pressure from the rectum could be determined. In this latter event, to attempt a cure by *distal* ligature would involve less risk to life than the operation of tying the internal or primitive iliac.

I would therefore venture the conclusion, in view of the uncertainty of diagnosis of gluteal aneurism and its generally slow progress, together with the great probability of its traumatic origin and the ascertained difficulty and risk to life of the Hunterian operation, that its employment, in this variety of aneurism, is of doubtful propriety.

Ligature of the external iliac for inguinal and femoral aneurism, in ninety-seven cases collected by Norris, gave twenty-six deaths, or a mortality of about twenty-seven per cent., a comparatively good result, and equal to that attained by compression; so that judging from mortality alone, there is little choice in these localities between the two methods.

According to Holmes's table of cases, the same general conclusion will apply to popliteal aneurism.

Owing to the number and size of the branches given off by the common femoral artery, relapse of aneurism has occurred quite frequently at the groin after obliteration of the external iliac artery, whether by ligature or compression, and this is an argument in favor of the old operation in this locality. It is certainly to be preferred in cases of relapse, and after the failure of flexion, to ligature of the primitive iliac, as Mr. Annandale, of Edinburgh, decided in a case which he has treated successfully within the year by laying open the sac.[2]

The *distal* application of the ligature for the cure of aneurism, as proposed originally by Brasdor, and executed successfully half a century later by Wardrop, after having been for a long period regarded with distrust, is again finding favor. In the well known case of Lambert, it produced a cure of an aneurism confined to the root of the primitive carotid.[3] At the suggestion of Dr. Cockle, of London, Mr. Christopher Heath, in a case of aneurism of the arch of the aorta involving the origin of the left carotid, recently ligated the latter vessel, and the operation

[1] I have since learned that this idea had already been conceived and placed on record by Dr. Frank Woodbury, Resident Physician at the Pennsylvania Hospital. Am. Journ. Med. Sciences, Jan. 1874, p. 131.

[2] Double femoral aneurism treated with success by rapid compression; return of one aneurism; ligature of external iliac, unsuccessful; cure by laying open the sac. Lancet, April 22, 1876, p. 297.

[3] Wardrop, on Aneurism, London, 1825, p. 36.

was followed by shrinking of the aneurismal tumor and decided benefit in relief of symptoms.[1] Mr. Annandale, of Edinburgh, tied the right carotid in a case of aortic aneurism, in March, 1875, and, as I recently learned from him, his patient is still living, and the disease is apparently arrested.[2]

After having failed in four cases of subclavian aneurism, the *distal* ligature has at last been crowned with success in the hands of Prof. Toland, of San Francisco, California. In January, 1874, a miner presented himself with an aneurism as large as his fist, extending from the inner border of the left sterno-mastoid to the anterior border of the trapezius. Two ligatures were applied to the third portion of the subclavian; they came away on the twentieth day. Pulsation in the tumor was perceptibly lessened immediately after the ligature, and gradually decreased up to the sixth week from the date of the operation, after which no pulsation or bruit could be discovered.[3]

Of innominate aneurism there are instances enough on record in which benefit has followed the obliteration of one or both of the branches of this great trunk to warrant Holmes's conclusion that "there are some cases of this disease in which the distal operation is not only allowable, but imperative." Although there are only three cures reputed certain (the cases of Fearn, Evans, and Morrison), of the forty-three he has collected of distal ligature for supposed innominate aneurism, yet the favorable consequences of the operation recognized in studying the individual cases leave a far more favorable impression on the mind than this bare statement would convey. In several of the unsuccessful cases, for example, decided relief to dyspnœa, and to other symptoms, followed ligature of the carotid.

In fact, in a case in which operative interference is decided on, our present experience points to ligature of the right carotid in the first place, as the most judicious course; and to the subsequent consideration of the question of tying the subclavian, and at what part of its course.

Any tendency to spontaneous obstruction of either branch of the innominata should be most carefully sought for, as this "would be a first and grand step towards a cure of the disease."[4] Since Holmes wrote, the double simultaneous ligature, which has never yet succeeded, has been done on a Hottentot in South Africa by Mr. Frederic Ensor; the patient survived more than two months, and died from bursting of the sac.[5] And in another case by my colleague Dr. R. F. Weir, of New York, whose patient did not survive so long.[6]

[1] Dr. Cockle, "Med. Soc. Proceedings," vol. i. p. 5, and Heath, "On the Treatment of Intrathoracic Aneurism by Distal Ligature."

[2] British Med. Journ., Oct. 30, 1875. The patient was a man of sixty-two.

[3] Western Lancet, July, 1874. Abstract in Amer. Journ. Med. Sci., Oct. 1874.

[4] Surgery of the Arteries. Lettsomian Lectures of the Medical Society of London, 1875, by C. F. Maunder, Surgeon to the London Hospital, p. 25. He refers in this connection to the striking case of Dr. Herbert Davies, in the London Hospital Reports, 1864, vol. i. p. 50, in which the subclavian and its branches were already occluded, so that ligature on the carotid would have completed the cure.

[5] Lancet, Feb. 6, 1875.

[6] In a most interesting case, published within a few days, of an aneurism of the aorta projecting at the root of the neck on the right side and simulating innominate aneurism, Dr. S. Fleet Spejr, of the Brooklyn City Hospital, Brooklyn, New York, obliterated the right carotid by his invaginating "constrictor," and forty-eight hours later applied a silk ligature upon the third portion of the right subclavian. Immediate shrinking of the aneurismal tumor and relief to symptoms followed each of the operations; but inflammation of the sac, taking its origin at the subclavian ligature, with subsequent hemorrhage from this

If the promise offered by the carbolized catgut ligature of Lister, or by the methods of Howard or Speir, be fulfilled, we shall be justified in applying these to the subclavian in the first part of its course. When this can be accomplished successfully, the distal treatment of aneurism of the innominata and also of the aortic arch—in certain cases—will be established as a more hopeful resource to which the sufferer from these formidable affections will be entitled, hereafter, when milder means have failed.

It will be more profitable, perhaps, in connection with the distal treatment of subclavian aneurism, to refer to the recent case in which Prof. Warren Stone, of New Orleans, succeeded in curing a traumatic aneurism involving the second division of that artery by distal pressure applied by the fingers upon its third portion where it crosses the first rib. Its interest will justify a glance at the details of this unique case, for it not only illustrates further the value of the distal treatment of aneurism by ligature, but also that of compression. A healthy man of twenty-five was wounded, in April, 1874, by a pistol ball which entered immediately above the left clavicle, near its inner extremity, and passed downwards and backwards, lodging under the skin at the posterior margin of the scapula. There was great pain, and partial paralysis of the arms, showing lesion of the brachial plexus of nerves, but only a trifling amount of hemorrhage. At the end of six weeks an aneurismal tumor the size of an egg had formed at the cicatrix of the wound, and was steadily increasing. It was attributed to a bruise of the artery at the summit of its convexity. Digital compression by a relay of competent assistants was begun on the 15th of June, 1874, and continued for thirty-nine hours, when the skin began to suffer, and the patient was exhausted. The tumor was reduced to one half its original size, was much harder, and feebler in its pulsations. It continued to diminish and to grow harder from month to month. About the middle of March, 1875, all pulsation had disappeared, and up to the present date it has not returned. The tumor is now as hard as a marble, and quite as small.[1]

This is the only recorded case I have been able to find in which a subclavian aneurism has been cured by distal pressure.

Since the Dublin surgeons re-introduced compression as a remedy for aneurism, and established its value on a permanent basis, it has steadily

point, produced a fatal result in the fifth week. At the post-mortem examination the aneurismal tumor, which sprang from the aortic arch by a narrow opening, was found filled entirely by a "solid mass of coagulated fibrin." The "constrictor" had obliterated the carotid successfully, and the wound had closed perfectly by quick union. See Archives of Clinical Surgery, New York, Sept. 1876, p. 96.

[1] A case of traumatic aneurism of the left subclavian artery treated successfully by distal compression, by Warren Stone, M.D., Professor of Surgical Anatomy, Charity Hospital Med. Coll., New Orleans, from the New Orleans Medical and Surgical Journal, July, 1875. In a private note, under date of May, 1876, Dr. Stone kindly adds the following details to the published case: "I examined the patient a few weeks ago in company with my friend Dr. Choppin. We found a mere remnant of the tumor, with not a particle of pulsation in it. But there was evidence that blood had found its way through the main trunk of the artery (probably by way of the subscapularis), for, by pressing it against the first rib, radial pulsation was arrested, as on the other side of the body." Dr. Stone also adds this interesting fact, since published by Dr. S.: "In Dr. Smyth's successful case of ligature of the innominata—which was also traumatic in its origin—notwithstanding all direct current was cut off, as well as the distal flow through the carotid and vertebral, the sac eventually filled again (taking ten years it is true), suppurated, bled, and caused the patient's death. Post-mortem examination showed that it was principally supplied by the subscapular branch of the axillary."

advanced in the favor of the profession, and has attained new methods and a wider range of application. At first the process was slow, intermittent, painful, accomplished only by mechanical contrivances, and uncertain in its results. Now, thanks to our countryman, Knight, of New Haven, and to the Italian surgeon, Vanzetti, who have taught us the advantage of accurate compression by the fingers, and thanks also to the American discovery of anæsthetics, we are often able to cure aneurism by compression in a few hours. The method has been extended from the femoral, to which it was at first confined, to the largest arteries; where the ligature has failed, it has succeeded; an aneurism of the abdominal aorta has been cured in five hours by compression applied to that vessel; another in ten and a half hours; and an iliac aneurism in five hours of aortic compression—all under anæsthetic influence.[1] The fatal case of Mr. Bryant, of St. Bartholomew's, of rapid aortic compression, in which the patient died of peritonitis,[2] and a similar case of Mr. Maunder, at the London Hospital, in which death occurred suddenly at the fourth hour, apparently from the effects of chloroform,[3] warn us as to the direction in which danger lies. Nevertheless, Mr. Holt has cured an abdominal aneurism at the Westminster Hospital, London, after fifty-two hours' pressure under chloroform.[4]

At first employed only indirectly upon the artery between the aneurism and the heart, compression has also been applied with advantage directly to the aneurismal tumor, as in a successful case of subclavian aneurism, recorded by Mr. Poland,[5] and another of femoral aneurism, cured by Dr. Buckminster Brown, of Boston.[6] In a solitary instance a subclavian aneurism has been cured by digital compression applied to the artery on the *distal* aspect of the tumor—the case of Prof. Stone, already cited; and, in not a few instances, both indirect and direct pressure have been used conjointly in the same case.

Perhaps the most remarkable cure of subclavian aneurism by pressure, is the case published last year by Dr. Tiffin Sinks, of Leavenworth, Kansas. A pistol ball at close range perforated and comminuted the clavicle, and, according to the best judgment of the surgeon, entirely divided the subclavian artery at the commencement of its third portion. The profuse discharge of arterial blood which the surgeon, who was present, saw welling up in a column of three-eighths of an inch in diameter to the perpendicular height of an inch, caused profound and death-like syncope; but the patient, a robust man of forty-six, unexpectedly rallied, and had no return of bleeding. The cylindrical opening through the bone, with its projecting spiculæ, seemed to have fixed the clot which formed during the syncope so firmly that it acted like a stopper, and the result was the formation of a circumscribed aneurism at the seat of injury. Although the pleural cavity slowly filled afterwards in consequence of internal leakage, showing that it also had been opened by the bullet, this ceased after a few days, and the pleural effusion was slowly

1 An account of a case of aneurism of the abdominal aorta, which was cured by compression of that artery immediately above the tumor; by Wm. Murray, M.D., etc., Phys. to the Dispensary, Newcastle-on-Tyne, and Lecturer on Physiology, etc., Med. Chir. Trans., vol. xlvii. Mr. Moxon cured a case in ten and a half hours, using Lister's aortic compressor under chloroform. Med. Chir. Trans., vol. lv., 1872. Mr. Claudius G. Wheelhouse cured an aneurism of the external iliac by Lister's instrument applied to the aorta, in five hours, under ether. Trans. Clin. Soc., vol. vii., 1874.

2 Holmes's Lectures, ut supra.

3 Surgery of the Arteries, ut supra, p. 11.

4 Trans. Clin. Soc., vol. vii. p. 56.

5 Med. Chir. Trans., vol. lii. p. 277.

6 Boston Med. and Surg. Journ., Oct. 21, 1875.

absorbed. The compound fracture went on to consolidate, and, strangely enough, the external wound closed entirely without any suppuration, which is ascribed to the fact that no compresses or plugs were at any time applied to it. Eventually a time came when diminution of the general swelling allowed pressure to be applied to the artery just at the outer border of the scalenus, by which the pulsation of the aneurism could be controlled. By the judicious use, at first of intermittent and later of permanent pressure at this point, conjoined with temporary pressure by a mass of lead modelled from a plaster cast of the parts, applied to the tumor itself, all swelling and bruit finally disappeared, so that at the end of three months the patient was able to take a trip to New York on business.[1]

In the form of *acupressure*—by needles, or by metallic wire, what Dr. Pirrie, of Aberdeen, styles "metallic pressure, removable at pleasure"—this remedy of systematic pressure has been subjected to various modifications for the purpose of curing aneurism. In one form it has been applied successfully as a wire compress to the carotid, and again, to the femoral, by Mr. Dix, of Hull, England.[2] Mr. George H. Porter, of Dublin, laid bare the innominata, and compressed it by an instrument of wire resembling a miniature lithotrite, hoping to obliterate the calibre of the artery without dividing its coats; but they ulcerated under the chafing and pressure, and the patient bled to death. I have seen the preparation from this case, and also the instrument. With the latter in a modified form, Mr. Robert Perssé White, of the Meath Hospital, has since cured an inguinal aneurism by compressing the external iliac.[3] Mr. W. Stokes, of the Richmond Hospital, has also applied Mr. Porter's instrument, as perfected by himself, to the abdominal aorta so effectually as to stop accurately the current of blood through that great vessel without injury to its coats, but the patient perished from shock and incipient peritonitis.[4] Mr. Bickersteth, of Liverpool, has also failed in an attempt to obliterate the innominata by means of an apparatus of lead wire and caoutchouc.[5] The so-styled "constricting ligature" of our countryman, Dr. B. Howard, in which a loop of silver wire is applied around an artery so loosely as not to cut through its coats, may also be regarded as a variety of "metallic compression." It is not removable at will, for the wound is closed over it as for quick union, with the belief—founded on experiment—that the little loop will be tolerated by the tissues, and become encysted, and that the inclosed vessel, which is not so tightly constricted as to cause ulceration of its coats, will nevertheless become permanently obliterated.[6]

[1] Medical Herald, Leavenworth, Kansas, Aug. 1875, p. 15.

[2] Two cases of aneurism, one of the carotid, and one of the femoral artery, treated by the wire compress, by J. Dix, M.R.C.S., etc. etc., in Brit. Med. Journ., August 28, 1875. Mr. Dix cuts down upon the artery, and introduces a strand of flexible iron wire beneath it in the usual way by the aneurism needles. Each end of the wire is then threaded to an ordinary needle, and brought out through the tissues by the side of, but clear of, the wound, which is then closed entirely as for healing by first intention. A piece of cork is then placed between the two points of emergence of the wire, and pressed down firmly upon the artery, and over this cork the wire is tightly twisted until the circulation is stopped. At the end of six or seven days the wire is withdrawn.

[3] Dublin Medical Press, November 24, 1875, p. 428. In this case, the artery is stated to have been compressed one hundred and sixty-two hours, and yet there was no ulceration nor hemorrhage.

[4] Dublin Quarterly Journ. Med. Sci., August, 1869.

[5] Holmes's Lectures, ut supra.

[6] An Essay on the treatment of Aneurism by a new method, with experiments upon the closure of arteries. By Benj. Howard, M.D., late Prof. of Clinical Surgery, etc., from Transactions of Amer. Med. Association, 1870.

Dr. S. Fleet Speir, of New York, has also devised an ingenious artery compressor which can be applied upon the continuity of an artery, so as to divide the middle and inner coats, and then to invaginate the external coat of the artery within the tube of the constricting instrument to a sufficient extent to roll up the two inner arterial tunics into an impassable barrier equally efficient with that produced by the method of torsion. After this, the instrument is withdrawn, and the wound closed. This ingenious device has been successfully applied to the carotid three times, to the femoral and the brachial, each, once; in each instance it has caused obliteration of the artery, and, as Dr. Speir informs me, has in four cases caused a cure of aneurism.[1]

On the whole, the results of compression by metallic wire have not been very satisfactory in any of its modes of application, so that, at the time he wrote, Mr. Holmes felt justified in asserting that, for his part, whether Mr. Lister be right or no in saying that, with the carbolized catgut ligature properly applied, the ligature of the innominata will be found a safe proceeding, he, Holmes, would much prefer tying that vessel on this method to making any attempt at acupressure.[2] But, although he alludes to a case in which he used a wire ligature successfully, Mr. Holmes does not mention B. Howard's method.[3] In my judgment, we cannot afford to ignore this method of employing silver wire, for, in case the carbolized catgut ligature should in the end prove unreliable, it seems to me that Howard's suggestion, and the invaginating constrictor of Speir, hold out the best remaining promise of occluding an artery without dividing its coats.

But the most interesting case of metallic compression has occurred since the date of Holmes's lecture: it is that in which Mr. Arthur Ferguson McGill, of Leeds, succeeded in consolidating an aneurism of the left subclavian artery by means of a pair of ordinary artery forceps applied to its first division for ten hours. Unfortunately, in consequence of the anomalous position of the vessel, the pleura was wounded, and through this accident he lost his patient.[4] In this case the efficacy of

[1] See Arch. Clin. Surg., ut supra, pp. 96, 106, and 115; also New York Med. Journ., vol. xv., 1872, p. 175, "Successful application of Dr. Speir's artery-constrictor," by Dr. Chas. A. Hart; New York Med. Record, March 1, 1873, p. 102, and March 15, 1873, "The artery-constrictor, with cases, by Dr. Speir."

[2] Holmes's Lectures, etc., ut supra.

[3] Holmes says of this case, in which he tied the femoral artery for popliteal aneurism with silver wire, that it was "thoroughly satisfactory," the wound healing, though not without suppuration, in less than a fortnight. Nothing was seen of the ligature; and he believes that it "buried itself in the tissues of the vessel, and in all probability without having divided the continuity of the latter;" Lect. iii., Part i., 1st series, June, 1872. He also speaks of an experiment upon an ass, in which the carotid was obliterated and not divided, and adds a caution about not tying the wire too tightly, lest it cause ulceration.

[4] A case of left subclavian aneurism, treated by temporary compression applied directly to the artery in the first part of its course, with remarks; by Arthur Fergusson McGill, F.R.C.S., surgeon to the Leeds Public Dispensary, Med.-Chir. Trans., vol. lviii. p. 338, London, 1876. A woman of 35, with a left subclavian aneurism, had already been very decidedly benefited by four applications of galvanic needles, but she had resumed a laborious occupation and the tumor began to grow again, and the pain of the shoulder and arm which had been relieved by the galvano-puncture returned. The tumor had extended above the clavicle into the neck, and was approaching the surface. She was too poor to spare the time to try Tufnell's medical treatment, and refused amputation of the shoulder, which had been proposed in consequence of its partial success in Mr. Spence's case. Mr. McGill had no confidence in the distal ligature without amputation, and makes no mention of distal compression. He, therefore, proceeded to cut down upon the artery in the usual way, failed to find it in its usual place on following down the inner border of the scalenus anticus, but discovered it after a tedious search at the depth of his finger, "apparently through a layer

rapid pressure by a metallic substance applied directly upon an artery is indisputable, for the subclavian, in its first portion, was certainly obliterated without division of its coats, and, therefore, without danger of subsequent hemorrhage; and it may be fairly assumed that the aneurism would have been permanently cured, but for the fatal wound of the pleura.

This brings us to consider the propriety of attempting the ligature of the left subclavian in the first part of its course. Holmes briefly passes it over as an operation not likely to be again undertaken on account of its great difficulty, the danger of wounding the pleura, and, perhaps, because Sir Astley Cooper once relinquished an attempt to tie this artery through fear of wounding the thoracic duct. But Rodgers accomplished the ligature of the artery at the New York Hospital safely, without wounding either thoracic duct or pleura. Unfortunately, his ligature of silk cut through the walls of the artery, and his patient died in the third week from secondary hemorrhage. McGill, hoping to obliterate the artery by employing metallic compression in place of the silken thread, and thus to surmount this danger, felt justified in repeating the operation; and he succeeded in his object of safely obliterating the artery and avoiding the danger of hemorrhage; but, less fortunate than Rodgers, the anomalous position of the vessel led to the perforation of the pleura, and this accident caused the death of his patient.

Now, is the danger of wounding the pleura in this operation so unsurmountable as to forbid its repetition? This question must be answered, for if, in the future, it becomes certain that we may trust the carbolized catgut ligature (or even Howard's constricting loop of silver wire) to safely obliterate an artery without dividing its coats, then the risk incurred in laying bare the left subclavian would be the only remaining obstacle to its successful ligature. I feel confident that others will undertake this operation, under the lead of Rodgers, as McGill has done; and as neither of these excellent surgeons had any trouble from the thoracic duct, we have only the danger of wounding the pleura to provide against, and the other intrinsic difficulties of the operation.

My late valued friend, Dr. Charles E. Isaacs, after Dr. Rodgers's operation in 1845, "in order to improve our knowledge as to the relations of the pleura to the parts at the root of the neck," examined in a series of years no less than one hundred dead bodies most carefully in reference to this point of anatomy. He found that the pleura rises higher above the clavicle than is usually recognized by anatomists, averaging in his 100 cases nearly an inch and a half at its highest point, whilst in 23 out of the 100 it rose to two inches and upwards; and that both subclavians within the scaleni muscles are in contact throughout their whole length with the pleuræ. The reason why the pleura has not been more fre-

of pleura" which, in passing the needle around the vessel, was perforated. He then effected compression of the artery by means of a pair of ordinary torsion forceps, stopping effectually all pulsation in the tumor, and closed the wound. Three hours later the pulse was 130; respiration 44; and the air was rushing in and out through the wound as she breathed; but the artery was effectually compressed, and when, ten hours after its application, the compressing forceps was withdrawn in consequence of the patient's feeble condition, so that the wound could be entirely closed, the tumor was solid and pulseless. She died on the sixth day from intra-thoracic inflammation, just as death occurs from a perforating chest wound. The pleura contained a pint of bloody serum. The aneurism was filled with a hard firm clot; in fact it was cured. "The artery had been compressed just before the origin of the vertebral; it was patent, and in a perfectly healthy condition." (p. 341.)

quently wounded in tying the right subclavian in its first division is, he thinks, because the vessel is nearer the surface, and can usually be seen.[1]

Now, the surgeon, as he approaches the first division of the left subclavian from above and in front, as it rises almost perpendicularly from the aorta to arch over the dome of the pleura, unless he fully recognizes the facts brought out by Isaacs, and systematically pushes the serous membrane out of his way, just as we are compelled to raise the peritoneum in approaching the primitive iliac, must incur great risk of perforating it; but an accurate anatomist, keeping these relations of the pleura always in view, may, as Rodgers did, accomplish the operation safely.

In cases where an aneurism is seated near a joint, so that its pulsation can be arrested by maintaining the limb in a flexed position, this simple device continues to be successfully employed, as another variety of compression, under the designation of "the method by flexion," or "position." It has been used most frequently at the knee-joint for popliteal aneurism, but Dr. Gurdon Buck, at the New York Hospital, extended its application to a case of inguinal aneurism which he cured by flexing the thigh upon the pelvis. This was a case of relapse, such as is liable to occur in this region, the aneurism having returned sixteen months after a cure by compression. Pressure upon the external iliac did not arrest the returned pulsation; the vessel in fact had been obliterated. Its ligature, therefore, was out of the question, and the old operation seemed to be the only alternative, as in the similar case of Mr. Annandale already mentioned. Buck had observed, however, that extreme flexion of the thigh arrested the pulsation in the tumor, and he decided to employ this method. In about a fortnight solidification was nearly complete, and an entire cure shortly followed.[2] In a case of femoral aneurism treated first by compression, without result, and finally by ligature of the external iliac, Dr. Gay, amongst other devices to arrest the circulation, tried flexion of the thigh upon the abdomen, which controlled it completely; but it was abandoned in six hours on account of the pain. The parts had become tender under the previous digital and instrumental

[1] Isaacs, "on the extent of the pleura above the clavicle," in Transactions of the New York Academy of Medicine, vol. ii., part i., New York, 1857. Isaacs ascertained that in 100 subjects the pleura averaged 1.42 inches above the upper margin of the clavicle on the right side, and 1.23 on the left. It averaged higher in the male than in the female—1.53 to 1.40. In 23 cases it rose to two inches and upwards above the clavicle, and there were only 5 out of the 100 in which the pleura did not project above its superior margin. The sternal or internal aspect of the pleura of the right side extended across, even to the left of the median line, in 11 cases, from half an inch to an inch and a quarter. A similar disposition of the parts was never observed on the left side. The pleura rises higher in long-necked people; in bull-necked subjects the reverse is the case. In the 23 cases in which the pleura rose two inches above the clavicle, 14 were on the right, 5 on the left, and 4 only on both sides. In these cases the subclavian was high in the neck, and in many of them this artery in the third part of its course rested upon the pleura for a very considerable extent. He asks if the symptoms of wound of pleura and collapse of lung in operations at the root of the neck have not been falsely attributed, in some cases, to entrance of air into the veins? Erichsen states that thoracic inflammation was the cause of death in 9 out of 22 cases of extra-scalene ligature of the subsclavian arteries, and that it was not the pneumonia which so often follows great operations, but inflammation attacking the pleura and pericardium in preference to the substance of the lungs, looking as though the complication arose from causes essentially connected with the disease or operation. Isaacs thinks that one of these causes is probably the unrecognized proximity of the pleura, which he shows exists in one case out of every four. He also speaks of "the curious fact that the top of the pleural sac is not always dome-shaped, but sometimes forms cul-de-sacs extending upwards, and sometimes laterally into the recesses at the root of the neck." (p. 18.)

[2] Amer. Journ. Med. Sciences, January, 1870, p. 69.

pressure.[1] In the case of a Japanese of 40, who was too fat for compression of the external iliac, and unwilling to submit to ligature, Dr. Stuart Eldridge, of Yokohama, succeeded in curing an inguinal aneurism by this method. He devised an apparatus consisting of a jacket and thigh-sheath connected by copper wire, in which the thigh was kept flexed on the pelvis for twenty days, at the end of which time pulsation had permanently ceased. The patient was seen six months later, and there was no return of the disease.[2] Dr. Edward T. Caswell, of Providence, R. I., reports a case of double popliteal aneurism in which genuflexion short of completely arresting the circulation cured both tumors. At the end of six weeks, during which time the man had resumed work, one of the tumors suddenly returned (evidently by rupture), and, after failing with ordinary compression, it was necessary to tie the femoral. There was subsequent suppuration, and discharge of clots.[3] In April of the same year Dr. Montgomery cured a traumatic aneurism at the bend of the arm by simple forced flexion of the forearm upon the arm for twenty-four hours.[4]

From these facts we may infer that this method is still in favor, and certainly gaining in extent of application, for heretofore it has been confined to aneurisms at the knee and elbow; and also that, when tried, it should precede compression by ordinary means, which are liable to leave the parts tender. It is well to remember, also, that Theodore Maunoir, of Geneva, who reported the first case of cure by the method of flexion, in 1858, demonstrated that the position need not be forced or painful, but that, as in ordinary compression, a very moderate obstruction to the circulation may effect a cure.[5]

To conclude the subject of pressure, it is evident that digital compression is steadily gaining ground and taking the place, wherever it is applicable, of pressure by means of mechanical appliances; and also that the process known as "rapid compression," under anæsthetics and anodynes, is increasing in favor.

Still, we must not forget that the old method by moderate and intermittent pressure has also from time to time effected striking cures, and these in localities—as, for example, upon the primitive carotid—where it cannot be safely replaced by more permanent or rapid pressure. The following cases of orbital aneurism are in point: that of Galezowsky, who cured a traumatic case in an adult female by digital compression applied to the carotid, for an hour or so every day at first, and afterwards at longer intervals, for several months.[6] In Scaramuzza's case of idiopathic orbital aneurism a cure followed digital compression, employed very cautiously on account of aortic disease, for not more than five minutes at a time, and afterwards for eighteen days, twenty or thirty minutes every day in five or six turns—the total period of compression being only seven hours and twenty minutes.[7]

I have noticed amongst hospital surgeons of experience a tendency to allow intelligent patients to manage pressure for themselves, by means of bags of shot, or of sand, or by a mass of lead modelled to the part, and gradually increased in weight. At St. Thomas's Hospital, in London, I saw a man in Mr. McCormac's ward with a subclavio-axillary aneurism, who was successfully controlling its pulsations by a buckskin bag of shot,

[1] Amer. Journ. Med. Sciences, January, 1875. [2] Ibid.
[3] Ibid., October, 1875, p. 444. [4] Ibid., April, 1875.
[5] L'Echo Médicale, Neufchatel, Switzerland, September, 1858.
[6] Gazette des Hôpitaux, 1871, p. 237. [7] Archives Générales, 1858, t. xii. p. 731.

to which he had given a peculiar shape; and Prof. Tilanus, at the principal hospital of Amsterdam, showed me a weight of three kilogrammes, by which, he said, several cases had been treated in this way.

If time permitted, the subject of *orbital aneurism* might occupy us profitably. Mr. Rivington, of the London Hospital, has lately made a very thorough study of this curious and obscure affection in connection with a case which he finally cured by ligature of the carotid.[1] Dr. Morton, of the Pennsylvania Hospital, by his unusually rich experience, has also added to our knowledge of this subject. His last published case illustrates the great difficulty that often attends its diagnosis—already shown in the cases recorded by Bowman,[2] and by Aubrey, of Rennes,[3] in both of which, with unmistakable symptoms of aneurism—such as pulsation, bruit, and protrusion of the eyeball—no arterial lesion whatever was found after death. In Dr. Morton's case, with all the characteristic symptoms of orbital aneurism, except pulsation in the protruded globe, symptoms sufficient to warrant the diagnosis of aneurism of the internal carotid in the cavernous sinus, and ligature of the carotid, no trace of aneurismal disease was found after death on the day following the operation.[4] It is to be noted that in each of these cases ligature of the carotid produced immediate relief of the symptoms.

Mr. Holmes, as usual, discusses this subject ably in his lectures.[5] In view of the great uncertainty of diagnosis, and the possibility also of a spontaneous cure—such as took place in the cases recorded by France,[6] Collard, of Berne,[7] Erichsen,[8] and I may add also a more recently reported case of Mr. Jonathan Hutchinson,[9] together with the admitted dangers of the operation, he urges delay in resorting to ligature of the carotid. At the same time, whilst reasoning against it, Holmes speaks of the operation in this affection, in his Surgery, as "a most successful one;" and it is noteworthy that the delay which he advised in Rivington's case led to more loss of vision, and that the ligature was resorted to, of necessity, in the end.

In a still more recent case of Dr. Greuning, of New York, diagnosticated as aneurism of the internal carotid in the cavernous sinus, the operation was promptly and entirely successful.[10]

Mr. Rivington arrives at the conclusion, derived from his very complete collection of cases, that "ligature of the carotid is, beyond question,

[1] A Case of Pulsating Tumour of the Left Orbit consequent upon a fracture of the base of the skull, cured by ligature of the left common carotid subsequently to injection of perchloride of iron, after digital compression and other means of treatment had failed; with remarks, and an appendix containing a chronological résumé of recorded cases of intra-orbital aneurism. By Walter Rivington, etc. etc., Surgeon to the London Hospital. Read March 3, 1875, published in Med. Chirurg. Trans., vol. lviii. p. 183.

[2] Ophthalmic Hospital Reports, April, 1859.

[3] Gazette des Hôpitaux, 1864.

[4] Amer. Journ. Med. Sci., April, 1876, p. 339.

[5] Op. cit., June, 1873, 3d Lect., 2d series.

[6] Guy's Hospital Reports, 3d Series, vol. i., 1853, p. 58.

[7] Gazette Médicale, 1866, p. 631.

[8] Science and Art of Surgery. Phila., 1873, vol. ii. p. 108.

[9] A pouched aneurism of the internal carotid as large as a bantam's egg was found at the post-mortem examination of a woman of forty, who had died of aneurism of the abdominal aorta. Ten years before, Mr. Hutchinson had seen her with ptosis of the eyelid of this side, dim vision, paralysis of the ocular muscles, except the superior oblique, intra-cranial bruit, throbbing, and bad headaches. He had diagnosticated intra-cranial aneurism, and advised ligature of the carotid, but had been overruled, and spontaneous cure followed. British Med. Journ., April 17, 1875.

[10] Archives of Ophthalmology and Otology, vol. v. No. 1, p. 40.

the means most generally applicable to cases of intra-orbital aneurism dependent on morbid states of the arteries." His cases give forty-six instances of ligature of the carotid in forty-four cases (two being double), and six deaths.[1] With such experience, ligature of the carotid in orbital aneurism will continue to be employed, even though in some degree empirically.

I may record the fact that our countryman, Dr. McGill, of Maryland, was the first to tie both carotids in this disease.[2] Erichsen places his name second on his list of double ligatures of the carotid, for all causes. Dr. Foote, of Cincinnati, also tied both carotids at the same interval of a month, arresting a traumatic orbital aneurism at the second ligature.[3] It is proper to observe, in concluding my remarks under this head, that ligature of the primitive carotid for aneurism *other than orbital*, has proved a very fatal operation, mainly through embolism and interference with the blood-supply of the brain, probably less than half the cases on record having been successful.[4] The evidences, therefore, in favor of compression of this vessel, collected by Mr. Holmes, carry great weight. Of his seven cases, five were cured; and in the two in which compression did not succeed, the ligature also failed.[5]

Galvano-puncture, as a method of treatment for aneurism, can hardly be said to be gaining rapidly in the confidence of the profession, perhaps mainly on account of the intrinsic difficulties which attend the process; but, as has been truly said, it occupies at the present time a more promising position than did compression in the days of Guthrie, who did not hesitate to speak almost contemptuously of the pretensions of the latter. The promise held out by the relief of pain and the arrest of pulsation, which so often follow an electrolytic operation, is not fulfilled with certainty; for after a short interval the pulsation almost always returns. This disappointing result seems to be due to the softness and friability of the resultant clot, which is broken and wasted away by the arterial current. We have learned, however, that this soft clot possesses

[1] Of these 44 cases, 25 were cured—one after subsequent injections (case of Brainard of Chicago; Lancet, 1853, p. 162, vol. ii.), 7 received partial benefit, 5 were unsuccessful, and 6 died. Two of these deaths were in patients over sixty years of age, and two others (cases of Critchett and Pétrequin), in patients who survived several weeks, and died eventually from other causes, are not chargeable to the operation.

[2] In a letter to the late Dr. J. Kearney Rodgers, of New York, from Dr. Joshua J. Cohen, of Baltimore (in the N. Y. Med. and Phys. Journ., Sept. 1825, p. 576), it is stated that in this case "an ugly, confused mass protruded beyond the sockets of both eyes," and was increasing every day. The operations were done at an interval of about a month. Dr. McG. found the effect of tying one carotid to be limited to one tumor, and ventured upon the bold attempt (in which he had but one precedent, and probably was not aware of that) of arresting the progress of the other by tying the remaining carotid. Several months after the last operation the patient was doing well, and the tumors subsiding. At this early date there were no recorded examples of successful ligature of the carotid for tumors about the head except the cases of Travers (1809), and Dalrymple (1812), in both of which the disease was supposed to be "aneurism by anastomosis." Doubtless Dr. McGill regarded his case as of the same nature. Cases of soft cancer protruding from both eyes, and doing well some months after ligature of both carotids, are not of probable occurrence, whilst double aneurism has existed in several instances.

Since the above was written, I have seen a note from Dr. Charles McGill, of Richmond, Va.; dated June 9, 1876, in which he writes as follows: "In 1823 my brother ligated both common carotids of a woman in Williamsport, Md., for vascular tumors of the orbits. The interval between the tying was about four weeks. The operation was successful, and the woman was still living and in good health in 1861, when I left the State of Maryland."

[3] Reported by Dr. E. Williams, in New York Med. Record, April, 1868, p. 75.

[4] Maunder, ut supra, p. 44, and Le Fort, Gazette Hebdomadaire, 1868, Nos. 28, 30, 35.

[5] Op. cit., Lect. ii., second series, June, 1873.

neither noxious nor irritating qualities, so that the operation, which is better done under an anæsthetic, may be repeated indefinitely.

We owe to Dr. John Duncan, of Edinburgh, the knowledge that inflammation of the sac, the principal danger that attends the process, is not to be feared if there is no cauterizing effect produced by the needles, and that this can be certainly avoided if they are effectually insulated by a coating of vulcanite—a device introduced and perfected by Dr. Fraser and himself. "I have never met with the slightest reaction," says Duncan, "when using insulated needles; and in one case I continued the operation for two hours and ten minutes."[1]

This being the case, the eschars and fatal hemorrhages, and inflammation and suppuration of the sac, which complicated some of the earlier cases of galvano-puncture, can be now surely escaped, and we have only the uncertainty of the result to contend against.[2]

The plan of operating which experience seems to point out as the best, is to insert both the positive and negative needles, thoroughly insulated by means of vulcanite, into the sac; to employ a battery of large electromotor force; to prolong the operation so that the sac may be occupied as fully as possible by clot; and, finally, to repeat the application as soon as improvement shall have distinctly ceased.

Perhaps the best idea of the value of the electrolytic treatment can be got from Holmes's abstract of the cases of Ciniselli, of Cremona, who has employed it more largely than any other surgeon. In these, twelve in number, there were two relapses after many months of good health, one of which was permantly cured by repetition of the operation, and three other cases in which a cure had been maintained for still longer periods at the time of the report. Of the eight cases which were not benefited, in only one was death caused by the direct action of the electricity, and no harm seems to have followed its use in the others.[3] Abeille cured a case of subclavian aneurism in a woman, having kept a weight of one kilogramme upon the tumor for ten hours after the application of the needles.[4]

The great point in favor of this method is that it may be applied to thoracic and abdominal aneurisms where other surgical remedies are powerless, in combination, for example, with Tufnell's medical treatment. In thoracic aneurism, both Ciniselli and Duncan have certainly succeeded in averting imminent death from hemorrhage where the tumor was already projecting beyond the ribs.[5] In abdominal aneurism no success has been as yet attained. In Keyes's case the manœuvre was accomplished safely, but without apparent result beyond relief of pain.[6] In Dell'

[1] Lectures on Electrolysis, by John Duncan, M.D., etc., Assistant Surgeon to the Royal Infirmary, Edinburgh, Lecturer on Surgery, etc.; British Medical Journal, June 28, 1876, p. 621.

[2] See the case of Pétrequin (Journal de Chirurgie de Malgaigne, t. iv. p. 246, 1840), and another case in Broca (sur les Anévrysmes, etc., Paris, 1856, p. 342), in which the needles, uninsulated, were applied seven times in one week for an aneurism under the jaw; there was no consolidation, but inflammation and suppuration followed, with secondary hemorrhage, necessitating ligature of the carotid, and free opening of the sac, and other measures to stop the bleeding.

[3] It is proper to note that Ciniselli's mode of operating is probably more liable to irritate and inflame the sac than when insulation by vulcanite is employed.

[4] Archives Générales, t. xx. p. 49, 1849.

[5] Lectures, etc., ut supra.

[6] New York Med. Journ., December, 1871, p. 569. Keyes demonstrated, however, that eleven needles could be passed through several layers of peritoneum and convey the current from thirty-two cells (of a Stohrer's battery) without causing inflammation. His needles were insulated by vulcanite.

Acqua's case the aneurism burst during the struggles of the patient under chloroform, and he died on the table.[1] McGill reported a case of subclavian aneurism as greatly benefited after four operations, manipulation having been also employed conjointly with electrolysis after the last application. But relapse took place, and nine months later the woman was subjected to the operation on the left subclavian already reported.[2]

As to galvano-puncture in external aneurisms, Ciniselli cured a popliteal case in 1846—the second recorded example of success by electrolytic treatment, Pétrequin, of Lyons, having cured a small aneurism of the temporal artery the year before.

Broca, in external aneurisms, recommends direct moderate pressure to be applied immediately after the withdrawal of the needles, with the view of its effect upon the tumor in the way of malaxation, or manipulation, as it was employed, in fact, in Abeille's successful case of subclavian aneurism; and this seems to me an excellent practical suggestion. An Esmarch's bandage for example would really offer the best means of effecting distal pressure (which in the leg and forearm is not easy to accomplish) during the operation, and, if it were afterwards carried upwards over the whole limb including the tumor, it might materially assist in preventing the liability to washing away of the clot.

Dr. Duncan says, in his summary of conclusions, that "in external aneurisms electrolysis should be tried, if compression be not available, or have failed; and it is to be preferred to the ligature—at least of the innominate and iliac arteries."

In his address on Surgery before the British Medical Association, in August, 1875, Prof. Spence, of the University of Edinburgh, stated that his colleague Dr. Duncan had had successful results in treating large cirsoid aneurisms by electrolysis. There is additional evidence on this point in Dr. Duncan's lecture since published.[3] It is also very generally conceded that for most forms of aneurism by anastomosis, and erectile tumors, electrolytic treatment offers, at the present time, greater advantages than any method of treatment in use.

Manipulation, one of the bloodless methods of treating aneurism, with which the name of Sir William Fergusson is principally connected,[4] has not yet achieved credit for the success which it seems to promise and to deserve. According to Holmes, this method has been practised in five cases of subclavian aneurism, and in two of aneurism of the lower extremity, "with clear evidence of cure in two, and a good advance towards cure in another," and no certain proof of injury to the sac, which is the great danger of the proceeding, in any case.[5] This surgeon has proved by carefully studied preparations and cases that

[1] Gazetta Medica Italiana, Lombardia, No. 28, 1870, p. 217.

[2] My colleague Dr. Sands, with the aid of Dr. Guleke, attempted to arrest the progress of a traumatic subclavian aneurism in a young man of nineteen, at the N. Y. Hospital, in March, 1869. He made four electrolytic operations of fifteen minutes each, following Ciniselli's method mainly. The tumor became momentarily firmer, but it continued to enlarge, and finally burst. (Thesis of Dr. Allan McLane Hamilton, New York, 1870.)

[3] British Med. Journ., August, 1875, p. 189. In the same Journal, June 10, 1876, p. 715, Dr. D. says: "There are four recorded cases in which electrolysis has been used in cirsoid aneurism, and in three the disease has been cured. In a case in which Syme declined to operate, six applications of the battery effected a cure."

[4] Med.-Chir. Trans., vol. xl.

[5] Op. cit. Lect. v. Part i., 1st series, 1872. I do not think that Dr. Lidell's successful case is among these.

impaction of clot in the distal end of the artery as it leaves an aneurismal sac is naturally (not always necessarily) followed by obliteration, more or less complete, of the sac. The curative effect of the distal ligature is thus explained, and also that of the method we call manipulation. By the latter proceeding a portion of clot is intentionally and by violence detached from the internal wall of the aneurismal sac, and pushed by the fingers, or carried by the current, into its distal outlet, so as to plug it like an embolus. A large proportion of the cases in which aneurism is said to undergo spontaneous cure are doubtless brought about in this manner, and Sir William Fergusson has wisely recognized Nature's lead in proposing it as a remedy.

I think that many cases reach their final cure in this manner after the interference of art, in which the real *methodus medendi* is not recognized, or is referred to some other cause. Take, for example, the cases collected by Mr. Tufnell, at page 89 *et seq.* of his excellent "Practical Remarks on the Treatment of Aneurism by Compression," published in 1851, in which, after a certain amount of contraction and hardening of the tumor, this treatment seemed powerless to arrest pulsation entirely, and the patients, as the disease had ceased to increase, were sent about their business. In every such instance in which the patient was followed up, a final cure was found to have taken place, and it had always occurred suddenly and after some unusual effort, when a new, painful sensation would be felt in the tumor, and presently all pulsation would be found to have ceased. Here, I take it, a fragment of clot had become detached and plugged the distal outlet of the half-cured aneurism.

This method of cure is only applicable to aneurisms in which a certain amount of clot has already formed, and of which the walls present, as yet, no evidence of thinning or softening, so as to endanger rupture by handling. Dr. Bontecou, of Troy, recently tied the external iliac in a case of inguinal aneurism, which had been ruptured by the manipulation of a professional rubber, undertaken for the relief of pain.[1] A prominent medical man of Dublin caused the rupture of an abdominal aneurism in his own person. He had known of its existence for ten years, and it had undergone partial cure. The object of his manipulation was to convince his attendants that it was a mass of hardened feces, for, when he grasped the tumor and drew it away from the spine, its pulsations were arrested. Shortly after they had retired to consult, he was found dying from internal hemorrhage.[2] These examples teach us the necessity of caution.

Manipulation of an aneurism of the innominata or of the primitive carotid might occasion cerebral embolism, and inflammation of the sac is always a possible contingency.

It has seemed to me that in one, if not both, of the remarkable cases of apparently spontaneous cure of subclavian aneurism recorded by the late J. Mason Warren, of Boston, the violence previously inflicted upon the tumors was the real means of cure, by causing fragmentation and distal impaction of organized coagula.[3] And this is also the most probable explanation of the success of Staff-Surgeon Reid in the interesting case of popliteal aneurism in which, after failure of compression by ordinary methods, he effected a cure in fifty minutes by the application of an Esmarch bandage.[4] His patient subsequently died within the year of visceral disease; and, in describing the condition in which he found the

[1] New York Med. Journ., 1876, p. 281.
[2] Holmes, ut supra.
[3] Surgical Observations, etc., Boston, 1867, p. 424.
[4] Lancet, Sept. 25, 1875.

cured aneurism, Dr. Reid speaks of the manifest displacement of the fibrinated laminæ which it contained, which laminæ were probably due to the previous attempts at its cure by compression.[1]

I have been able to find no very recent cases in which the use of *coagulating injections* has led to satisfactory results, except a case ascribed to Plagge, of Darmstadt, who cured a traumatic aneurism, just below Poupart's ligament, by injection of extract of ergot, alcohol, and glycerine, continued daily for three weeks, with an ice-bag applied to the tumor in the intervals.[2] The use of ergot is ascribed to Langenbeck, and the absence of details detracts from the value of the case. In the case of Dr. Dutoit, of Berne, the cure of an aneurism supposed to be subclavian, in a man of 40, is attributed to fifteen injections of a solution of ergotin into the tissues immediately over and around the tumor, at intervals of two or three days. This caused diminution or shrinking of the tumor, and subsequently indirect digital pressure was employed to confirm the cure.[3]

The danger of embolism, of which so many instances have followed the injection of erectile tumors by the perchloride of iron, has apparently inspired a fear of using this drug in the sac of an aneurism; whilst its known escharotic qualities, and the hardness and irritating nature of the magma of iron and blood-salts with coagulated albumen and blood-corpuscles resulting from its use, have deterred surgeons from employing it, through well-founded apprehension of inflammation in and around the sac.

In the case of Lenoir, after a third injection which, like the previous ones, was apparently ineffectual, solidification of the tumor took place unexpectedly, and inflammation forthwith developed itself in the sac, and resulted fatally.[4] Here coagulating injections failed, but inflammation caused consolidation of the aneurism, and unhappily also the death of the patient.

In cases where coagulation by the perchloride has succeeded, the resultant mass takes on an almost calculous hardness, fails to be absorbed, and either becomes permanently encysted, or excites suppuration to secure its expulsion as a foreign body.

The question is pertinent whether the tissues would tolerate any better a clot produced by the *persulphate* of iron.[5] This substance, which is not escharotic, has been used by the late Dr. Isaacs, of New York, and also by Drs. Minor, Hutchison, and Enos, of Brooklyn, with success in injecting varicose veins.[6] In July of the present year, Dr. J. C. Hutchi-

[1] Lancet, Aug. 5, 1876. Dr. Reid says in substance that the portion of the cavity of the aneurism apposite its mouth was occupied by several layers of laminated fibrin. Some of these were partially separated from the others and approximated towards the centre, the interspace thus caused being filled by an amorphous, softer, cheesy coagulum.

[2] Amer. Journ. Med. Sci., April, 1874, from Lond. Med. Record.

[3] Langenbeck's Archiv, Band. xii. No. 3.

[4] Broca, op. cit.

[5] Or subsulphate, as I believe it should be called. I mean the salt of iron which is the basis of Monsel's solution.

[6] I learn from Dr. James M. Minor, and also from Dr. Squibb, of Brooklyn, that in experiments with this salt of iron they have been impressed with its great harmlessness, and even with its healing properties when in contact with the tissues. Dr. Minor has published six cases in which it has been used successfully—one, a pulsating (erectile) tumor, and five cases of varicose veins. See American Medical Times, July 1st, 1860. He employed a Pravaz syringe, and a solution containing four parts of distilled water to one of the salt, which, he thinks now, was too strong, as Dr. Jos. C. Hutchison, of Brooklyn, since informs him that he has used it successfully in a solution of thirty to one, without exciting ulceration in any case. (Private communication of this date.)

son, at the Brooklyn (N. Y.) Hospital, injected a large and increasing aneurism of the abdominal aorta in a sailor of thirty-two, three times, at intervals of two or three days, with thirty minims of a solution containing about half a grain of this salt. Death followed within a fortnight with symptoms of inflammation of the sac (*i. e.*, local tenderness, increased temperature, 103°, frequent pulse), and subsequent rupture. There was much stratified fibrine in the sac, apparently recent. (From notes of case as recorded by house surgeon.)

Brainard, of Chicago, cured an orbital aneurism by injecting a solution of lactate of iron, after ligature of the carotid had failed; but destructive inflammation followed.[1] Bourget used the perchloride successfully in what seems to have been a cirsoid aneurism following injury, in a boy of twelve, after failure by galvano-puncture.[2] Desormeaux had similar success in an adult.[3] Pétrequin lost a case, but apparently from causes unconnected with the injection.

Embolism has not occurred in any of the cases of orbital aneurism treated by coagulating injections.

Broca records his success in the injection of large vascular trunks of cirsoid aneurism of the scalp, where accurate pressure could be made upon the vessel on either side of the portion injected.[4]

Our best hope for the future success of the operation of Pravaz would seem, at present, to depend upon the results of experiments with the persulphate of iron, in solutions of different strength, in cases where accurate pressure can be made on both the cardiac and distal aspects of the aneurismal sac.

Closely allied to the use of coagulating injections as a remedy for aneurism is the idea of inserting foreign material into its cavity in order to provoke, mechanically, the deposit of fibrinous clot. The earlier attempts in this direction, inaugurated by Moore, of Middlesex Hospital,[5] London, who employed fine iron wire, and whose example was followed by Domville and Murray, failed in each instance, because the wire produced inflammation of the sac—a result which was no doubt anticipated. Dr. Levis, of Philadelphia, ingeniously substituted an animal substance for the more rigid and irritating metallic wire, and inserted twenty-four feet nine inches of horsehair through a fine needle canula into a subclavian aneurism, in a man of twenty-seven, at the Pennsylvania Hospital in October, 1873. The tumor became hard, and the pulsation in it and in the radial artery, ceased at once. The man died during the month from internal hemorrhage, when it was discovered that the sac of the aneurism had already ruptured previous to the operation, and that a part at least of the walls of the cavity into which the horsehairs had been inserted was formed of lung tissue. Notwithstanding this untoward condition, their presence had caused the formation of hard clot, and obstructed the flow of blood into the axillary artery, solidifying the portion of the tumor which projected above the clavicle, and causing entire cessation of the radial pulse. There were no evidences of inflammation caused by the foreign body.[6]

The conclusion can hardly be avoided, in this interesting case, that if

[1] Lancet, 1853, vol. ii. p. 162.
[2] Gazette Médicale, 1855, p. 772.
[3] Rivington's paper, ut supra, p. 288.
[4] Bulletin de la Société de Chirurgie de Paris, 1859.
[5] Med. Chir. Trans., vol. xlvii.
[6] Translations Pathological Soc. Phila., vol. v. p. 79, 1876.

there had been a true resisting sac to the aneurism, a cure would have followed.

This operation has been repeated by Mr. Bryant, of St. Bartholomew's Hospital, London, who introduced twenty feet—three hairs at a time—into a rapidly-growing popliteal aneurism, "with the effect of causing almost complete consolidation of the tumor. The man survived the operation five days, dying from ulcerative endocarditis, a condition that existed before the operation." This surgeon considers these results as quite sufficient to justify a repetition of the operation.[1]

If we succeed in getting a preparation of catgut somewhat more permanent than that now in use for ligatures, but still capable of blending ultimately with the tissues, the use of this substance would be still less likely to provoke inflammation or suppuration.

Finally, there is still a variety of aneurism to which experience has taught us that the Hunterian operation is inapplicable; but the lesson is being learned slowly. I refer to the cases in which an abnormal opening has been formed between an artery and vein—what we call arterio-venous aneurism, a term properly used whether any tumor be present or not. This affection is almost invariably the result of a wound, and is characterized by an exaggerated and peculiar thrill[2] often perceptible in the neighboring veins, and also by enlargement in calibre of the arteries involved, and thinning of their walls.

The Hunterian ligature has been wrongly applied to the cure of this affection, as is proved by the occurrence of gangrene or secondary hemorrhage in all the cases in which it has been done in the lower extremity, and in some cases in the upper. Unfortunately it has had the sanction of Scarpa, Hodgson, Physick, and Liston. The rarity of its occurrence explains the delay in accumulating experience enough to reverse this authoritative endorsement. But on the other side we have Dupuytren, Breschet, Mott, and Norris; and, in addition, we have this great fact, which experience has taught us, that this form of aneurism is not usually progressive, so as to threaten life and to justify the use of a remedy attended by risk. This is exemplified in a man of forty, whose case was recently reported to the Clinical Society of London by Mr. Hulke, of the Middlesex Hospital. Three years previously, in Missouri, a Derringer pistol had exploded as the man was putting it in his right hand trowsers pocket, and a bullet had traversed the thigh, wounding both vein and artery at a point about six inches below Poupart's ligament. There was little bleeding at the time, and relief was ultimately sought some years afterwards because the limb was weak and painful, and its surface affected by eczema and ulcers. From the point of injury upwards, the femoral artery and vein were greatly dilated, and the risk in attempting

[1] The Practice of Surgery, etc.; London, 1876, vol. i. p. 435.

[2] Nélaton has the credit of pointing out the fact that, whilst the murmur of an ordinary aneurism can be heard only after the systolic impulse, and is therefore interrupted, the pulsatory thrill of an arterio-venous aneurism is continuous. It is intensified by the systolic impulse, but does not intermit. This circumstance should always arrest attention, as indicating the existence of venous communication. I have not verified the statement of systematic writers that, in varicose aneurism (*i. e.*, where a distinct tumor has formed), the interrupted bruit resulting from the passage of the blood in and out of the aneurismal tumor can be heard distinct from the continuous thrill caused by the opening into the vein. The "rasping bruit" is heard also in cirsoid aneurism, but everywhere equally; whilst in arterio-venous aneurism it is loudest at one spot, viz., near the point of communication. There is also less forcible pulsation in the artery below the point of communication in most instances where an artery opens directly into a vein; and a tendency to dilatation of arteries above.

to tie the former above and below the abnormal opening was considered too great. Compression, direct and indirect, was patiently tried and found useless, except in benefiting the eczema and ulcers. Ligature, as for simple aneurism, was seriously thought of, but deferred. Meanwhile the patient received so much relief by wearing an elastic laced stocking reaching from the foot to the groin, that he willingly accepted the advice to content himself with this palliative treatment.[1]

In a recent case, and in a young subject, the probability of a permanently weakened limb would possibly justify an operation as for a wounded artery, as in the cases of Spence and Annandale, of Edinburgh, and the use of an Esmarch's bandage would greatly facilitate such a proceeding. But in an old case, the number and size of the enlarged and tortuous vessels all carrying arterial blood, the enfeebled vitality of the limb, through habitually defective blood-supply, and the thinned walls of the arteries, unite to render all operative interference difficult, and, as regards gangrene and secondary hemorrhage, especially dangerous,[2] as pointed out by Breschet in 1838.[3]

As long ago as 1848 I sought to aid in establishing this point of practice. The late Valentine Mott tied the external iliac artery in a young man from Alabama, for an arterio-venous aneurism in the same locality as in the case just related, two years and a half after the gunshot wound by which it had been caused. The patient had complained of lack of power, and more or less pain in the limb, but mainly of the exaggerated pulsation and continuous vibratory thrill over the course of the vessels. After the operation the limb fell into gangrene at once, and the patient died on the sixth day. I had an opportunity of dissecting the parts, and found two small aneurismal tumors formed out of the adjacent tissues, besides a direct communication between the femoral artery and vein. Impressed by this grave reverse, I collected all the recorded cases within my reach in which arterio-venous aneurism of the lower extremity had been treated by ligature on the Hunterian plan, and found that gangrene, or secondary hemorrhage, had occurred in all of them. In twelve cases, the external iliac was tied in five, and gangrene followed in all; the common femoral in two, and gangrene followed in both; the femoral artery in five, and gangrene occurred in two of these cases, and hemorrhage in all.[4] Subsequently Norris deduced a similar result from the analysis of the cases of Hunterian operation for arterio-venous aneurism in his tables of collected cases of ligature of the femoral and external iliac arteries; and considering this result in connection with the clinical fact that arterio-venous aneurism in most instances tends to lose its progressive character and become stationary—causing only inconvenience, and not danger to life—this excellent surgeon recorded his opinion that sound surgery condemns a resort to operative measures in the treatment of such cases, "so

[1] Trans. Clin. Soc. London, vol. viii., 1875, p. 175. Similar cases of arterio-venous aneurism of indefinite duration, without material progress, must have presented themselves to most surgeons of experience. Norris quotes a case of Dupuytren's of twelve years' duration, and speaks of another he saw in Velpeau's wards in 1835, of twenty years' standing; and still another of "long continuance, which inconvenienced though it did not distress the patient."—Contributions to Practical Surgery, etc., Phila. 1873.

[2] Prof. Donald McLean, of Ann Arbor, dissected out and tied the popliteal artery and vein in an old case of arterio-venous aneurism (traumatic) above and below the point of communication, but lost his patient. (Private Communication, May 22, 1876.)

[3] Mémoires de l'Académie de Médecine de Paris.

[4] Cases of inguinal aneurism, with remarks. New York Journal of Medicine, vol. ii. (new series), 1848, p. 168.

long as the infirmity can be made at all bearable by the use of compresses, laced bandages, and other like measures."[1]

I regret that my time will allow me to say nothing of traumatic aneurism, especially as there are several most creditable American cases by which its treatment might be illustrated, in which in the neck, and even behind the ramus of the lower jaw—that most dangerous of all localities for a deep punctured wound—the tumor has been laid open and the vessel successfully tied above and below the wound in its walls. I may mention particularly the cases of Prof. Briggs, of Nashville, Tennessee,[2] of Prof. Donald McLean, of Ann Arbor, Michigan,[3] and also the more recent case of Dr. G. E. Frothingham of the same city.[4]

I am conscious that I have already taxed your patience too long with these imperfect notes; and I am equally aware that the extent of the subject has precluded the possibility, in such limited space, of reasoning up to very clearly defined conclusions. The treatment of aneurism at the present day clearly does not consist, as thirty years ago, in a choice between ligature and compression, but it involves judicial weighing of the claims of many remedial measures, the number of which is constantly increasing, and the selection of those which offer the best chances of safe and certain cure; and, where the chances of cure are uncertain, duty requires us to try those remedies which give the best promise of ameliorating symptoms and prolonging life. With the new methods of treatment at our command—to alleviate, if not always to cure—I will venture to assert that, whilst some of the operations on the great arteries sanctioned by distinguished names have failed to fulfil the hopes of their projectors—without any diminution, however, of the lustre of those names—Surgery is, nevertheless, richer in her aggregate resources; for there is no form of the disease which the well-trained surgeon of the present day can truly call incurable, or for which his art does not offer at least some hopeful resource.

Finally, I would submit the following conclusions for the consideration of the Section:—

I. Tufnell's treatment of aneurism by rest, position, and restricted diet offers a valuable resource in thoracic and abdominal aneurism.

II. It should always be tried in innominate, subclavian, subclavio-axillary, and iliac aneurisms, before resorting to measures attended by risk to life.

III. For aneurisms of the subclavian and iliac arteries, the Hunterian operation, with our present means of preventing secondary hemorrhage, is not justifiable.

IV. For reasons formally set forth by Holmes and Henry Lee, the "old operation" cannot properly be formally substituted for the Hunterian operation in these cases, but should be held in reserve for special cases.

V. It is the most safe and surgical resource in gluteal aneurism, if the circulation can be commanded by the hand in recto.

VI. The mode of cure by embolism, aimed at in the method of manipu-

[1] Contributions to Practical Surgery, Phila. 1873, p. 312.

[2] Nashville Medical and Surgical Journal, March, 1871.

[3] Peninsular Journal of Detroit.

[4] This case, which Prof. Gross in Amer. Journ. of Medical Sciences, April, 1876, p. 442, attributes to Prof. McLean, of the University of Michigan, I learn from this gentleman, belongs to Dr. Frothingham; Prof. McL. participated in it only as consulting surgeon.

lation, is a not unfrequent explanation of what is called spontaneous cure of aneurism.

VII. The value of Esmarch's bandage in the treatment of aneurism is probably not fully estimated.

VIII. In view of the promising features presented by the cases of Levis and Bryant, in which horse-hair was introduced into an aneurismal tumor, the repetition of this operation, or the substitution for the horse-hair of Lister's prepared catgut, or other animal substances, may be properly tried.

Discussion on Dr. Van Buren's Paper.

After the reading of the preceding paper, Mr. Jolliffe Tufnell, of Dublin, said:—I have brought with me from Dublin a number of alcoholic specimens, together with photographs and drawings, which show some of the cures which have occurred under the plan of treatment which I advocate in cases of internal aneurism. In following out the idea of the cure of aneurism by compression, I have found that it is not necessary to completely arrest the flow of blood through the sac, but that a simple diminution of the force of the circulation is all that is required. I have also found that the object can be attained by reducing the force and frequency of the heart's action, by placing the patient upon his back and by regulating the diet in such a way as to secure nutrition, and yet produce a decided lowering of vitality. A moment's calculation will show that if the number of pulsations be reduced from 86 to 56 in the minute, a gain will be secured of about 43,000 fewer pulsations in the day, a lessening of force which no one at first glance would fully realize. For the successful carrying out of this plan, certain conditions are necessary. Thus, (1) the aneurism must be upon the anterior wall of the artery, as those upon the posterior wall are frequently of the dissecting variety; (2) the sac must be entire, and (3) the blood must possess the proper fibrinating power. In those cases in which a hemorrhagic diathesis exists, and the patients bleed profusely from trivial injuries, the method is of course inapplicable. The difference in the force with which the blood is driven through the vessels is easily noticeable in one's own person, if the test is made between the reclining and the erect posture. I believe that the majority of cases of internal aneurism would recover if the patients would be content to lie in bed for three months and submit to the restrictions of diet, etc., which I have recommended.

In the treatment of aneurisms in the lower limbs, I have succeeded in effecting a cure by simply elevating the heel about six inches. I have, by this method, produced consolidation of a popliteal aneurism the size of a hen's egg, in twelve days, and the man is now doing full duty in the Royal Guards. I have here alcoholic specimens of several aneurisms cured by the plan which I have proposed: one is an aneurism of the thoracic aorta, in which the sac is filled with fibrinous layers; another, of the abdominal aorta, was cured in forty days, but soon afterward a dissecting aneurism formed lower down upon the vessel, and bursting, caused death, the first tumor being entirely solidified. In a third, in which the internal iliac artery was affected, both the aneurismal swelling and the degenerated artery became obliterated.

[The specimens referred to were then exhibited, and other cures were illustrated by drawings.]

Dr. J. H. Pooley, of Columbus, Ohio, said:—An opportunity is offered by one of the recent advances of surgery for us again to consider the old operation of laying open an aneurismal sac and tying both ends of the artery. This is the Esmarch bandage, which would not only act as a compressor, but would also give time to cut freely into the sac and leisurely hunt for the vessel

without fear of hemorrhage. In traumatic aneurisms, I believe that this is the most promising form of treatment, as these are practically wounded arteries.

Dr. Alfred C. Post, of New York, said:—Although the plan referred to by Dr. Pooley is undoubtedly a good one for aneurism of the traumatic variety, it would not be applicable to those of the idiopathic form. In the former case the walls of the vessel do not constitute the sac of the tumor, in the latter the vessel is diseased, and is likely to be in the same condition for some distance both above and below. In laying open the sac, in such a case, the artery itself would be opened and the ligature could not be properly applied within the sac. Even if the artery could be reached within the sac, this diseased condition of the coats would render secondary hemorrhage almost a matter of course.

Dr. George A. Otis, U. S. Army, said:—I understood Prof. Van Buren to refer to a case of ligation of the subclavian artery as a solitary example of shot-wound of the subclavian, in which the accident had not been followed by immediate death. The annals of surgery present other such cases, and several have been recently published. Prof. Billroth, in his Chirurgische Briefe zur Schusswunden, has cited several cases in which he was able to secure the right subclavian by ligature, after the above-mentioned injury, in the hospitals near Mannheim, in the Franco-Prussian War, 1870–71. I have endeavored to establish, in the fifth chapter of the Surgical History of the War of the Rebellion, that shot-wounds of the subclavian are not altogether beyond the resources of art, and have adduced five cases reported by reliable observers in which the fatal result after shot-injury of this vessel was delayed for several days, the artery having been secured in two instances by ligature. In one of the cases, the left subclavian was transfixed by a splinter from the shattered clavicle. I think that even in the presence of such supreme danger as is caused by an arterial lesion so near the heart, operative interference may sometimes be essayed.

Dr. John Ashhurst, Jr., of Philadelphia, said:—There is another set of cases in which the "old operation" may be properly preferred to the Hunterian method, and that is in certain cases in which the aneurism has suddenly become diffused. No doubt when this accident occurs in a case of popliteal aneurism, amputation is the safest mode of treatment, but in a case of femoral aneurism, laying open the sac and tying both ends of the artery may be advantageously substituted, as has been successfully done by Messrs. Birkett and Cooper Forster, of Guy's Hospital. Again, if an aneurism, in any locality, has ruptured externally, or has been accidentally opened through a mistake in diagnosis, the "old operation" may be the most available mode of treatment. Hemorrhage during the operation may be prevented by using, as suggested by Dr. Pooley, the Esmarch bandage, or, which I confess I think quite as good, the ordinary tourniquet. In axillary aneurism, the subclavian artery may be compressed over the first rib, through a preliminary incision, as advised by Mr. Syme; and in iliac or inguinal aneurism, bleeding may be prevented by using the aortic compressor, for the introduction of which surgeons are so much indebted to our President, Professor Lister.

I have lately had occasion to study the statistics of the operations of tying some of the larger arteries, and have been much impressed by the fact that as cases have accumulated the percentage of mortality has greatly increased. Thus, as regards the internal iliac artery, when the late Dr. George W. Norris published his classical paper, there were known but six[1] cases in which this

[1] Dr. Norris erroneously attributed a seventh (fatal) case to the late Dr. J. Kearney Rodgers, of New York; a mistake in which I confess to have followed him in my work on the "Principles and Practice of Surgery," in company with other systematic writers; and which has only recently been corrected by the laborious research of Dr. Otis, in the second volume of his Surgical History of the War.

vessel had been tied, and, of these six, four had ended in recovery; but while the whole number of recorded cases has since risen from six to twenty-five, the recoveries have risen but from four to seven, so that the death-rate has increased from only thirty-three per cent. to seventy-two per cent. Similarly, Dr. Norris collected sixteen cases of ligation of the common iliac artery, with eight recoveries and eight deaths, a mortality of fifty per cent.; while there are now recorded sixty-one cases, with only thirteen recoveries and forty-eight deaths, a mortality of seventy-nine per cent. These figures, it seems to me, speak very strongly in favor of bloodless methods of treatment, such as that which has been so beautifully illustrated to us to-day by the specimens shown by Mr. Tufnell, and which I, for one, am quite ready to agree with him in calling the "successful method of treating internal aneurisms."

Dr. S. Fleet Speir, of Brooklyn, said:—I would invite the attention of the Section to the "Artery Constrictor," which I have devised, and for which I claim the advantage of both effectiveness and safety. It consists of a mechanical arrangement for closing two jaws, one of which is curved so as to admit of its being passed beneath the artery, while the other forms a tongue, or male blade, and is forced down into a groove on the inner surface of the first. The advantage claimed for the groove is that it is so narrow as only to admit the external coat of the vessel along with the tongue, and that consequently the middle and inner coats must give way, and then retract and curl up so as to plug the artery with its own contents, and favor speedy coagulation. In this respect the "Constrictor" differs from Travers's instrument. I have used the "Artery Constrictor" successfully upon the femoral artery, in a case of popliteal aneurism, and also upon the common carotid in a case of supposed dilatation of the innominata, in which, however, it was found that the aorta was involved. In this case I tied the subclavian and constricted the carotid.

[Dr. Speir's instrument was then exhibited.]

The President, Mr. Lister, of Edinburgh, said:—I can testify to the value of Mr. Tufnell's method in many instances. One case I particularly recall which occurred in the practice of Mr. Syme, in which a patient, in whom it was proposed to tie the femoral artery for an aneurism of the popliteal, was placed in bed for a few days preparatory to the operation, and, although no particular attention was paid either to the lessening of the heart's action or to diet, yet, when he was brought to the operation table, the pulsation in the tumor was found to have entirely ceased. In regard to the distinction which has been drawn as relating to the traumatic or idiopathic nature of the affection, I do not think that this is a matter to be so much considered as the question which is the best and safest operation in the particular case, and the settlement of this question will depend more upon the position of the aneurism than upon its cause. In the case of popliteal aneurism, for instance, ligature of the femoral at the very apex of Scarpa's triangle, as practised by Mr. Syme, is a very successful operation, whether the tumor be traumatic or idiopathic in its origin; but ligation of the brachial for an enlargement of the vessel at the bend of the elbow, will probably fail to effect consolidation on account of the free anastomosis in this part. This intermingling of vessels is sometimes so intimate that occluding the brachial may not even destroy the radial pulse for one moment. In the arm, again, laying open the sac is attended with less danger than is the same operation in other portions of the body. Other important elements in deciding upon the form of operation are: The character of the tumor, whether fusiform or sacculated, and also the condition of the walls of the vessel. It should be remembered that the artery is just as likely to be diseased at a distance from the tumor as in its immediate neighborhood, since the location of an aneurism is usually the result of accident or some mechanical cause.

Should the Hunterian operation fail in aneurism at the elbow, I should certainly try the old operation.

With regard to the aortic compressor, or abdominal tourniquet, I would say

that, although I formerly supposed it to be original with myself, I afterwards found that it had been previously used by Professor Joseph Pancoast, of Philadelphia, who is entitled to whatever credit pertains to its introduction. As to the danger in the application of this tourniquet, referred to by Professor Van Buren, I think that it is due to the misapplication of the instrument. An inexperienced person, finding the artery beating below, may screw down the pad vigorously, while the difficulty really is that proper adjustment has not been effected. In such a case the intestine may easily suffer, but I guard against this by always placing a sponge beneath the pad.

I would like to say a word as to Mr. Holmes's allusion to the late Mr. Syme's advocacy of the ligature, as evidencing an "unaccountable prejudice" in its favor. The fact is that Mr. Syme lost but one case of femoral ligation out of forty, and that one from pyæmia. His other unsuccessful case was one in which a vascular tumor, not properly an aneurism, was treated by ligation, but proved fatal from hemorrhage. Mr. Syme always cleaned the artery very thoroughly before applying his ligature, and he tied the femoral below the apex of Scarpa's triangle, because, as he said, there are frequently very large branches given off from the femoral instead of from the profunda.

Professor Van Buren has said that one of the advantages of the antiseptic ligation of arteries is the avoidance of secondary hemorrhage. This is true, but there is also gained another important desideratum, namely, the diminished danger of diffuse inflammation. In ligation of the subclavian, if the operation were done thoroughly antiseptically, it would make no great difference even if the pleura should be opened, for the danger is not from such a puncture, but from suppuration, and when this is avoided the operation becomes a far safer one.

Dr. Ashhurst said:—I must say one word as to the history of the aortic compressor. I had the pleasure of being present when Professor Pancoast first used an abdominal tourniquet in amputation at the hip-joint (in 1859), and have myself since used, with satisfaction, the identical instrument, which is still extant in Philadelphia, in hip-joint amputation. But with all due regard to national pride as American surgeons, I do not think that we can afford to let the modesty of our President, Professor Lister, deprive him of the credit which is justly due him in this matter. The instrument which was used by Professor Pancoast was simply a large-sized Skey's tourniquet, and it is inferior to Professor Lister's ingenious clamp, or compressor, being more difficult of application, and of more complicated construction. Moreover, Professor Pancoast's use of the instrument was hardly known out of Philadelphia, and did not attract the attention of surgeons generally; so that although as a matter of fact he undoubtedly preceded Professor Lister in the employment of mechanical compression of the aorta, to the latter is as undoubtedly due the credit of having perfected the mechanical means of effecting such compression, and of bringing the method before the profession all over the world as a practical resource in surgery.

Mr. Tufnell said:—I would just mention that, many years ago, a similar instrument was devised by Mr. L'Estrange, of Dublin, for the compression treatment of aneurism, and is still preserved in the museum of the Royal College of Surgeons of Ireland.[1]

On motion, the conclusions of Dr. Van Buren's paper were adopted as expressing the opinion of the Section.

[1] [It may be added that in Mr. Birkett's and Mr. Forster's cases of diffused femoral aneurism, treated by "the old operation," the aorta was compressed by a tourniquet "recommended by Dr. Humble, many years ago, for compressing the abdominal aorta in cases of post-partum hemorrhage" (Trans. Clinical Society of London, vol. i. p. 38), so that the question of actual priority in the use of an aortic compressor probably cannot be settled.—Editor.]

THE PATHOLOGY AND TREATMENT OF MORBUS COXARIUS.

BY

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Mr. President and Gentlemen:—

The time allotted me for discussing Morbus Coxarius, its causes, symptoms, and treatment, is so limited, that I can only briefly refer to the first two, and for more minute information must refer you to my published writings on this subject, where more definite and full information has been given; and I shall occupy a portion of the time with a practical illustration of the plan of treatment in each different stage of the disease, as being the best manner of conveying the largest amount of information in the time that has been allotted to me.

Morbus coxarius, or hip-disease, as it is generally called, is more frequently observed in childhood and early youth than at any other period of life, although it may occur in the infant or the adult. It is generally, if not always, the result of some direct injury to the joint involved, either (1) by a blow or concussion of the head of the femur against some portion of the acetabulum, producing an extravasation of blood in the network of vessels underneath the articular cartilage of one of these parts, and ending in caries and necrosis; (2) by some strain or overstretching of the joint, producing rupture more or less complete of the ligamentum teres, and its vessels and nerves, thus interfering with the nutrition of the head of the femur, and inducing necrosis; or (3) by excessive violence and over-exertion, producing free perspiration and synovial secretion, which is suddenly checked by an exposure to damp and cold, thus producing a synovitis which not unfrequently ends in suppuration.

The young infant is protected by the watchful mother or the careful nurse; and the adult has generally sufficient prudence to protect himself; but the reckless, romping, healthy child indulges in wild sports without discretion, and slight injuries are unnoticed until serious consequences have resulted, and often until so long a time has elapsed between the receipt of the injury and the development of symptoms sufficient to attract attention, that the cause of the difficulty is entirely forgotten, and the constitutional effects of long-continued irritation and suppuration are mistaken for the *cause* of the disease, instead of the *result*.

Until within a very few years, every author taught that the disease was necessarily connected with a strumous condition of the system, and could not exist without it, and that it was therefore necessarily of constitutional origin, and never occurred in the robust and healthy. This doctrine I believe to be incorrect; in fact, by a careful examination of my recorded notes of many hundreds of cases of morbus coxarius, I find that by far the larger number occur in children of perfect health, and born of healthy ancestry, and the simple reason is that children of this class are more active and daring, and therefore more exposed to accidents and in-

juries, than sickly strumous children who seldom have energy enough to expose themselves to any danger. But even the strumous constitution requires some local injury to the part itself, in order to develop the disease, and therefore I am inclined to regard the disease as almost always, if not always, of traumatic origin.

Of course the sickly strumous child, having less recuperative power and vital force to resist disease, will have it developed from a much less exciting cause than would be required to develop the same trouble in the healthy and robust; but even among the strumous I believe that, if sufficient care is taken in the investigation, the disease will nearly always be traced to some slight injury which was considered of so little importance as to pass unnoticed at the time, and that months afterwards, when the serious consequences of this slight injury have been fully developed in the well-recognized hip-disease, the universal belief in the doctrine of its constitutional origin has prevented the surgeon from examining for any other cause.

This has been the cause of the fatal error in the treatment of this disease; for, of course, as long as we believe that the disease depends upon constitutional taint, all our efforts will necessarily be directed towards correcting this constitutional poison or element. The result of all such treatment has been either death, after many months or years of suffering and exhaustive suppuration, or else recovery with more or less deformity, and with imperfect motion, or anchylosis more or less complete. Whereas, belief in its traumatic origin (no matter what the constitutional condition may be) will direct our treatment to the part involved, and, if the disease is early detected and properly treated, will result in the vast majority of cases in recovery with perfect motion and without deformity.

For convenience of description, I will divide the disease into three distinct stages, as each represents a different pathological condition of the parts involved; and, as the symptoms vary to a greater or less degree, so does the treatment applicable to each:—

I. The stage of irritation.

II. The stage of effusion.

III. The stage of rupture of the capsule, or perforation of the acetabulum.

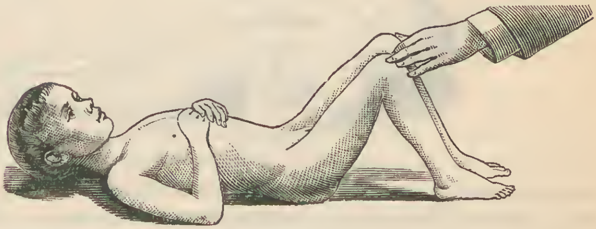
In the *first stage*, or the stage of *irritation*, before effusion has occurred within the joint, the symptoms are not well pronounced, and it often requires a very careful investigation in order to recognize the disease. Generally, the first thing noticed is that the child appears very slightly lame when he first gets out of bed in the morning, or when he first moves about after some hours of rest. This limping or halting gait is so slight as hardly to be observed, and, after a few minutes of exercise, may disappear altogether until the following day, or until after a few hours of rest, when it will again make its appearance on the first attempt at movement. The patient may sometimes complain of pain even in the early stage of the disease, but it is generally referred to the knee. Even in this early stage, however, if the child be properly examined, the disease can always be detected.

By stripping the child naked, and standing him on a table or on the floor, with the back towards you, the first thing noticed will be that the child bears his entire weight upon one limb, the other being slightly bent at the knee and hip, with its natis lower and more flattened than on the opposite side, while the corresponding gluteo-femoral crease is

lower than the other, is less distinct, and nearly obliterated at its outer angle. If you now let the child walk around the room, he may not limp at all, or not sufficiently to attract attention, in this early stage of the disease; but when you bring him back to the position as first described, and let him stand a moment or two, you will find that he invariably resumes the position of sustaining his whole weight upon the sound limb. (Fig. 1.)

Fig. 1.

Fig. 2.



You next lay the patient on his back upon a table, floor, or some other solid plane, covered only with a blanket, and lift his lower extremities until the entire spine touches the plane (Fig. 2), and twist the pelvis one way or the other until a line drawn from one anterior superior spinous process of the ilium to the other will be crossed at right angles by another line drawn from the centre of the sternum over the umbilicus to the centre of the symphysis pubis.

When the spinal column touches the plane, and the two lines above mentioned are at right angles to each other, the spinal column is slightly straighter than normal; but it and the pelvis are at right angles, and, if no disease exists within the hip-joint, the limbs can be brought down upon the plane, so that the popliteal spaces can be made to touch the latter without disturbing the relation of the lines described, and without lifting the spinous processes from the plane. If you therefore hold the suspected limb in your hand in such a manner as to keep the spinous processes on the plane while the other lines are at right angles with each other, you will observe that the well limb can be pressed down to the table so that the popliteal space will touch. (Fig. 3.) The diseased limb can be pressed down nearly to this position, but, before the popliteal space touches the plane, you will notice that the pelvis becomes tilted, making a curve in the lumbar vertebra, so that the hand can be passed between the child's back and the plane. (Fig. 4.) In flexing the limbs,

the well one can be completely flexed, so that the knee will touch the thorax; the diseased one cannot be flexed to this extent; and before the knee touches the thorax the pelvis becomes lifted from the plane. Abduction and external rotation may, possibly, at this early period, be

Fig. 3.

Fig. 4.

carried nearly to their normal extent without much pain, but adduction and internal rotation are much more limited. Pressing the head of the femur into the acetabulum, by concussion at the knee or pressure over the trochanter major, will give pain, providing the pressure is made so that the head of the femur shall sweep around all portions of the acetabulum, the pain being made manifest the moment the parts come in contact in which the disease exists. Extension, even though very slight, in the proper direction, gives instant relief from pain, while pressure causes it as instantly to return.

Atrophy of the muscles of the thigh, from tonic contraction caused by reflex irritation, will often be found even in the earliest stages of the disease, so that the measurement of the two limbs will often differ by from one-eighth to one-fourth, or even half an inch. Rigidity of the muscles is one of the earliest symptoms observed, and is always present until the disease is entirely arrested.

If the disease be detected in this early stage, and properly treated, I am satisfied, from an extensive experience, that the great majority of cases will entirely recover, with perfect motion and without deformity. If the disease be not detected at this stage, and properly treated, it progresses until effusion takes place within the joint; and, in order to accommodate this increasing effusion, the limb becomes more flexed, more abducted, and more everted, or outwardly rotated—to unfold, so to speak, the capsular ligament, thereby enabling it to accommodate itself to the increased amount of fluid within.

This is the *second stage* of the disease, or that of *effusion*. The adductor muscles become more rigid and contracted under the influence of reflex irritation; constant efforts are made to draw the thigh inward, but without avail, as it is impossible for the limb to yield to their tractile force, because the effusion within the capsule compels it to assume the position above described.

This stage of the disease is attended by the most acute and agonizing pain, the slightest attempt at motion, concussion, or compression causing

the most extreme torture; even the jarring of the bed, stamping upon the floor, the slamming of a door, or anything that causes the least movement of the bed upon which the little sufferer lies, may be followed by an increase of pain. At this period of the disease the attendant is frequently awakened at night by a sharp, shrill, agonizing shriek. The nurse runs to the child, and probably finds it asleep. She will have hardly left the bed before the same thing occurs again; and this is often repeated a number of times during a single night. The inflammation of the joint produces reflex contraction of the muscles, thereby adding to the pressure between the diseased surfaces of the bones, and promoting absorption of the same, as also the peculiar deformity connected with this disease. The muscles are kept in this state of constant contraction in order to prevent any motion in the joint; but this incessant, constant, unremitting effort so exhausts the patient that finally he falls into a moment's slumber, from sheer fatigue, when, the muscles being relaxed, the limb changes its position, thereby producing motion in the joint, and causing such instantaneous pain that the muscles instantly give a spasmodic contraction, followed by the piercing scream to which I have alluded.

Of course, at this period of the disease, it can hardly be mistaken for any other, or misunderstood; but upon stripping the child, and examining him as I have directed that he should be examined in the first stage, he will be found to present, both in the erect posture and in the recumbent position, precisely the same appearances as in the first stage, only in a more marked degree (Fig. 5), the chief differences being that the limb

Fig. 5.

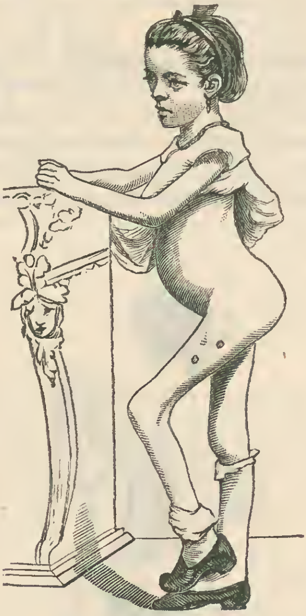
will be more flexed, *abducted*, and *everted*, or rotated outward, and the joint more fixed; in fact, any attempt to move the limb in this stage of the disease is futile, the entire pelvis rolling upon the opposite acetabu-

lum as if the diseased joint were positively anchylosed, though the supposed anchylosis is only apparent, being wholly due to muscular rigidity.

If the disease be not arrested at this stage, it goes on to ulceration of the capsule and effusion of its contents into the cellular tissue of the thigh, or else to perforation of the acetabulum and escape of the fluid into the pelvic cavity, pressing off the internal periosteum before it.

At this point begins the *third stage* of the disease; and as soon as the capsule is ruptured, or the acetabulum perforated, and the contents of the joint escape, either into the cellular tissue of the thigh, or into the pelvic cavity, the limb at once assumes an entirely different position. It now becomes *adducted*, *inverted*, and more straight at the knee; the pelvis on the diseased side becomes raised (whereas in the first and second stages, it was lower than on the sound side); and the limb is shorter. The gluteo-femoral crease is higher than upon the opposite side, whereas, in the previous stages it was lower, or entirely obliterated. (Fig. 6.)

Fig. 6.



If the rupture through the capsule is very large, so that the contents escape rapidly, this change in the position of the limb from that of the second to that of the third stage may take place in a single night; while if the opening is small and fissure-like, and the contents ooze out slowly, the change of form may not be completed for several days. In some cases, even when the capsule has ruptured, or the acetabulum been perforated, the limb will remain in the position of the second stage, owing to adhesions which may have formed, or to the head of the bone being locked in the opening through the acetabulum. The pain is greatly relieved upon this rupture of the capsule and escape of its contents, but the disease has only advanced one stage further in its progress.

It has been thought by many authors that, when this change takes place, the head of the femur is absolutely luxated from the acetabulum, and that this luxation is due alone to spontaneous muscular contraction. The

sudden change in the distortion, from flexion, abduction, and eversion, with elongation, to shortening, inversion, and adduction, has caused this belief in the occurrence of positive luxation, but in the sixty-three cases in which I have exsected the hip-joint, I have never seen luxation upon the dorsum of the ilium except in one single instance, and in that the dislocation was produced a few days before the operation by bending the limb in the effort to remove the patient from the bed, and was therefore due to the carelessness of the nurse, and not to spontaneous muscular contraction. The absorption of the head and neck of the femur, produced by constant pressure, diminishes its size, while the ilium being also pressed upon, becomes eroded and absorbed, thereby immensely increasing the size of the acetabulum. (Fig. 7.) But while this inter-

Fig. 7.

stitial absorption has been going on within the acetabulum, there has been at the same time periostitis upon its upper and outer borders, causing the formation of new osteophytes; and the capsular ligament, attached to these new deposits, has thus been gradually pushed upward and backward on the dorsum of the ilium, immensely increasing the size of the joint, but still retaining within its embrace what is left of the head and neck of the femur. It might therefore with propriety be called a displacement of the acetabulum, but not a luxation of the head of the thigh-bone.

This may appear a small point to cavil about, but accuracy in observation is essential for obtaining correct knowledge of the pathology of any disease, and unless our pathology is correct, our treatment will be necessarily empirical.

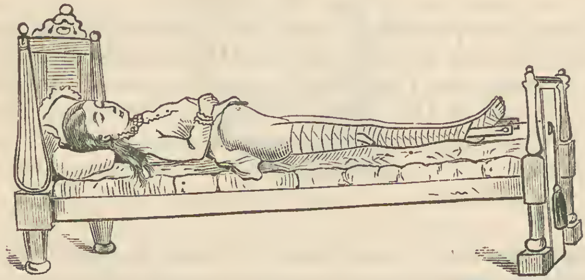
The pus or other fluid having now escaped from the capsule, the patient is greatly relieved from the pain, but the disease still progresses. Constant muscular contraction produces interstitial absorption of the head and neck of the femur, and of the acetabulum, and the pus, which has escaped into the cellular tissue, burrows in different directions, according to the position which the patient maintains, forming extensive abscesses which finally make their way to the surface. These sometimes open behind the trochanter, sometimes at a long distance from it, opening either upon the outer or inner side of the thigh, or on both; and even,

as I have seen in cases in which the acetabulum has been perforated, making their way between the internal periosteum and the ilium, and forming openings above the pubis; so that the third stage of hip-disease may be accompanied with external fistulous openings in various directions, and with very great exhaustion from extensive suppuration.

We are now prepared to study the *treatment* of morbus coxarius, which of course varies according to the stage in which we find the disease; and the reason that I have dwelt so particularly upon the diagnosis of the affection in its earlier stages, is because, as I have before stated, proper treatment at this time, in the great majority of cases, will be followed by perfectly satisfactory results.

In the *first stage* of the disease, if the symptoms of inflammation be very acute, and pain and tenderness very great, *absolute and perfect rest of the joint* is most essentially requisite. In the hearty and robust patient, in vigorous health, leeches or cups, or some other form of local depletion, may be necessary; ice bags, surrounding the joint, often afford the greatest possible relief, while in other instances hot fomentations, the exact opposite, will give the greatest ease. There is no rule with which I am acquainted, that will guide you in the application of heat and cold, except the feeling of the patient, and this can only be determined by a practical test. The remedy which gives the greatest relief, and is the most agreeable to the patient, is the one to employ. In addition to these local measures, extension (very slight, but continuous) *in the line of the deformity*, should be made by means of a weight and pulley, secured to the limb by strips of adhesive plaster, and a roller. The adhesive strips should always extend above the knee, to avoid traction upon this articulation. The pulley should be attached in some manner to the bed, the foot of which should be elevated ten or twelve inches, so as to make the body act as a counter-extending force. (See Fig. 8.)

Fig. 8.



When the extension is first applied, the traction should be made in the line of the deformity, and the direction should be changed by slow degrees, day by day, until the limb is gradually brought into its natural position. It is sometimes necessary to apply a second extending power, at right angles to the limb, to remove the pressure of the head of the femur against the inner surface of the acetabulum. This is readily done by passing a handkerchief around the upper and inner part of the thigh, securing in its outer loop a cord to which is attached a weight playing over a pulley at the side of the bed.

If the bowels are constipated, cathartics as a matter of course are indi-

cated; and all the secretions and functions of the body must be carefully looked to and kept as nearly in a normal condition as possible.

This plan is to be pursued until the more acute symptoms have subsided; but as the disease is chronic in its nature, *time*, as well as *rest*, is a very important element in its treatment. And as long confinement in bed is injurious to the general health, we must contrive some mechanical appliance which will give the necessary amount of extension and counter-extension to relieve the joint from pressure, while at the same time it allows it to have free motion, and permits the patient to take exercise in the open air.

In some cases, when the disease is very acute, and the child very small, this is best effected by placing him in a wire cuirass (Fig. 9), which is a modi-

Fig. 9.

fication of Bonnet's "*grand appareil*," and which will be found very useful. When this instrument, or any other fixed apparatus, is employed, it is necessary that the patient should be taken from it very frequently; and all the joints should be carefully moved, lest too long-continued rest may terminate in anchylosis, not only of the joint diseased, but of all the other articulations thus permanently deprived of motion. I am aware that Dr. Thomas, of Liverpool, has denied this doctrine; but having seen the result in a number of cases, I must be pardoned if I insist upon placing more confidence in my own personal observations than in the theories of

any one. Perfect rest, too long continued, even of the diseased joint, is decidedly injurious, as there is danger of its resulting in anchylosis; hence the objection to plaster of Paris, or any other fixed apparatus, in the treatment of this affection. The disease is essentially *within* the joint, the capsular ligament not being involved; hence, all that is required is extension and counter-extension, just sufficient to prevent the diseased surfaces from coming in contact; while at the same time motion is permitted, to keep the capsular ligament and other parts not involved in a healthy condition, by allowing the free use of this their natural stimulus.

If the child is large enough to run about, and the thigh sufficiently long to give attachment to the adhesive plasters, then the short splint (Fig. 10) is altogether preferable. I have used this splint for many

Fig. 10.

years, and having tried all others, I find it altogether the best wherever it can be applied, as it allows free flexion at the knee, and is, therefore, more comfortable in the sitting posture. If the patient is ten or twelve years of age, and too heavy to bear the weight of the body upon the instrument without breaking it, or if too much tension is produced upon the skin by the adhesive plasters, then crutches will be necessary when the short instrument is used. If the child's thigh is too short, and he is too small, to receive a sufficient amount of extension by the use of the short splint, then the long splint, which I here show you (Fig. 11), is much to be preferred, and with it, if properly applied, the patient will be able to walk without the use of a crutch.

The short splint and its various modifications, together with the long splint with its abducting joint and rotating screw, and their mode of application, have already been so fully described (in my work on Diseases of the Joints), that I shall barely refer to them here.

The short splint (Fig. 12) consists of a curved cross-bar, surmounting the crest of the ilium or entire pelvis, well padded on its inner surface, and to its two extremities are fastened a perineal band or bands, for counter-extension, and on its outer surface a ball-and-socket joint, from which

runs an iron rod or bar down the outer side of the thigh to within about two inches of the lower end of the femur. This outer bar is divided into two sections, one running within the other, and gauged or controlled by a ratchet and key, which can make it longer or shorter. At the lower

Fig. 11.

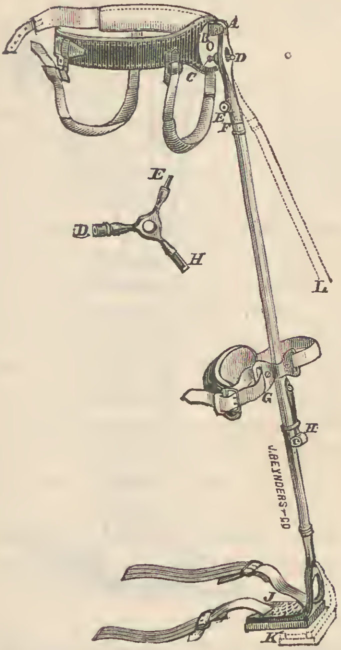
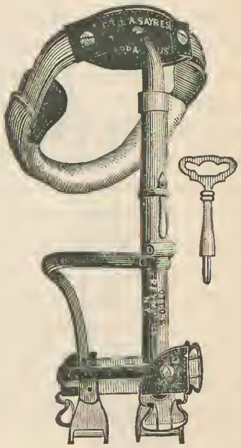


Fig. 12.



extremity of this outer bar is a projecting branch going around to the inner surface of the thigh to receive the attachments of the plaster, hereafter to be described. Both of the lower extremities terminate in cylindrical rollers, over which the tags of the plasters are attached to the two buckles placed at the lower ends of the instrument.

In applying the instrument, it is first necessary to have the adhesive straps to which it is to be fastened properly secured, and this is done as follows. When using the short splint, which is only worn during the day, night-extension is necessary, which is effected by means of weight and pulley: for this purpose a strip of adhesive plaster, to the lower end of which a stout piece of webbing is sewed, is placed on either side of the leg, extending from the malleoli to *above* the knee, in order to avoid traction on the lateral ligaments of the knee-joint; this is secured by a well-adjusted roller, leaving the pieces of webbing projecting for the attachment of the extending force. (Fig. 13.) Next, for the application of the instrument, triangular pieces of plaster, in which are cut several slits converging toward the apices of the pieces, are placed on both the outer and inner side of the thigh, first measuring with the instrument so that the tags which have been sewed to the apices of the plasters will exactly conform to the places of attachment upon its lower extremities. Having secured these with a roller, using care at the upper part of the thigh to reverse each alternate strip of the plasters in carrying round the

roller (Fig. 14), and with another turn taking in the other strips—braiding them in, basket-shaped—run the roller down the thigh again and sew.

Fig. 13.

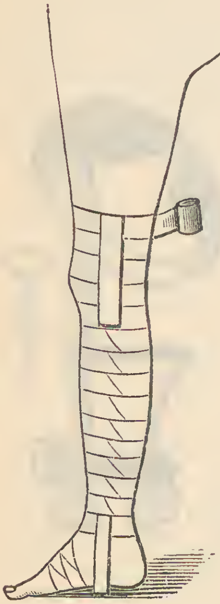
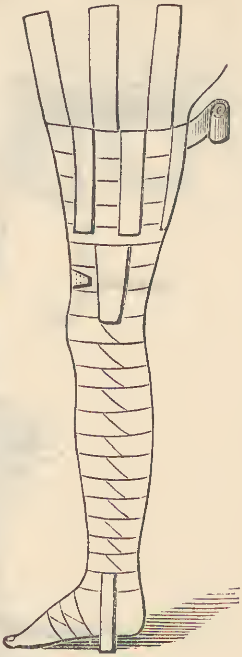


Fig. 14.



In applying the instrument, first buckle on the straps at the lower extremity of the femur. Then pass the strap under the femur to the buckle at the outer side of the instrument for the purpose of keeping it in place. We now pass the perineal band around the perineum, and buckle it snugly, but not too tightly. We next insert the key into the ratchet, and make gradual extension, until the patient is made perfectly easy, and until compression can be made upon the femur against the acetabulum without pain. It is sometimes necessary to employ two perineal bands, and then the cross-piece at the top nearly surrounds the pelvis.

If the disease has arrived at the *second stage* before we see it, and the effusion is very great, which will be indicated by the abduction, flexion, and eversion of the limb—sometimes even fluctuation can be detected—the patient must be kept in bed and the extension applied to the limb exactly in the line of the distortion, which will be in the line of flexion and abduction. This line of extension is to be changed day by day, by slight degrees, until the limb is brought as nearly as possible to the straight position. Blisters applied occasionally over the joint may hasten the absorption of this effusion. Firm strapping with adhesive plaster around the joint and compression with a sponge and roller may also be applied for the purpose of aiding absorption; of course, extension being used before this compression is employed. It is barely possible that the effusion may be so great as to paralyze the absorbents, and that no treatment will decrease the effusion. Under such circumstances aspiration of the joint is not only advisable, but the proper treatment, and

will be immediately followed by a restoration of the joint to its natural position.

When the limb has been brought to nearly its normal position, then the treatment by the short or long splint, according to circumstances, is the same as in the first stage of the disease, the plasters being re-adjusted as often as necessary. Good adhesive plaster (Maw's, of London, I have found to be the best), properly applied, will frequently remain in position from two to four months, seldom requiring removal oftener than once in six weeks or two months.

If the disease has gone to the *third stage*, capsule ruptured, abscesses formed and not yet opened, it is necessary to puncture these abscesses at various points where they are nearest the surface to prevent the pus from burrowing. The limb then being adducted, the extension, as a matter of course, must be exactly in the opposite direction from what it would have been in the second stage of the disease, and the limb gradually abducted until it is brought parallel with the other, when the splint, either long or short, is requisite, to be modified by the *abducting screw*, which I have been in the habit of using for many years with great advantage. In numerous instances, even when the disease has progressed to this stage, by the use of the splint the patient is enabled to improve the general health by out-door exercise, which frequently results in perfect recovery, and in some cases with a moderate degree of motion. The majority of the cases, however, that have arrived at this point before proper treatment has been adopted, are apt to recover with more or less complete anchylosis; in fact, anchylosis should be considered in this stage of the disease a very favorable termination.

The long splint (Fig. 11) which we sometimes have to use, differs from the short one, in the following particulars:—

In the first place, it extends the entire length of the limb, receiving the weight of the body at a cross-bar under the foot, and upon two perineal straps which are attached to an iron girdle which very nearly encircles the pelvis. Where the adduction is great and the joint fixed, it will be necessary also to apply the abducting screw, and in some cases, when the inversion is very great, a screw for the rotation of the foot outward is also necessary. The long bar, reaching from the pelvis to the bottom of the foot, is hollow, and has another bar running inside of it furnished with a ratchet and key, by which we make extension, and which is locked in the same way as in the short splint. The cross-bar at the bottom of the instrument is covered with leather to keep it from making a noise on the pavement while walking, and a strong leather strap is passed beneath two iron rods above this latter for the purpose of buckling on to the adhesive strap upon either side of the leg to make extension.

In applying it you take two strips of strong moleskin adhesive plaster, from two to four inches in width, according to the size of the patient, and the entire length of the limb, the upper extremity of the plaster being divided into strips for two or three inches. Strong webbing, an inch or two in length, with buckles, is sewed fast to the lower extremities of the plasters. These plasters are then placed on either side of the leg in such a manner as to leave the buckles a little above the ankle-joint, and secured by a snugly-adjusted roller, so applied as to leave the tags with buckles attached hanging loose, the roller being carried up over the knee, and as far up the thigh as can be done with convenience, when the upper split ends of the plasters are reversed and braided in with the roller as it

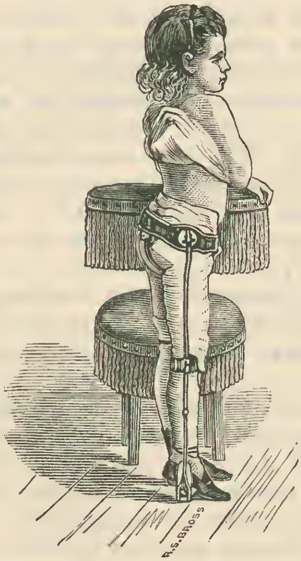
turns down the thigh, securing it smoothly. The stocking is next pulled up on the foot, and holes cut on either side for the buckles to pass through, and the shoe then applied, and corresponding holes cut through it for the same purpose.

The instrument is now placed on the outer side of the leg, with the cross-bar at the bottom brought in front of the heel of the shoe, and securely buckled to the tags above described. The pelvis-belt is next brought around the hips, and secured by the buckle upon the opposite side, and the perineal bands attached as firmly as need be. The knee-pad band is then slipped up or down until it is made to rest opposite the knee, where it is passed round the leg and buckled fast. Extension is now made by the key upon the ratchet until free compression is borne without pain. The abducting screw is then to be used, and daily turned, for the purpose of abducting the limb.

If the limb be strongly flexed, an additional power is applied at the posterior part of the instrument at the knee, running up the back of the thigh, and secured to the posterior portion of the pelvis-belt, and made tighter as occasion may require for the purpose of extending the limb. This latter strap should be always elastic, for the purpose of keeping up a constant tractile force, and at the same time allowing of flexion when the patient wishes to sit down. A fixed or leather strap, as used by Taylor, prevents any motion whatever at the hip, and simply anchyloses the joint.

By this means many cases that have gone to the third stage of the disease may in the course of time recover, with tolerably good form and a moderate degree of motion, without any further operative procedure. (Fig. 15.)

Fig. 15.



If, however, notwithstanding your treatment, the disease progresses, and suppuration increases, the joint becoming more and more impaired, showing a case of progressive caries, we then have no remedy except in *exsection*.

Nature's only way of curing these cases after they have arrived at this point is by the slow exfoliation of the carious bone, and, if this is limited in amount, she is often successful; but if involving the entire head and neck of the femur, with more or less of the acetabulum, as it frequently does, the process is a very tedious one, and the patients often succumb before nature completes the cure; and even in the most favorable cases healed by nature in this way, they have been left always with permanent deformity and imperfect motion—in fact, with a very much less useful limb than those which have been cured by exsection. I have now performed this operation sixty-three times, and can, therefore, speak with positive assurance upon the subject.

The operation is very simple, indeed, and in itself attended with almost no danger. The patient being anæsthetized and laid upon the well side, an incision is made from a point midway between the crest of the ilium and the top of the trochanter major, the knife carried firmly down to the ilium, and drawn with a single sweep downward and outward over the posterior edge of the trochanter major, and then curved forward and inward, making a crescent-shaped incision of some four to six inches in length, according to circumstances, and carried fairly down to the bone in its entire extent. The wound is then held open with spatulas, and a narrow firm-bladed bistoury is carried around and close to the femur just above the trochanter minor, and at right angles to the first incision, dividing only the periosteum, but in both directions, as far around the bone as can be reached, one-half or three-quarters of its circumference. By this circular division of the periosteum you avoid the danger of tearing it off from the femur below the point where section is to be made. If the first incision has not divided the periosteum completely, then carry your knife again through the first incision from the top of the trochanter major down to this cross-incision just described, pressing it firmly through the periosteum down to the bone. The periosteal elevator is then placed in these two triangles, and the periosteum peeled off from the trochanter major, carrying with it necessarily the muscular attachments to it. This can be very successfully done until you reach the digital fossa at the neck of the bone and behind the trochanter major, where the blade of the knife will be necessary to divide the tendinous insertions of the rotator muscles. The capsule being freely opened, the head of the bone will now be easily lifted from the acetabulum by strongly adducting the limb and depressing it, thereby tearing off the internal portion of the bone from its lining periosteum, when the finger can be glided around the bone, and with a finger or chain saw it may readily be removed below the trochanter major. By this means the periosteum will not be peeled off from the bone below the point of section with the saw, as is too often done by luxating the bone too forcibly.

If upon the first section it is found that the caries has extended still further down the femur, you can very easily separate it from its periosteal attachments, and whatever amount of bone is necessary can be removed in the same manner with the saw. Under no circumstances should bone forceps be used in the section of so large a bone. The trochanter major should always be removed, even if it is not diseased, as otherwise it would occlude the opening and prevent the escape of the discharge; and by peeling it from its periosteum, as I have before described, the attachments of the muscles are all left for future use.

When the head and neck have thus been removed, you have a fair opportunity to explore the acetabulum, and to remove all the carious or

necrosed bone by scraping and gouging. If the acetabulum be perforated, which I have frequently found to be the case, with a little care the necrosed bone can be chipped off down to the point at which the periosteum is attached. I have only in one instance found the internal periosteum perforated.

After washing the wound carefully with warm water, fill it with Peruvian balsam; a small plug of oakum should be inserted to the very bottom of the acetabulum, and left dependent from the wound. The upper and lower ends of the incision are then brought together by stitches, and if necessary adhesive plaster, and the patient placed in a wire cuirass (Fig. 9) with a window opposite the place of incision. As it is of the greatest importance that this dressing should be done with care, I will describe to you my mode of doing it.

The cuirass being properly prepared and well padded, the patient is laid in it so that the anus is opposite the opening, and free from any possibility of obstruction. The well leg is the first one to be dressed. By making it perfectly straight and screwing up the foot-rest until it is brought firmly against the heel of the patient, having a pad between the foot and the iron rest to absorb the perspiration, the instep is then well padded with cotton or a blanket, and a roller is carried firmly round it and the foot-rest, running up over the limb; but before going over the knee a piece of pasteboard, or leather, or several pieces of folded paper, are placed over the leg, knee, and thigh, and the roller carried firmly over this extemporized splint for the purpose of preventing the slightest bending of the knee, when the roller is carried up the entire length of the thigh, around the perineum and over the outer arm of the instrument, and several times back around the perineum, and then across the pelvis, by which means the well limb is made a firm counter-extending force.

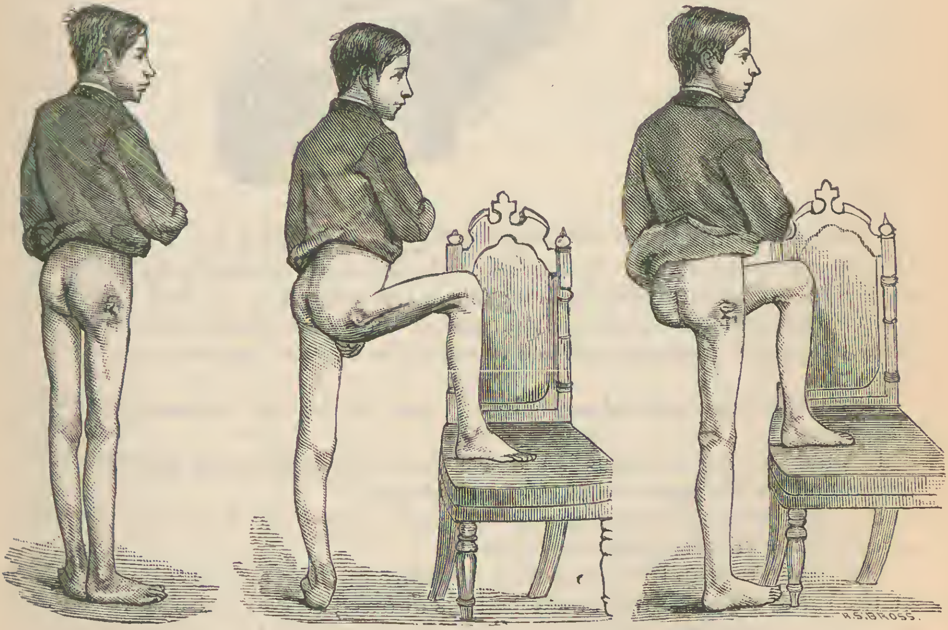
Two strips of adhesive plaster from two to four inches in width, according to the size of the patient, are then placed upon either side of the limb which has been operated upon, and secured with a nicely-adjusted roller over the foot and up the limb and thigh, as far as the abscesses on it or the wounds will permit, being careful to leave a sufficient length of the plasters, at the lower extremity, free for the purpose of applying them to the foot-rest when extension is made. The foot-rest is then screwed up to meet the heel of the shortened limb, and these strips of adhesive plaster are brought down around the foot-rest and securely fastened. The foot-rest is then extended by the screw, slowly and gradually, at times waiting a few moments for the muscles to yield, which have been so long contracted, until the limb is brought down to its full extent. It sometimes happens that, from long contraction of the adductors and the tensor vaginæ femoris, subcutaneous section of those tendons and fascia will be requisite before the limb can be brought to its proper position, even after the head of the femur has been removed. After the limb is brought into this position a roller is carried from the foot over its entire surface; a large wad of oakum is placed around the wound to absorb the discharge, and the roller is carried firmly over the wound, inner surface of the thigh, and around the pelvis. I place great stress upon this latter part of the dressing, as we thereby compress the tissues and prevent the burrowing of pus, the oakum, which has already been placed in the wound, allowing of free drainage, no matter how tightly the roller may have been applied.

Immediately after the patient is dressed in this way, and has recov-

ered from the anæsthetic, he is capable of being stood up against the wall, or riding out in a carriage or boat, and can take his daily exercise in this way. I have, in several instances, had patients removed a long distance, some miles, in fact, within an hour of the operation and without the slightest inconvenience or pain. This dressing will probably not require to be changed for from forty-eight to sixty hours, or until sufficient secretion has been formed to moisten the dressings, when the oaken plug can be removed without hemorrhage. If this dressing does not come away easily, warm water injections will readily float it out. The wound, made clean, is again filled with Peruvian balsam and dressed as before. After this it may require dressing once or twice a day, according to the amount of discharge, and the child should be removed from the entire instrument as often as it is requisite. The well leg should be removed from the instrument at least once a week, and free movements given to all the joints, ankle, knee, and hip, otherwise we may anchylose them, although they are not diseased. The wire cuirass should be used from a month to two months, or more, according to necessity, after which the patient can be put upon the long or short splint, and allowed to exercise, thereby increasing his prospects of perfect motion of the new joint.

In many of the cases which I have exsected, the motion has been as perfect and complete as in the normal joint, and in one case, that of Adolph Rousell (Figs. 16, 17, 18), the motion is greater in that joint than upon

Fig. 16. Fig. 17. Fig. 18.



the opposite side, and the limb less than a quarter of an inch shorter than the other, although it was sawn three inches below the top of the trochanter major, the head and neck having been entirely absorbed, and the acetabulum perforated. (Fig. 19. The four small pieces of bone are fragments from the acetabulum.) Only one of my exsections has recovered by anchylosis, and that was from neglect in the after-treatment, I

never having seen the patient after the operation for two years, and the gentleman who had it in charge having had no experience in the treatment of this class of cases. All the other patients that recovered have more or less good motion, and infinitely less deformity than those which have recovered by nature's process.

Fig. 19.

If the surgeon has not the opportunity of obtaining the wire cuirass, the after-treatment can be carried out just as successfully by applying extension and counter-extension, while the patient is in bed. But, of course, the patient loses the advantages of out-door exercise and fresh air, which in many instances will be found to be of vital importance.

From the foregoing observations, I am led to the following conclusions :—

I. That morbus coxarius is a disease most frequently met with in early childhood, or the age of reckless indifference.

II. That it is almost always of traumatic origin, and not necessarily connected with a vitiated constitution.

III. That rest and freedom from pressure of the parts involved, while at the same time the rest of the body is allowed free exercise in the open air, and a nutritious diet, is the best treatment that has yet been discovered for this disease.

IV. That if this plan of treatment is adopted in the early stages of this disease, the majority of cases will recover with nearly if not quite perfect motion, and without deformity.

V. That in the advanced second stage of the disease, when absorption of the effused fluid cannot be effected, then it is better to puncture or

aspirate the joint, and remove its contents, than to leave it to rupture by ulceration.

VI. That in the third stage of the disease, when the treatment recommended in this paper has been properly applied without satisfactory improvement, but progressive caries continues, then excision of the diseased bones is not only justifiable but in some cases absolutely necessary.

VII. That the operation of exsection of the hip is easily performed, and in itself attended with little or no danger.

VIII. That after exsection of the hip-joint, in cases of progressive caries, the recovery is much more rapid and certain, and infinitely more perfect as to form, motion, and the usefulness of the joint and limbs, than when left to the slow process of nature.

On motion, the Discussion on Dr. Sayre's paper was postponed until the next meeting of the Section, in order to afford the members an opportunity of witnessing a practical demonstration of the modes of treatment recommended, upon several patients who had been courteously placed by Dr. W. H. Pancoast at Dr. Sayre's disposal.

Accordingly, the next day, at 11 A. M., many members of the Section assembled in the surgical amphitheatre of the Philadelphia Hospital, when the first patient presented was a stout woman suffering from hip-disease in the *first* stage. She had been under treatment for some time in the hospital, and had been almost cured; but the splint had been removed too soon, and the disease had been re-developed. The night extension and short splint were reapplied by Professor Sayre in the manner described in his paper.

The second case exhibited was one in the first stage very far advanced. The patient presented the characteristic symptoms of this stage of the disease, but, as no instrument of a proper size was at hand, the case was referred for future treatment.

The third case presented was one in the stage of effusion, or the *second* stage. The patient was wholly unable to stand or bear the slightest pressure; weight extension in the line of deformity was applied.

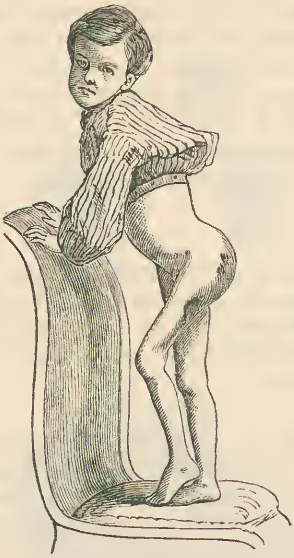
The fourth case was one in the *third* stage of the disease, and the following history was elicited: Thomas S., aged six, had been a healthy, active boy until about eighteen months ago, when he was knocked down and kicked in the region of the left hip. Soon after this he began to limp, but complained of no great amount of pain, that which he did experience being referred to the knee. During this time he was seen by several physicians, who did not discover any disease. He continued to become more and more lame, and at the end of six months a swelling made its appearance on the upper and outer part of the left thigh. He then began to complain of pain which was spasmodic in character, and always increased at night. The swelling was punctured, and, after poulticing for a few days, discharged a large quantity of pus. He has gradually lost flesh, and has become pale. This case was kindly furnished by Dr. J. M. Barton, clinical assistant to Professor Gross.

Present condition.—Extremely emaciated and anæmic; left thigh flexed, adducted across the middle of the opposite one, and fixed. (Fig. 20.) There is an opening in the outer part of the thigh, about the junction of the middle and upper thirds, which discharges considerably. The orifice has all the appearances of leading to dead bone, which is detected by the probe. A large abscess over the trochanter major is yet unopened.

Upon consultation with Mr. Adams, of London, Professor Lister, of Edinburgh, Professor Hueter, of Griefswald, Professor Hjort, of Christiania, Profes-

sor Hingston, of Montreal, Professor Post, of New York, and others, exsection of the head of the femur was decided upon, as affording the only chance of recovery.

Fig. 20.



While chloroform was administered by Dr. Sayre's son, Dr. Sayre said that his plan of using chloroform was entirely at variance with that taught in the books, and with the doctrine of most authorities, viz., that air must be inhaled with the anæsthetic. Air, he said, was the antidote to the anæsthetic, and as long as it was introduced anæsthesia would be prevented; he therefore carefully excluded all air not saturated with chloroform, and found that five, ten, fifteen, or twenty drops thus administered, according to the age of the patient, produced prompt anæsthesia, without that muscular resistance and contortion of the body which followed its administration mixed with air. If, by any possible contingency, this small quantity should produce dangerous or unpleasant symptoms, a few artificial respirations, effected by compressing the chest, would exhale the small quantity of poison, and thus avoid any fatal result. When chloroform was given in the usual way, *i. e.*, mixed with air, anæsthesia was not produced until a large quantity had been inhaled, in some cases many ounces, the patient during this time struggling violently; and the damage done to an inflamed joint by these struggles more than counterbalanced the good resulting from the anæsthetic. If, under these circumstances, failure of the heart or respiratory organs took place, the system was so saturated with the chloroform that resuscitation by artificial means was almost impossible.

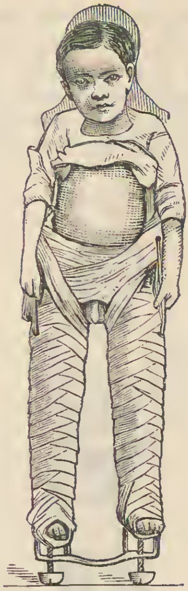
The administration of the drug in the present instance confirmed the correctness of Dr. Sayre's statements.

The operation was performed in the manner described in Dr. Sayre's paper, and the child placed in the wire cuirass (Fig. 21); great stress being laid upon the importance of careful dressing, after-treatment, and nursing. Dr. Sayre remarked, as he stood the child in the cuirass against the side of the amphitheatre, "now this child is ready for transportation."

Professor LISTER, of Edinburgh, being called upon by Professor Pancoast to address the company, spoke to the following effect:—We must all regard it as an extremely fortunate circumstance that Dr. Sayre has had at his disposal a series of cases so well adapted for illustrating his admirable diagnosis and

treatment of the various stages of hip-joint disease. As these subjects are to be brought before the Surgical Section of the Congress this afternoon, it is

Fig. 21.



needless for me to allude to them in detail on the present occasion, but I cannot refrain from expressing the admiration which I feel, in common I am sure with all present, at the mode of execution and the immediate results of the excision of the hip-joint which we have just witnessed. Dr. Sayre, indeed, has made no attempt at display, and he emphatically endorsed a remark of mine that the days of such exhibitions in surgery were over. For when pain and shock are prevented by anæsthetics, and when all loss of blood is avoided, whether by the use of a tourniquet, or, as in this instance, by the skilful manner in which the incisions are planned and carried out, mere rapidity of operating becomes a matter of absolute indifference, except that the work is probably somewhat better done if a little more time is spent upon it. Nevertheless, gentlemen, we have seen an extremely skilful performance, not only as regards the operation, but also in the application of the highly ingenious apparatus used for the after-treatment. The manner in which the sound leg, firmly fixed upon the cuirass, is made to serve as a perfectly efficient means of counter-extension, is indeed beautiful; and to see that boy who was but a few minutes ago a miserable crouching object, and on whom a capital operation has since been performed, standing erect before us upon this apparatus, with a smiling countenance, is as astonishing as it is delightful.

I feel that this demonstration would of itself have been a sufficient reward for my voyage across the Atlantic, and I beg you to join with me in a cordial vote of thanks to Dr. Sayre.

Professor Lister's suggestion was instantly seconded, and a resolution of thanks to Dr. Sayre unanimously adopted.

The following notes of the subsequent history of this case are furnished by Dr. D. J. Holland, Resident Physician at the Philadelphia Hospital:—There was not the least untoward symptom following the operation. The patient was given the pyrophosphate of iron and iodide of potassium, in small doses, three times daily, and for the first few nights a few drops of laudanum, as an ano-

dyne, though there was comparatively little pain. The limb was dressed every day, and a new tent placed in the wound; after the first month, passive motion was instituted. The patient's appetite gradually increased, and at no time had he any fever. His condition was excellent throughout, and within six weeks the wound had closed by granulation.

Two months after the operation, the patient was exhibited before the class at Bellevue Hospital as the most successful case upon record. There was at this time good motion. The cuirass was now removed, and Dr. Sayre's long splint substituted. Within a week the child became accustomed to the splint, and could walk without any exertion, and there was almost no shortening. He was discharged from the hospital, wearing the splint, November 11, 1876. (Fig. 22).

Fig. 22.

The following note has been received from Dr. J. M. Barton, under whose care the patient came again after his discharge from the hospital:—

Professor SAYRE:

Dear Sir:—I examined the boy Shields to-day, and found that there was five-eighths of an inch shortening; that the small of the back and the popliteal space readily came in contact at the same time with the hard table on which he was lying; that the foot could be raised eighteen inches before the pelvis moved; and that the limb readily passed a few degrees to each side of an imaginary line at right angles to a line connecting the anterior superior spinous processes, so that the boy could quite readily bring his limbs parallel, though he makes much more extended motion than that by moving his pelvis.

All the above motions, as well as pressure sufficient to move the boy upon the table, were totally painless. The limb rotated well; and the foot could be made to touch the table by its external edge, the boy lying upon his back, and could be rotated inwardly enough to touch the opposite foot.

There was about one fluidrachm of discharge in the twenty-four hours, and the boy's general health was excellent, and his appetite ravenous.

Respectfully,

J. M. BARTON,

201 S. Eleventh Street, Philadelphia.

Jan. 18, 1877.

Discussion on Dr. Sayre's Paper.

At the next meeting of the Section, the President having announced that the discussion of Dr. Sayre's paper was in order, Dr. Alfred C. Post, of New York, said:—In regard to the danger which attends excision of the upper extremity of the femur, I would say that the operation in early childhood is comparatively free from risk, but at later periods of life a very considerable proportion of fatal cases has occurred. Yet in many cases the danger of the operation is less than the danger of leaving the disease to pursue its own course. Even in some instances in which the operation is not necessary for the preservation of life, yet, if the case be allowed to go on to a natural cure, after having reached an advanced stage, such an amount of deformity will result as will render the limb an encumbrance, and in these cases the operation is of very great benefit to the patient.

Dr. S. D. Gross, of Philadelphia, said:—I came here this afternoon simply as a listener, but it strikes me that the second conclusion of Dr. Sayre's paper is entirely at variance with the received opinions of the profession, or at all events with my own experience. Many cases of coxalgia, according to my observations, have been cases in which it was impossible to trace as a cause anything like an injury. I have given special attention for many years to these cases of coxalgia: I have inquired "has the child received any injury—a blow, or a fall, or a contusion, or anything of the kind?" and in the great majority of instances the answer has been "no." I am quite certain that the majority of my cases—and they have been very numerous—have been of this character. That the disease may be developed or excited under the influence of traumatic causes is unquestionable; but I maintain that in the great majority of instances the affection is of spontaneous origin, and that it is not necessary for a blow, or a fall, or any such injury to produce the disease. This is my experience. In regard to the conclusion that it is not necessarily connected with a vitiated constitution, my teaching has certainly been greatly at fault if I err in this respect; I maintain, as the result of my dissections, that this affection cannot occur in a child or in any person whose constitution is not in a state of degradation, or who is not laboring under some constitutional taint. I believe that it is impossible for a person to contract consumption without a predisposition to it. So, in relation to hip-joint disease, consider, if you please, the nature of the suppuration which occurs in coxalgia. The surgeon finds an abscess, and puts his knife into the part; what is the character of the matter that issues? Manifestly the same kind of matter that is expectorated by a patient laboring under pulmonary consumption. This is beyond all question. I have never seen any other kind of matter escape.

This, I think, settles the question that the affection occurs naturally in persons predisposed to such diseases. It cannot arise in persons healthy in other respects. There must be a constitutional vice as a predisposing cause. The body must be in a state of debility or feebleness. This is my belief, nay, my solemn conviction. This degraded state of the system may arise from hereditary predisposition; the parent may have had scrofulous disease of the neck or spine, or some syphilitic taint; the predisposition has been transmitted, and the system has become degraded in consequence of the transmitted taint. The child has hip-joint disease, just as in other circumstances it might have had disease of the lungs, or tubercular phthisis, or disease of the spine. Indeed, we not unfrequently find more than one of these affections coexisting in different parts of the body.

The hip is affected in any particular case, simply because it is a weak point. The child may have taken cold, and the disease fixes itself in that locality. It might have fixed itself in the ankle-joint, or in the spine. The child may have received a blow, or a fall, or what not, but I maintain that that part is in a state of predisposition at the time when this takes place. I maintain that it is im-

possible for a child, born of healthy parents, well nourished, well taken care of, unless there is a previous predisposition or some degradation of the constitution, to have coxalgia, pulmonary consumption, or diseased spine.

Dr. Henry Fraser Campbell, of Augusta, Georgia, said:—In regard to the explanation of Dr. Gross, as to why the child has hip-joint disease instead of lung disease, I would say that scrofulous affections are developed in various forms at different ages. I agree that this disease is frequently produced by traumatic causes; I might go further and say that most cases which I have seen have been the result of some fall from the bed, or some carelessness of the nurse, or some twist of the child's limb or joint. But the same fall or twist, or the same carelessness, would not produce such a result in a healthy child, without any disposition to scrofula—in a child that had no syphilitic parentage. I recall one case in a very scrofulous child, in which an ordinary fracture of the arm resulted in the exfoliation of the entire bone. But this never would have occurred had that child not been of a scrofulous constitution. I believe that coxalgia may be one manifestation of scrofula, in childhood, just as at a later period of life tuberculosis of the lungs is another manifestation of the same general vice.

Dr. Wm. H. Hingston, of Montreal, said:—I formerly believed that scrofula or struma had everything to do with common coxalgia, but of late years I have come to think that it has little to do with that affection. Some years ago, in reading the papers of Dr. Sayre, Dr. Bauer, and others, I asked myself whether I was not in error in regard to the constitutional nature of this disease, and whether we were never wrong as to its treatment. In the large hospital at Montreal, the largest in Canada, to which I was surgeon for fifteen or sixteen years, I collected a number of cases which I published a few years ago—twenty-nine cases I think—and of those twenty-nine cases, I believe I could trace twenty-six to traumatic injury. And the treatment to which we have resorted is I think conclusive upon this point, differing as it does so much from the treatment formerly adopted. Without any constitutional treatment whatever, local measures suffice in a great number of cases. Again, we often see one child, out of half a dozen, having morbus coxarius, while the other children of the same father and mother are not affected with the disease. Now, is it the most sickly one that is affected? Not at all. In four cases out of five it is the healthiest child of the whole family. The child has a predisposition to the disease, but it is a predisposition to climb and to fall about. The sickly child rarely has hip-disease. I believe also that the affection is more common among boys than among girls, because they climb more; and it is more common also at that period at which children are cut loose from their mother's apron-strings, and are allowed to run about. As soon as they acquire sufficient intelligence to take care of themselves, the disease becomes rare; it is only in the period between infancy and adolescence that it is frequent.

Dr. E. M. Moore, of Rochester, said:—Like Dr. Gross, I have often asked whether or not there has been any injury, and I have largely failed to find any recollection of such an occurrence to which the disease might be attributed. But then we all know that these little bodies are constantly tumbling around, falling and straining themselves, and I have asked myself how is it that this particular joint, and no other joint, should be the one affected? It seems to me that the explanation of the fact has not yet been given. In the hip-joint we have a surface, small as compared with the ankle-joint, and very little larger than the shoulder or elbow-joint; and yet at this particular point in the body we have an immense mass of muscles surrounding the whole, and these huge muscles take hold of the bone whenever there is an injury inflicted, as they do in any other locality. So we have a reflex action set up in these cases, these muscles taking a sort of tetanic hold, night and day, to steady the joint. Why? Because nature is determined to secure rest, which is the cardinal cure of everything. So these muscles hold the bone up, and the surface is so pressed upon that whenever there is the slightest degree of inflammation, it must continue. Day by day

it will increase, while the child has forgotten the fall, until perhaps a deep-seated abscess is formed in the tissues. Dr. Gross asked whether we ever saw anything not like a scrofulous discharge from these abscesses? I answer, No; and the reason is that they are chronic abscesses, but there may not be the slightest evidence of scrofulous disease. In the same way we were formerly taught that Pott's disease originated from scrofula, but I have seen it traced directly to falls in cases in which no scrofula could be found for three or four generations backward. Again, we are told that these diseases originate from a deposit of tubercle, and the mere fact of finding tuberculous disease in connection with these affections is received as affording proof of their origin. But the connection may be reversed. It is what our medical friends are every day coming more and more to do. If there is no consumption but such as comes from hereditary taint, I ask why men who pick mill-stones never live above forty years. They do not expect a longer duration of life. I had one man, with a family dependent upon him, to tell me that he did not expect to live beyond forty years, but he was more fortunate; he lived to forty-five years, and then died of consumption. The grinders of needles and axes die of consumption. But is it of hereditary taint? It is because the particles of steel get into their lungs, and then produce chronic inflammation. These facts force themselves upon us. We all appreciate and understand perfectly well the force of hereditary taint, but it is unsafe to say that such is always the cause of tuberculous disease. I have myself become thoroughly convinced that a large number of cases of consumption can be traced to disease of cancellated bone. I recall one case of a young man of nineteen years who had inflammation of the tarsus, and a year afterwards presented evidences of tuberculous disease. I could not with the utmost care trace any consumption in that man's family. I am firmly convinced that a large number of cases of Pott's disease, which are followed by consumption, come from inflammation of the bone, and not from hereditary taint; because a large number of cases of the same affection which are cured can be traced to injury.

Dr. George Cupples, of San Antonio, Texas, said:—In regard to the question, why the hip-joint is more often affected than any other, I would say that perhaps a physiological fact may throw some light on the subject; this is the circumstance that the long bone of the body, the femur, is the last to be completely ossified.

Dr. T. G. Richardson, of New Orleans, said:—I would like the positive advocates of the local origin of coxalgia to explain why a severe injury in an adult is never followed by hip-disease. You cannot mention a case of the kind which has been followed by this affection. Then, too, I deny that the age of childhood, or the age of carelessness, is the age at which the hip-joint is most liable to injury, as is shown by the fact that we seldom or never have dislocation of this joint in childhood. Then, again, I call on those who maintain that coxalgia originates from traumatic causes to explain why it is that in cases of congenital dislocation of the femur we sometimes find hip-disease.

Mr. William Adams, of London, said:—I am satisfied that, in a large proportion of cases, it will be found that the exciting cause of hip-disease has been some slight injury; yet years of practice have taught me that there are cases in which the disease occurs without any injury. We used to be taught that hip-joint disease often originated as a bone disease, but my own belief is that it is usually the result of a ligamentous lesion. We have very few post-mortem examinations of cases in the first stage of hip-disease to refer to, but, in those which are reported, we find that an inflammatory condition of the ligamentum teres and a slight exudation into the synovial membrane were noticed;[1] and I believe that, in seeking the cause of the

[1] [See report of post-mortem examinations, made by Aston Key and by Mr. Adams himself, in Chelius's System of Surgery, edited by South and Norris, Philadelphia, 1847, vol. i. p. 290.—Editor.]

affection, we may look to an injury or wound of the round ligament, as from some strain, or twist, or jump, when the muscles of the child are in a relaxed condition. The injury, however slight, causes subsequent inflammation, the extent of the disease depending much on the constitutional condition of the patient. This inflammation may lead on to secondary bone disease, or, in other cases, and in particular states of the constitution, we may have primary bone disease.

Dr. D. Hayes Agnew, of Philadelphia, said:—It is important in discussing this subject to understand what we mean by the term traumatic injury, as preliminary to the whole question. Now, if it is necessary that a joint should have a powerful twist, or wrench, or that a blow should be inflicted upon it, or that the patient should be projected from some height—that is one thing; but, if it is meant that a child by jumping down, for instance, two or three steps, or by tripping on the floor in walking over a carpet, can in such a way receive traumatic injury—that is another thing. My impression in regard to hip-joint disease is this: It is impossible to follow a child in all its movements. Most of the time the child is out of the notice of its mother, in the care of nurses, many of whom are exceedingly careless. The child may receive a great many slight injuries which may yet be quite sufficient to produce such a contusion or such an irritation of the joint as, in a child predisposed (for I must confess that there is after all a predisposition, which may remain latent if the child be kept absolutely at rest), may kindle up the slumbering elements and bring on the disease. I see a great many of these cases, and I must say that I usually find it not difficult to trace behind all a constitutional predisposition. In the family, perhaps, one child may have phthisis, and another enlarged glands, and another hip-joint disease, and another knee-joint disease. But, in most cases, the immediate cause of the disease is some external injury of the joint.

Dr. Sayre said:—Both Dr. Gross and Dr. Campbell agree that some little injury may produce the disease. That is all that I claim; I do not assert that it is necessary for a child to be run over by a railroad car. If coxalgia is constitutional, I would ask why it is that, having recovered from hip-joint disease, the patient becomes perfectly well, and sound, and hearty? why is it that simply having hip-joint disease cures the constitutional taint?

Dr. Gross said:—The difference between Dr. Sayre and myself seems to be simply this: He says that hip-disease is almost always of traumatic origin, and not necessarily connected with a vitiated constitution. I, and those who think with me, do not deny that injury may excite the disease; on the contrary, we confess that it frequently does so; but we maintain that the disease is always necessarily connected with a vitiated condition of the system.

Dr. J. A. Grant, of Ottawa, said:—I must acknowledge that for many years I entertained similar opinions to those now expressed by Dr. Gross; but in one or two cases observed during the last few years, in the hospital at Ottawa, where I see many cases of hip-disease, I have been forced to a different conclusion. One case came under my observation about three years ago, which I investigated most closely, and in which I failed to trace the slightest predisposition of a scrofulous character, either in the boy, or in either of his parents, or in any of his ancestors. In another case that came under my notice, the disease arose purely from irritation or excitement in the joint, entirely independent of scrofulous complication. We must acknowledge, of course, that persons laboring under a scrofulous taint are much more liable to irritation, and to the development of this disease, than others; but we must also acknowledge, from the peculiar construction of the hip-joint, that it is one extremely susceptible to irritation, whether in a scrofulous constitution or in a constitution not of scrofulous character. I am of those who adhere to the view that we may have coxalgia occurring in an individual entirely independently of scrofulous taint.

Dr. J. H. Pooley, of Columbus, Ohio, said:—I will go further than Dr. Sayre's proposition, and say that I believe the disease is not only not necessarily connected with a strumous diathesis, but never connected with it except by the accident of coexistence. What is scrofula? Take all the gentlemen in this room, one by one, and make each man give his description of scrofula, and you will have as many separate descriptions as there are separate describers. We hear a great deal about scrofula, and hardly know what we mean. I not only endorse the proposition of Dr. Sayre, but I would have framed it in stronger language.

Dr. Hunter McGuire, of Richmond, Va., said:—I think I can reconcile in one or two words the different views expressed in regard to the occurrence of scrofulous pus in hip-joint disease. I cannot believe that anybody here ever saw any other kind of pus in coxalgia than scrofulous pus, but it does not necessarily follow that coxalgia is of strumous origin. I was taught five and twenty years ago, and I think I have profited by the teaching since, that scrofula resulted very frequently from pain or irritation, or from the privations of life, such as insufficiency of food and clothing; so that, if a little child gets a fall, rapid coxalgia may follow, and then, in consequence of the pain and loss of appetite, struma may be developed, and there will then be scrofulous pus.

Dr. John T. Carpenter, of Pottsville, Pa., said:—The point at issue, it seems to me, is the local origin of constitutional diseases, upon which Professor Niemeyer has laid so much stress. I published a paper recently, in the Transactions of the Pennsylvania State Medical Society, in regard to the diseases of miners. Miners are subject to phthisis, and rarely live over 45 years. That disease, which we always believed to be constitutional, is caused by nothing but the inhalation of coal dust. We find solid carbon in the patients' lungs. The men die, and the old women live in the villages. Now, if their sons leave the mines and go into the agricultural districts, they live as long as other people. Here is a local origin of constitutional disease, and so it is with coxalgia. Every one of us knows that the first sign of constitutional disease in cases of coxalgia is an irritative fever, and thereafter sinuses form, and there is a discharge of pus, and phthisis may follow. The struma does not exist in the case until after long suffering and long disease. It is the consequence, and not the cause. Children whom I have treated by Dr. Sayre's method are now healthy and happy, and have never had any phthisis, or scrofula, or anything of that kind.

Dr. William Brodie, of Detroit, said:—Some years ago, when Dr. Sayre's views were first promulgated, I had occasion to see a great many cases of this disease in Detroit, and, with my friend Dr. Pitcher, had occasion to examine a great many people who came in from the surrounding country, all supposing that they were going to be cured by the new method. Some of these patients died of their coxalgia, and some from other causes. A great many post-mortem examinations were made, and I came to the conclusion that the disease commenced in the cartilage, and that, when this was absorbed, the bone itself became affected. I noticed in these cases the presence of a low grade of constitution, as if the patients had suffered from typhus, or were in an impoverished condition from cold. We know that children frequently lie exposed at night, or get into water, or get chilled in other ways, and then the external surface is affected, and a low grade of inflammation produced, which results in suppuration of the cartilage. I have always considered hip disease as scrofulous in its nature, but I believe that it originates in inflammation of the cartilage.

The President, Mr. Joseph Lister, of Edinburgh, said:—The question whether this disease is or is not constitutional, must not be allowed to determine absolutely the treatment to be adopted. We must not say that because a disease is constitutional, that it is therefore hopeless to produce a permanent cure by treating it with local means. Take cancer, for example; that may be

in many cases constitutional, yet we know also of many cases of cancer which recover if operated upon early enough. There is a local manifestation of a hereditary taint, but if we take away the local manifestation, there may never be any other. This is the case also with struma. Glandular abscesses in the neck are strumous, and yet how many persons live to be healthy and sound after having these! It seems to me that if we have such a disease as struma at all, we have that disease in morbus coxarius. A child came to me in Glasgow to be treated for disease of the tarsus. He was treated with a long splint, and after a while, though still lying in bed, with the splint, several weeks afterwards, morbus coxarius showed itself. In that child there was a constitutional tendency which developed itself without traumatic cause. But I admit that traumatic causes are frequently operative, and we know the constant liability of children to be affected by traumatic causes.

Dr. Gross said:—Have you ever seen suppuration in disease of the hip-joint, in which there was not scrofulus pus?

Mr. Lister said:—I must confess that I have. In affections dependent upon strumous disease, there are great varieties of pus. The treatment which I have adopted has been, if there has been flexion of the limb, to extend it on a long splint, and keep the patient at rest. In the great majority of these cases in Edinburgh, perfect cures are obtained. In Glasgow, a considerable number of the cases are cured, but not so many as in Edinburgh. Then, again, in Manchester the number of cures is a minority. Now I believe the reason for this is that Edinburgh is a more healthy place than Glasgow, and that Manchester is a less healthy place than Glasgow.

Dr. Sayre said:—I can hardly find language to express my feelings of regret whenever I am compelled to differ in a professional point of view from my distinguished friend Dr. Gross. But we must not let personal friendship, or personal respect and veneration, guide us in the expression of our opinions upon points of science. It seems to be conceded that an injury is often the exciting cause of the disease. Now, with regard to the matter of predisposition, I say that the ordinary teaching has been erroneous, and I believe that the doctrine has led to bad results in treatment. If the disease is necessarily of constitutional origin, it cannot be cured by local means. You must get rid of the constitutional poison or taint. If it is of constitutional origin, the development of some constitutional cause, the treatment ought to be constitutional, to affect the blood and the whole body. What has been the result of the belief in the constitutional origin of hip-disease? The use of internal remedies to correct the constitutional taint, and of local applications to the parts that are simply irritating. Dr. Gross has referred to the character of the pus: I happened to be present at an autopsy in a case of hip-disease in Berlin, made by Prof. Virchow, where an examination was made of the lungs, and heart, and intestines. In the specimens taken from that case there was not a trace of tubercular matter to be found. It was simply a case of pure chronic coxalgia. The pus was not of the kind referred to by Dr. Gross. Had that case been carefully examined and properly treated, in my judgment it would not have terminated fatally. The trouble was a local one, and the constitutional effects of the local trouble were mistaken for its constitutional origin. Prof. Gross will remember seeing a little child whose hip I exsected in Brooklyn. If there ever was a strumous condition, that child's condition was certainly entitled to be called so. In fact the child was almost dead at the time of the operation. It had lain in an exhausted condition for five or six years, with continued suppuration, yet by removing the cause of the trouble, without any constitutional treatment, or medicine of any sort or kind except something to eat and fresh air, from being a dead child, the child is running about to-day in perfect health. I sent its picture as a Christmas present to Dr. Gross himself. Now, in reply to the remarks of Dr. Richardson about cases of congenital dislocation, I may say that congenital dislocation has never been seen, although it has been described by Dr. Carnochan and others, and I believe that I am justified

in saying that the term is a misnomer. There is no congenital dislocation, but an arrest of the development of the acetabulum, the bone having never been completed. It is a congenital *displacement*, from an arrest of development, but not a congenital *dislocation*. The dislocations that occur in the adult are immediately recognized and attended to, and after reduction the parts are kept at rest until all danger of inflammation has passed; whereas the slight injuries which cause hip disease in the child are overlooked, and attention is not called to them until, by continued irritation, inflammation has supervened; and the general health frequently becomes involved before the slight local injury is recognized. Moreover, in the adult the head of the femur is much less vascular than in the child.

Dr. Hingston said:—We are told that hip-disease may originate from injuries to the cartilage and bone, and also from ruptures or injuries of the ligamentum teres. Now, I am wholly prepared to admit that in the first set of cases local treatment would probably suffice to effect a cure, but I wish to know if the same degree of probability applies also to cases originating in lesions of the ligamentum teres?

Dr. Sayre said:—I can answer that question by a practical illustration. Five or six years ago a case of coxalgia was brought to me by Dr. Jourdon for excision. There was an open abscess, which had gone through all the various stages, the disease having originated from a blow two and a half years before. At the time I saw the case the child was hardly in a condition in which it was deemed advisable to operate, and so I thought it better, in order to save the child, to get it in a better condition. In the mean time, to make the child as comfortable as possible, and put it in a condition for treatment, I brought it out of its surroundings, and got it out of doors to build it up a little. When the time came for the operation, the child was so much better that I concluded to give it a chance without excision. I made a free incision into the joint, and took away a comparatively small portion of the bone only. Repair took place; the child got entirely well; and ran around a year and a half without any splint, and with only slight deformity, and with a tolerable degree of motion. Some year and a half ago, through exposure, the child got into a very bad condition, and the physician in attendance informed Dr. Jourdon that it was going to die. The child did die, and I have the specimen. The head of the femur and the ligamentum teres have entirely disappeared; a new covering had been formed, of what I do not know, but it was smooth, hard, and cartilaginous. So that that child lived two or three years without any ligamentum teres at all.

Dr. Post said:—With regard to the necessity of resorting to excision, we all have known of advanced cases of the disease in which there has been a favorable result. I was called, two or three years ago, to see a patient in Jersey City. The knee was thrown up almost to the chin. It was impossible to place the child in a position even approximately straight, in consequence of the extreme pain which the effort gave him. He was almost ready to die from the constitutional disturbance attending the advanced stage of the disease. He was placed under the influence of ether, and the limb brought into as straight a position as possible. It was during cold weather, when there was a fire in the grate, and I put a poker in the fire, and made a thorough application of it behind the great trochanter. Improvement began from that time; and some time afterwards the child was brought to me free from deformity. Here was an entire recovery, and this case goes to show that excision is not always absolutely necessary.

Dr. Sayre said:—I have only advised the operation in cases in which the disease progresses in spite of proper treatment.

Mr. Lister said:—I think that a distinction between the stage of effusion, and the stage of suppuration, would be more in accordance with the general pathology of the subject. I believe the last stage of hip-joint disease is invariably the stage in which suppuration is present. Where effusion exists

without suppuration, a permanent cure may result by drawing off the fluid. If the case has gone on to the condition in which sinuses present themselves, I should be prepared to endorse the proposition that exeision is the proper treatment; but if there is suppuration without an external opening, I am bound to express my strong conviction in favor of merely opening the abseess. I know of numerous eases in which the abscesses have been treated antiseptieally, and in which the patients have recovered. Certainly in these eases the treatment had better results than if excision had been resorted to. I remember one patient, a healthy young woman in Glasgow, whose two limbs beeame exaetly alike, exeept that there was the mark of the small incision made in opening the abscess; and this result was a great deal better than if excision had been adopted.

Dr. HINGSTON said:—I wish to ask Dr. Sayre if he ean tell from the position of the sinuses, whether the aeetabulum or the head of the bone is diseased?

Dr. SAYRE said:—I eannot; the pus will, in either ease, gravitate in the easiest direction.

On motion, the conclusions of Dr. Sayre's paper, with the exception of the seeond, were adopted as expressing the opinion of the Section. With the seeond conclusion, the Seetion could not unanimously agree.

REPORT OF A CASE OF SUB-PERIOSTEAL EXCISION AND DISARTICULATION OF THE ENTIRE INFERIOR MAXILLARY BONE FOR PHOSPHORUS NECROSIS.

BY

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THE record of the case I have the honor of presenting was intended to be incorporated in a paper on extirpation of the entire inferior maxillary bone, which I have in course of preparation, and in which I am desirous of including as many as possible of the illustrative cases contributed by surgeons of different parts of the United States and of foreign countries. I venture to report it now in order to recall attention to the subject, and thereby elicit information regarding cases, the notes of which I have not as yet been able to obtain, and also to gain access to unpublished cases.

The following is a list of names of surgeons whose cases have appeared in divers journals and surgical works, or have been communicated to me by the operators. The operations were done for tumors, as well as for necrosis; some of them were intra-buccal, but the majority were by external incision, at one or at two sittings, by one or, as in three instances, by two different surgeons at a considerable interval of time.

Name of Operator.	Residence.	No. of Cases.
Valentine Mott,	New York,	1
John G. Perry,	England,	1
James Syme,	Edinburgh,	1
Signoroni,	Padua,	1
J. F. Heyfelder,	St. Petersburgh,	3
Ackley,	Cleveland,	1
J. M. Carnochan,	New York,	1
Maisonneuve,	Paris,	2
J. R. Wood,	New York,	1
G. C. Blackman,	Cincinnati,	1
J. H. Pooley, senior,	New York,	1
Pitha,	Prague,	1
Borelli,	Turin,	1
Langenbeck,	Berlin,	1
Holmes Coote,	London,	1
Rizzoli,	Bologna,	1
John Adams,	London,	1
Thomas Smith,	London,	1
March,	Albany,	1
A. B. Mott,	New York,	2
Boker,	Philadelphia,	1
Weir,	New York,	1
J. J. Hull,	New York,	1
Trélat,	Paris,	2
Obalinski,	Cracow,	1
Miner,	New York,	1
Carnochan and Pancoast,	New York and Philadelphia,	1

Name of Operator.	Residence.	No. of Cases.
Enos and Cochrane,	Brooklyn,	1
Hodgen and Mudd,	St. Louis,	1
Willard Parker,	New York,	1
J. Mason Warren,	Boston,	2
Maunder,	London,	1
Westmoreland,	Atlanta,	1

Dr. Blackman (American Journal of Medical Sciences, October, 1856) mentions Walther, of Bonn, Marsh, of Cincinnati, Hutton, of Dublin, Gauwesky, of Westphalia, Cusack, of Dublin, and Liston, of London, as having each once excised the entire inferior maxillary bone, but I have not as yet found the original accounts of their cases.[1]

The subjoined notes of my case embrace the principal points I am desirous of obtaining from surgeons whose cases have not been published.

Mary N——, aged nineteen, came to this country from Ireland when nine years old. At the age of thirteen she was employed in a lucifer-match factory, where she remained for more than two years. Within a few months after entering the factory she had a decayed right inferior molar tooth extracted, and resumed work on the following day. But this does not appear to have caused any trouble. A year and a half afterwards another inferior molar was removed, also from the right side, and as before she at once went to work. Very soon after this she was attacked with phlegmonous periostitis, accompanied by inordinate swelling, which rapidly extended and involved the whole jaw. The abscess opened spontaneously at a number of points along the base of the jaw, on both sides of the face, and also in the buccal cavity. She lost successively the molars of the right side and the two left incisors. The fetid pus which continually issued from these various fistulous openings was so profuse as to greatly drain her vital powers. Her appetite and strength soon failed; she became anæmic; and her condition was worsening so steadily that, at length, she applied for treatment to a surgeon who made an incision along the right ramus, and removed a sequestrum which afterwards proved to be the coronoid process. This operation was of little use, as only a very small part of the dead bone was removed.

I found the patient at St. Vincent's Hospital, early in March, 1864, still further reduced in strength by her great suffering and by the profuse discharge already referred to. The right side of the face was paralyzed from division of the facial nerve at the time the incision was made to remove the coronoid process. The jaw was extremely rigid and mastication impossible. For nearly a year the patient had lived entirely on fluid food, and with it always swallowed a considerable amount of pus. All the sinuses led to dead bone. She was much disfigured by an enormous under jaw, made up of an unusually large involucrum with the overlying soft parts greatly swollen. On consultation with my colleagues, I proposed to remove the whole of the involucrum, as well as its contents, by external incision, as it did not seem expedient to attempt to save such an unwieldy bony mass, and because it was believed that its extirpation would not only remedy the existing deformity and distress, but also greatly facilitate the ingestion of proper food. To this proposition they assented, and, on March 19, 1864, I proceeded to operate after the manner described below.

The patient having been etherized and placed on her back, with the head

[1] I shall be greatly indebted to those who will have the kindness to indicate to me in writing the operators whose names may have been omitted, and to furnish me with facts regarding such unpublished cases as may have come to their knowledge. Each case must be one of disarticulation of both condyles, and ablation of the whole body of the bone, whether done at one or more sittings, by one or more surgeons. Communications relating to the information solicited may be addressed to 311 Madison Avenue, New York.

elevated, an incision, including only the thickness of the skin and superficial fascia, was begun in front of the left ear, carried downwards along the posterior border of the ramus to the angle, thence close under the body of the jaw, and continued around until it reached the right ear, at a spot corresponding to its starting-point, so that, in a front view, nearly the whole line of incision was out of sight. The left facial artery and vein were soon exposed, tied above and below, and divided between the ligatures. The dissection was then carried to the bone, and the periosteum separated with a blunt steel knife, beginning at about an inch to the right of the median line, under the chin, and extending up to the coronoid and condyloid processes. To avoid its sudden retraction, the tongue was secured by a strong ligature, passed vertically through its substance in the median line near the apex, and confided to the care of an assistant. The periosteum of the buccal surface of the jaw was detached from below, as it could not be reached through the mouth on account of the rigidity of the parts. The whole left side having been so far enucleated, a chain saw was passed into the gap between the right central incisor and left canine, and the bone sawn through a little to the left of the symphysis. Then, seizing this partially liberated segment with the left hand, and depressing and rotating it outward, the temporal and external pterygoid muscles were made tense, and successively divided with the scalpel close to their insertions. Further use of the knife was rendered unnecessary by the previous destruction, by disease, of the capsular and adjoining ligaments, and the bone being free was laid aside. The right segment was detached from its position by a process similar to that described above, but the capsular and other ligaments, which were intact, were divided with the scalpel from before backwards. The vessels, on either side, requiring ligatures were the facial and their submental and masseteric branches, the pterygoidean, and the inferior dental.

The lips of this extensive wound were brought together by six fine silken sutures, on each side, and a few strips of adhesive plaster; and the ends of the ligature which held the tongue were brought out, one at each corner of the mouth, and secured to the cheeks by adhesive plaster.

Before the patient recovered from the effect of the anæsthetic, a dose of ten minims of Magendie's solution of morphia was administered hypodermically, and this was repeated twice daily for five days, during which time she lay in a somnolent state and had to be roused to be fed. Her diet consisted of beef-tea and milk-punch. She said afterwards that during those five days she had experienced no pain whatever.

The greater part of the wound healed primarily, and the sutures and the ligature confining the tongue were removed on the third day. Four weeks after the operation, a slight facial erysipelatous blush appeared, but soon subsided under the application of evaporating lotions and the internal administration of chloride of iron. In May, 1864, the patient was discharged from the hospital, the remainder of the wound (at the seat of the cloacæ) having nearly healed.

The patient called upon me on June 5, 1865, feeling quite well and strong. Her appetite was excellent, her food consisting chiefly of hashed meats and potatoes, eggs, bread and butter, and cream. She had gained very much in weight, and her condition was all that could be desired under the circumstances. I last saw her in September, 1871, seven and a half years after the operation, when she was in excellent health, and when I took the opportunity, as I had done before, of making a careful examination of the parts with a view of ascertaining the extent of bone production, which I found to consist of a thin rim or plate including the mental portion, and the body and part of the ramus on either side, all of which existed in June, 1865.

The pathological description of the parts removed will be given elsewhere. I have lately learned that the patient was in 1874 seized with variola, from which she died, and that no autopsy was made.

It is unusual, in cases of phosphorus necrosis, to find an involucrum, but in the case just reported there was a large one, and, on the other hand, very little of the "pumice-stone deposit" so frequently seen. This is probably to be accounted for by the existence of violent phlegmonous periostitis in the preceding year.

Another point to which I wish to call attention is the method of making the incision. Various lines of external incision have been suggested from time to time, but I wish particularly to elicit the opinion of the Section in regard to the intrabuccal operation, which, as applied to the entire bone, is said to have originated with Signoroni, in 1843.

It is generally considered safer to divide the operation of extirpating the lower jaw into two sittings, removing one-half at a time; but Heyfelder advises that the whole operation should be done at once. Nearly all the cases included in my table were operated on at different sittings, the intervals in some instances having been as long as six months. I have, however, tabulated them as single operations, as the removal of the second half of the bone is but the completion of a previously unfinished proceeding.

Discussion on Dr. Gouley's Paper.

After the reading of the preceding paper, Dr. J. C. Hutchison, of Brooklyn, said:—I think that the intrabuccal form of incision is an unsafe one for the removal of malignant tumors, since there would then be a greater probability of leaving behind some portion of the diseased tissue, but for the removal of sequestra, or even of the bone itself, I consider it perfectly feasible. It is even more applicable to the upper than to the lower jaw, as external incisions in the region of the superior, are more difficult to conceal than those in the region of the inferior, maxillary bone. I have myself, by this plan, broken up and removed the whole upper jaw, together with the malar bone and even the orbital plate.

Dr. Frederick Hyde, of Cortland Village, N. Y., said:—The circumstances of the individual case, the location of sinuses, etc., should determine the form of incision. For the removal of sequestra I usually employ the intrabuccal operation, but for other cases I make such incisions as seem best suited to the accomplishment of the object.

The President, Mr. Lister, of Edinburgh, said:—I was yesterday shown a specimen, in the Museum of the Pennsylvania Hospital, from a case in which more than half of the jaw had been removed by Dr. Hunt, from within the mouth; and I was informed that the patient had recovered with very slight deformity. I do not think that the intrabuccal incision is permissible in malignant disease, but for other tumors it may be practised when the circumstances of the case admit of its use. An external incision, however, gives the operator much more freedom for both sight and manipulation, and, especially in the male, I should prefer its employment in the majority of cases in which the entire jaw was to be removed; even in females the scar can be partially concealed behind the ramus and beneath the base of the jaw. The method of Maisonneuve, which consists in making the incision further forward and thus avoiding the facial nerves, is also a good one, and I have recently removed half of the jaw by a median incision, the resulting scar being but slight.

THE CAUSES AND GEOGRAPHICAL DISTRIBUTION OF CALCULOUS DISEASES.

BY

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In investigating the etiology of calculous affections, it will be necessary briefly to examine the composition of the secretion in which they take their origin, to see what is the normal condition of the fluid, so that we may observe any changes which take place in its departure from a standard of health. In this research, we must consider the relation between healthy or normal urine, and urine which has been changed by blood alterations; we must observe the causes which produce those changes, and then decide upon the probable causes which induce the formation of urinary calculi.

Healthy urine is composed of certain constituents, water being the menstruum which holds in solution the organic products of digestion and assimilation. First among those ingredients characteristic of healthy urine, which are furnished from destructive assimilation of the tissues and separated from the blood by the liver and the kidneys, are, urea, uric acid, creatine, coloring matter, and odorous principles. Secondly, and derived chiefly from the food during the process of assimilation, are, hippuric and lactic acids, together with certain accidental constituents, and also some inorganic products—saline combinations which have been separated from the blood, such as the sulphates, the phosphates, and the chlorides, of which the bases are respectively soda, potassa, lime, and magnesia; soluble salts which have been introduced into the economy along with the food, and have undergone decomposition in the system, or such as have entered into new combinations during the process of destructive assimilation, as is especially the case with the sulphates and the phosphates. In addition to these, we find certain ingredients derived from the lining membrane of the urinary passages, as the débris of epithelium, from the kidneys; mucus, from the bladder; and phosphate of lime, the result of the decomposition of mucus and pus in cases of cystic inflammation.

We know that, during health, the composition of the blood is continually changing by the introduction of new matter through the process of digestion, the *primary* assimilation of the food; and that it is constantly being altered by the presence of waste or effete material, the result of the breaking down of the tissues in the course of nutrition, or *secondary* assimilation. Then we find constant changes going on, the result of excretion by the liver, the skin, and the kidneys; changes which control and govern the healthy condition of the urine, and any departure from which produces certain pathological alterations, which are either *general* or *special*. We also know that, in disease, the blood is constantly undergoing change in its component parts, receiving new material of a morbid character through the process of either primary or

secondary mal-assimilation, or through the co-operation of both; the first representing the process of deficient digestion, the latter of faulty nutrition. The blood is necessarily changed when these morbid matters are not properly eliminated, as is the case when we have mal-excretion by the kidney, liver, skin, or other secreting and excreting organs.

Now, as *general* pathological changes depend upon some general systemic disorder, as, for example, fever, rapid waste of tissue, anæmia, etc.; so also *special* changes depend upon some special unnatural ingredient present in the circulation and showing itself in the urine, as, for instance, albumen or sugar; or some natural constituent may be present in unnatural proportion, and thus form a leading feature in disease; or again this natural constituent may become so altered in its physical condition as to produce a particular morbid state, as when uric acid in combination with soda gives rise to gout, or when uric acid, oxalate of lime, earthy salts, and such materials, produce calculous concretions.

Whenever we have mal-assimilation in the process of nutrition, one of its consequences is what Liebig has termed "destructive metamorphosis" of the tissues: and, as the various tissues are composed of certain chemical constituents, we have as the result of their disintegration particular chemical agents which bear upon and constitute a diathesis. For example, when those textures which are rich in albuminous material are broken down, they furnish an amount of lithic or uric acid which surcharges the blood, and gives us the expression, "a gouty diathesis." Like changes taking place in the destructive metamorphosis of highly nitrogenous tissues, as the muscular structures, or even where obstinate constipation has locked up in the alimentary canal the excrementitous residue of ill-digested food, urea is formed too rapidly for elimination, and it is also found in excess in the urine. Then, again, when the brain and nervous system become overtaxed from any cause, a consequent prostration follows, and, abounding as they do in phosphoric acid, the result of this destructive metamorphosis, so to speak, is an excess of the phosphates, and we have what is known as the "phosphatic diathesis." Here we find the débris of the nervous system adding an undue proportion of phosphoric acid to the blood, a combination resulting with its alkaline and earthy bases, and, as a consequence, a deposition of the phosphates in the urine. In certain conditions of the bladder, as in cystitis of long duration, or in enlargements of the prostate where a change has taken place in the mucous membrane, these salts become precipitated, and phosphatic calculi result; injuries to the spine, from which paralysis follows, produce similar results.

Following this course of investigation, we see that when there has been a destructive metamorphosis of the gelatinous tissues, there is a conversion into oxalic acid, which, entering the circulation, gives an "oxalic acid diathesis," with a tendency to deposit of oxalic concretions. In like manner, we could trace the formation of sulphuric, hippuric, and lactic acids, and show that a destructive metamorphosis or transformation of the tissues in general will produce the chlorides.

We also observe that, during the process of digestion, changes take place in *primary* mal-assimilation, by which certain constituents enter the blood in the same manner that they do during the process of *secondary* mal-assimilation, or destructive metamorphosis of tissues. These changes are referable to the stomach, duodenum, and chyliferous organs, and thus it is that we find lactic or uric acid the result of imperfect digestion of albuminous food, urea following an excess of nitrogenous material, or

the phosphates resulting from an undue proportion of alkaline ingesta uniting as a base with the free phosphoric acid contained in other articles of food which were eaten at the same time. Then, again, we find that certain articles of food supply certain chemical constituents which enter the circulation during the process of digestion; for example, oxalic acid is derived from special articles of diet, as sorrel, rhubarb, tomatoes, and similar articles, and this acid, acting on the lime contained in hard water which may be drunk at the same time, furnishes the oxalate of lime, one of the varieties of urinary calculi. In the same way an excess of sulphuric, lactic, and hippuric acids, is formed from articles of food in which they respectively abound.

Without pursuing this line of thought or reasoning further, we now turn to the process of mal-excretion, and perceive that certain structural or functional alterations of either the kidney, liver, or skin (the great depurating emunctories of the system) may interfere with their healthy action, and, through a faulty process of elimination, cause an excess of the urinary constituents to remain in the blood, thus constituting a diathesis referable to the particular ingredient which may predominate. Where uric acid is the morbid product, we have a blood change giving the expression of a uric acid or gouty diathesis, which may under certain circumstances develop a gout, and under others precipitate urinous salts in the pelvis of the kidney, and give rise to sand or gravel. For during the process of elimination, uric or lithic acid is separated from the blood in combination with a base, as soda or ammonia; the former necessary for the production of gout, the latter for the formation of gravel; and as both of them are soluble in the urine only when they are not in excess, and when the urine is at the normal temperature of the body, it follows that they must become precipitated whenever they exist in abnormal quantities, and whenever the urine is changed in temperature.

If from any cause we have a structural alteration of the kidney by which the proper excretion of these substances is prevented, they must as a consequence be either retained in the blood, or stagnate, so to speak, in the tubules of the kidney, and thus give us the disease of that organ, as a cause for the effect. In the same manner, the skin, acting as a great depurating organ, eliminating effete material from the blood, becomes by any arrest of its particular function (either from disease or accidental causes) a factor in the production of certain pathological conditions, as for example, rheumatism and nephritis; and certain structural alterations of the liver give rise to functional irregularities, which leave special morbific matter in the circulation, and thus produce a blood alteration which may aggravate an existing diathesis; for, when "oxidation is imperfectly performed in the liver, there is a production of insoluble lithic acid and lithates instead of urea, which is the soluble product resulting from the last stage of the oxidation of nitrogenous matter." We see this even in health, when individuals surfeit themselves by an excess of food which is over rich in nitrogenous principles.

There are some persons who, from habit, are remarkably prone to form lithic acid in the liver, and, when this tendency exists for a series of years, a diathesis is formed which controls the system, and the constant production of the lithates establishes either gout or calculus; for whenever a diathesis produces in one person the lithate of soda, and in another plain uric acid, it can with propriety be said to give rise to two different affections, two different series of phenomena which spring from one and the same root. Hence it is that we infer the identity of the causes of

gout and calculus; the same causes which operate in the production of the one, bear upon the formation of the other. Therefore as gout depends measurably upon lithæmia, so also urinary calculi depend for their origin upon certain functional derangements of the liver acting in concert with special affections of the kidneys, such as catarrhal inflammation of the urinary passages over-stimulated by an acid state of the urine.

Next in frequency to uric acid, the *oxalate of lime* is the most important constituent entering into the formation of urinary calculi, at one time forming the entire concretion, at another only the nucleus around which other urinary salts may collect. This is not one of the normal constituents of the urine, but is found frequently as a deposit, being probably produced through uric acid from the nitrogenized material of the food or tissues—oxalic acid combining with the lime in the economy and forming the oxalate of lime. Time will not permit me to enter into the various theories advanced as to the production of this substance in the system, and hence I will not refer to them.

This product is never present in the form of sand, as is the case with uric acid, but often exists in microscopic calculi composed of dumb-bell crystals aggregated together; we detect it also in the system in almost every form of debility, often in febrile affections, and most usually associated with an excess of urea. When this condition exists, we have what is termed "oxaluria," a condition of the system referable to some interference with healthy respiration; possibly produced by chronic bronchitis, emphysema of the lungs, affections of the liver, or exhaustion of the nerve centres from either mental or physical labor, or, in some individuals, the result of excessive alimentation during a too sedentary life which interferes with healthy digestion and assimilation. It is evident that this particular substance is the result of a retrograde metamorphosis both of food and tissue, and hence it is unnecessary again to refer to its production from special articles of diet. There is, however, one circumstance deserving notice, which is the uniform constancy of the relative proportion of oxalic and uric acid, a condition which exists uniformly and which suggests the inference that the same causes are at work in the production of both; especially is this the case when we notice the remarkable correspondence in the proportion existing between the nuclei of the two varieties, showing that they are very closely allied, and that the tendency to form one or the other in different individuals depends solely upon some particular idiosyncrasy.

Cystic and xanthic oxides are so very rare that we will not consume time in their investigation, but close this portion of our subject with a brief notice of the phosphates, which do exist in normal urine; uncombined, however, they are insoluble, being only held in solution by the presence of free phosphoric acid. Hence it is that calculi composed of the phosphates are not very common as constitutional calculi, and as nuclei they are exceedingly rare.

It is not necessary to consider the circumstances which cause the presence of the earthy phosphates in the urine, but it is essential to investigate the conditions which determine their precipitation; these depend entirely upon an over-alkaline state of the urine, which has been rendered so by the presence of ammonia, the phosphate of ammonia and magnesia being precipitated as crystals from the urine in combination with the phosphate of lime; here the ammoniacal condition does not arise from an increased excretion of ammonia, but is the result of the decomposition of urea, setting free carbonate of ammonia and water.

This state is favored by any persistent obstruction to a thorough evacuation of the bladder, producing an irritated or inflamed condition of the mucous membrane, and causing it to secrete a muco-purulent discharge; this discharge contains an alkaline pus which is acted upon by the phosphoric acid present with the phosphates of ammonia and magnesia (which are held in solution in the urine only by the excess of this acid), and the salts are precipitated in the form of insoluble phosphates. Such conditions are favored by enlargement of the prostate, which prevents complete evacuation of the bladder, the retained urine becoming decomposed, and the free ammonia given off, increasing the cystitis and reacting upon the pus, forming an adhesive gelatinoid mucus which cements and binds together the precipitated phosphates in a mass. In the same way the presence of gravel or foreign bodies in the bladder produces cystic irritation with its resulting ammonio-phosphatic deposits, and thus calculi of this nature are formed.

We have now shown that all calculi are either of local or constitutional origin; that local calculi depend for their cause upon some disease either primary or secondary within the bladder, whilst constitutional concretions are dependent upon some vicious action—some error of assimilation inherent to the system; and constitute by far the larger proportion of all urinary deposits. For convenience of description, we will divide calculi into two classes: the *first* including all those formed of uric acid and the urates of ammonia, soda, lime, and magnesia, with the rarer forms of uric or xanthic oxide, and fibrinous and blood calculi; the *second*, embracing the various phosphatic deposits and the combinations of the oxalates.

In examining different collections of calculi, it has been discovered that three-fifths of all calculi met with in adults of all ages are composed of uric acid and the urates, nearly two-fifths are phosphatic, and only some three or four per cent. are oxalate of lime; three-fifths, then, are the product of urine abounding in an acid, of which they are the expression; the remaining two-fifths are the products of urine, generally alkaline, mostly ammoniacal, of which they are the result. It follows, then, that the urates, the oxalates, and a few of the phosphates, which are formed in the kidney, must be the result of certain constitutional derangements; whilst, on the other hand, the phosphates, which, whether pure or mixed, are produced solely in the bladder, must be the consequence not of constitutional but of local disease within the bladder itself.

Hereditary Influence.—In 1776, just one hundred years ago, Scheele discovered uric acid; and so frequently is it now observed to form the leading constituent of urinary concretions, that, with Dr. Owen Rees, who has examined the subject most closely, we may safely state that, "without its presence, calculi would become much less common than they are." This being true, we first consider calculi in reference to this constituent. In doing so, we perceive that in all those cases in which uric acid is in excess, a particular diathesis exists—a diathesis which may have been acquired, but which in all probability has been intensified by some pre-existing habit of the system referable to hereditary causes; in fact it may be safe to state that it nearly always depends upon a previous vice of constitution which is entirely hereditary, for if we examine the history of one of these cases, we will almost surely find hereditary causes at work in its production, and, as Sir Henry Thompson has remarked, "either gout or calculi will be traced in the family of nearly every patient who presents himself for treatment."[1]

[1] Sir Henry Thompson, Preventive Treatment of Calculous Diseases; London, 1876.

We all recognize the hereditary tendency of gout—a fact too well established to admit of a question; therefore, as gout depends in a very great degree upon the formation of urate of soda in the system; and as five-sixths of all renal concretions and vesical calculi which have recently descended from the kidney are composed of uric acid (substances so very nearly the same), we infer that hereditary influences have controlled the one as well as the other. It is very true that hereditary tendencies vary in force in different families; that some members manifest the peculiar diatheses under which they labor, at one period of life, whilst others of the same family develop the same disease at another, and that particular circumstances, or accidental causes, control the pre-existing tendency, and either hasten or retard the affection; thus, for example, we see some members of a family suffering from gout in youth, others in middle life or old age; and likewise we find some who develop a calculus in infancy, others not until adult life; or gout and calculi may alternate, one generation producing the one, and the next the other; or then again we may meet a person with gouty symptoms and deposits of chalk stones, which after a lapse of years subside, when a uric acid gravel may form in the kidney. Here we certainly have a manifestation of two different effects which arise from one and the same cause. I am aware that hereditary influences, as causes of calculi, have been questioned by many of our best informed investigators of this subject; but in view of such facts as have been produced from the concurrent testimony of Civiale, Le Roy d'Etiolles, Thompson, and others of like reputation, I am led to believe that hereditary influences stand first in the scale of causes which produce calculous affections.

Seeking a cause for this particular diathesis (uric acid), we find it in a great measure due to defective assimilation in the organs forming the primæ viæ; we see the stomach failing to digest food in a proper manner; primary mal-assimilation furnishing improper materials for the healthy constituents of the blood; the liver defective in its action, and failing to excrete its due proportion of the urinary salts; the solid constituents of the urine becoming augmented; and the over-worked kidneys unable to eliminate the excess of uric acid from the super-saturated blood, the remainder being thus precipitated in crystalline form. Uric acid being very insoluble, unless in a large proportion of water, and that too at the normal temperature of the body, it follows that whenever the solution becomes denser, or its temperature lowered, the uric acid must be precipitated and left in some portion of the urinary organs, forming in the kidney renal calculi which afterwards become vesical.

As we have seen a certain diathesis cause an excess of uric acid and the urates, so likewise it can be shown that other trains of circumstances will provoke diatheses which produce the other varieties of calculus.

Climatic Influences.—Climate doubtless exercises more or less influence in the production of these affections, for in very changeable regions calculous diseases are more common than in those localities where a more equable temperature exists. This is especially the case in the temperate zones, whilst in very cold or hot countries the disease is not so frequently met with. Still there are certain districts of the earth's surface, bounded by the same lines of latitude and longitude, and subject to the same climatic influences, that develop the disease in one section, whilst in another section similarly situated it is never encountered. This is especially noticeable in England and the United States.

Calculous affections are much more frequent in Kentucky, Ohio, Virginia, Tennessee, and North Carolina, than in any other portions of the United States, although we have a vast area of territory similarly located, geographically, geologically, and climatically, in which stone is rarely if ever encountered. The same may be said of England, where calculus is very common around Norfolk and Manchester, while in other regions which are within the same latitude, and subject to the same temperature, it is almost unknown. This is also the case in Hindostan and those portions of China in the vicinity of Canton; there, calculous disorders abound to a greater degree than in any other parts of the globe. These countries are noted for their variableness of temperature, which is one of the causes assigned for the frequency of this disease; and it may be possible that a due regard to a change of clothing is not sufficiently observed at such times as these changes occur, and that this neglect may have an important bearing upon the subject; for when the skin is not properly protected, its functions become disturbed, and the kidneys are required to perform an excess of labor.

Poverty.—The poorer classes are more liable to the affection than the rich; and it may be noted that they are worse fed and worse clothed than those in more affluent circumstances. Upon such a class of the population, climatic changes have a more depressing influence, and in this way may prove a cause of the affection.

Food.—The belief that food is one of the causes of calculous affections, is probably not altogether without foundation; for we find certain articles of ingesta, already mentioned, which have a tendency to induce acidity with flatulence, and thus directly produce dyspeptic troubles which may light up a diathesis lying dormant. Dyspepsia being one of the most usual of the causes of calculus, almost any article of food which deranges the function of assimilation, may stand in relation of cause to the effect. Highly nitrogenous food, when combined with acescent ingesta, increases the deposition of uric acid, and lessens the formation of the phosphates in the urine; again, an almost exclusively animal diet contributes to the rapid formation of uric acid, and, in persons who labor under a calculous diathesis, lays the foundation of stone. We believe that the various articles of diet which are considered productive of calculous concretions, are so secondarily, first producing irritation in the alimentary canal, followed by dyspepsia with mal-assimilation of the food, and thus becoming the remote cause of the affection; it is not probable that food properly digested is ever the cause of calculus.

Water.—Excepting in the case of oxalate of lime concretions, it will be safe to assert that water does not exert any direct effect upon the production of calculus. It is very true that, in the case of the oxalic variety, water may furnish a proportion of lime, which being acted upon by the acid, will produce the oxalate of lime deposit, but in regard to the other varieties, we see no sound reason urged, why it should be considered as a direct factor in their production. The assertion that water is a cause of calculus, is not sustained by the facts in the case, analyses of the calculi themselves proving that none of them correspond in chemical composition with the salts which exist in the waters drunk in those districts where the calculi make their appearance.

It is self-evident that the only way in which water could have any influence in the formation of calculi, would be by causing a deposit of those salts which exist naturally in the urine itself; and to this end they would have to be of alkaline reaction, and this would favor only a phosphatic

deposit; but even this view is not sustained by experience, for, on the contrary, alkaline waters, such for example as those of Vichy and Vals, and others of like constituents, enjoy a reputation for counteracting a calculous tendency, and even for dissolving the stone when formed. Then again the water of the Seine abounds with carbonate of lime, and were we to attribute to its use the calculous affections which are so frequent in and around Paris, we would naturally expect to find calculi composed of carbonate of lime; but both observation and analysis prove that carbonate of lime calculus is the least frequent of all varieties.

The same may be said of other regions where stone is prevalent. The sandstone districts of Kentucky and Tennessee, where soft water is used, furnish just as many cases of calculus as do the limestone sections where hard water is commonly drunk; and in Alabama, where stone is but seldom encountered—there having been only some fifty to seventy-five operations performed upon her natives since the settlement of the State in 1817—it is a fact worthy of consideration that the greater number of cases has originated in a belt of country running east and west through the State, just above and including Tuscaloosa; a pure sandstone region, with the softest and best water in the State. On the other hand, the northern and eastern sections abound in blue limestone, and the water is very strongly impregnated: yet we rarely, if ever, hear of a case of either gravel or stone within these limits. Again, in middle and southern Alabama, the base of the soil is what is known as "rotten limestone," and the water strongly alkaline; this is also the case in eastern Mississippi, and still we have been unable to obtain reliable information of even a half dozen cases of stone having ever occurred in these regions.

The testimony of Dr. James Guild, of Tuscaloosa, who has been the leading physician and surgeon of that vicinity for the last forty years, and who has probably operated for stone a greater number of times than any other surgeon in Alabama or Mississippi, is, that his cases have all come from the sandstone districts where soft water is commonly used.

Many other interesting examples could be brought forward to prove that water does not directly favor the production of calculous affections, but the time allotted for the reading of this paper will not permit an extended investigation of the subject. It may be well to suggest, however, that hard water as a constant drink may secondarily become one of the causes of stone, by deranging the digestive organs.

Exercise.—Deficient exercise is an important factor in the production of these concretions; they being more frequently found among those who are engaged in sedentary pursuits than in that class which leads a more active life. Inhabitants of large and densely crowded cities, as Paris, for example, who are confined within doors and live chiefly upon a stimulating diet of a highly nitrogenized nature, with deficient respiration and neglect of necessary muscular exertion, become fit subjects for the invasion of dyspeptic troubles, which, when supervening upon an hereditary diathesis, favor the formation of uric acid, with its sequelæ gout and calculus.

Respiration.—Respiration, which is in a great degree controlled by the amount and character of exercise taken, exerts indirectly a powerful influence over the production of the urinary salts. We have shown that a large proportion of all urinary concretions are composed of uric acid, and that this acid is usually precipitated in the form of one or another of the urates. We know that, in health, there are only about eight grains of this acid excreted per diem, whilst of the well-known substance

urea (which, on account of its greater solubility, is never precipitated) there is nearly half an ounce separated from the blood daily by the kidneys. Now, chemical analysis shows that the only difference between these two substances is the varying amount of carbon and oxygen present in their composition; uric acid being represented by the formula $C_{10}N_4H_4O_6$, while the composition of urea is $C_2N_2H_4O_2$. If then the principal difference is made up by carbon and oxygen, the elements of carbonic acid, we naturally refer an excess of uric acid in the circulation to that process by which carbonic acid is produced, and we know this to be respiration. Hence we infer that deficient exercise, by interfering with free respiration, healthy digestion, and assimilation, favors the production of calculous concretions.

Age.—Next in order, age seems to exert an influence in the production of calculi, the urine of infants being more abundantly supplied with uric acid than that of adults; which condition is doubtless the result of the rapid changes to which they are exposed, only a few hours after birth, cold, light, and deficient or improper nourishment, tending to disarrange the proper performance of the functions of their various organs. Taking into consideration their defective digestion, errors in their diet, the irritation of dentition, with the consequent excitement of the brain and nerve-centres, we find them in the very pathological condition most favorable to the formation of a diathesis which, when acted upon by hereditary influences, will produce these deposits.

It is not probable, however, that calculous affections are a great deal more frequent among infants and young children than among adults, although statistics would lead us to infer that such was the case, since these go to show that operations for stone have been performed oftener upon children than upon adults, and more frequently upon the aged than those in middle life; but when we investigate the subject, we find, as Mr. Coulson has remarked, "that an error has crept in from not using the proper precaution to distinguish between absolute and relative numbers. To determine liability, the absolute numbers should be corrected by the numbers of persons living at the several periods of life enumerated. Thus, if all persons under twenty were affected with stone, and all over seventy were affected with the like complaint, it is evident that the liability would be the same, though the absolute number of persons attacked would be very different." Then, to enable us to determine the liability of children of a certain age to stone, we must correct the absolute numbers by the numbers of persons of all ages living at the several periods of life enumerated. Corrected in this manner, statistics would most probably show that young persons are much less liable to calculous affections than is generally believed. In fact, we have observed that in India, where stone is exceedingly prevalent (more so, perhaps, than in any other section of the world), old men are more liable to the disease than any other class of the population.

Sex.—Sex has been considered as exercising some influence over these affections, simply because stone in the bladder is comparatively infrequent among females. This is no doubt chiefly due to the fact that they have short and large urethræ, without prostatic portions, thus affording a ready exit to the nuclei of stone which pass to the bladder after having been formed in the kidney. We are not aware of any reason why the female is not as liable to the formation of calculous concretions within the kidney, as the male; for women are subject to the same general laws and affections which produce the disease in the other sex. Indeed they are liable to,

and frequently suffer from, attacks of calculous nephralgia, and are oftentimes the victims of gout, when exposed to the same exciting causes and when governed by the same hereditary influences which produce these diseases in men.

Race.—On the other hand, race evidently does exert a direct bearing upon the formation of these concretions. In America, where we have ample opportunities of examining the subject, it has been shown that calculous affections are exceedingly rare among the negro population, and that gout is seldom, if ever, seen in the black. During the past year, I have been engaged in collecting statistical information of American lithotomies, and in examining the various reports in reference to this point, I find that out of some 3039 reliable cases which I have to this date tabulated, there are only 102 operations reported as having been performed upon the negro, and only 31 upon the mixed blood or mulatto. Similar observations have been made among the native African tribes, and stone has not been found as a frequent affection. Dr. Thomas Winterbottom states, in his Medical History of the Native Africans around Sierra Leone, that "notwithstanding the excesses committed by the pagan nations of Africa in the use of spirituous liquors, etc., I have never heard of an instance of gout among them, nor is it probable that they are acquainted with the disease;" again, he says, "except gonorrhœa, I have never met with any affection of the urinary passages, such as gravel, calculus, etc." This is further corroborated by the testimony of Dr. Livingstone, who states that "stone in the bladder and gravel are unknown; I have never met with a case, though the waters are often so impregnated with sulphate of lime that the kettles quickly become encrusted with the salt, and some of my patients who were troubled with indigestion, believed that their stomachs had got into the same condition."[1]

This apparent immunity from the affection cannot be due to color, for we find some of the dark-skinned races, as for instance the Asiatics, exceedingly prone to the disease; and in the peninsula of Hindostan, especially in the central and upper provinces, stone is met with probably more frequently than in any other locality of equal extent on the earth. In a paper upon this subject, by Dr. Curran, of the East India Service, it is mentioned that "from Peshawur to Calcutta the diathesis is general and almost universal;" and we note that at the Medical Missionary Society's Hospital, at Canton, Dr. J. G. Kerr operated for stone more than 300 times in the short space of three years. Other instances of the remarkable frequency of this operation at the hands of the native surgeons of India could be cited if it were necessary to do so.

Like the negro or black man, the aboriginal tribes of North America seem to be peculiarly exempt from stone affections; whilst, on the other hand, the white races are its constant victims—a fact which evidently can be used as an argument to prove the influence of race in the production of a diathesis favoring the formation of stone. If not, how else shall we explain the fact that the negro and North American Indian are exempt in a great degree from an affection to which the white man, and his darker complexioned Aryan brother of Asia, are so liable?

Habits.—It is possible that habits and modes of life of the white races may influence in a great measure the production of the disease, exposed as they are to all the cares and excitements of civilization; inhabiting sections of the globe subject to sudden vicissitudes of climate, with all

[1] Livingstone, Missionary Travels and Researches in Central Africa.

the errors of a life which is not led in accordance with the great laws of nature, it is reasonable that we should find stone more frequent with them than in those races which live in a comparatively natural state.

On the contrary, the negro and American Indian are not only accustomed to a life in the open air, and to a diet suited to their digestion, but having the finest teeth, they masticate their food properly, and thus still further favor healthy digestion. Their skins are more active, and, as great depurating organs, eliminate much effete material which in other races must pass off by the kidney. It is also probable that some inherent principle exists in the negro which in a great measure exempts him from this disease, just in the same way that he is to a degree exempt from certain fevers and other affections. What that principle is, I am at a loss to suggest. The inhabitants of India live principally upon rice and other cereals—materials rich in nitrogenous matter, and, being careless in guarding themselves against sudden changes of temperature by suitable raiment, they invite as it were the formation of a uric acid diathesis; and any exciting causes which supervene to disturb the normal relations of the urinary constituents must naturally tend to produce calculus.

Occupation.—Certain occupations seem to exert an influence in preventing or producing these affections, seamen and soldiers being peculiarly exempt; so much so, that it is a noticeable truth that the disease is rarely met with in persons who follow either of these modes of life. Civiale in his large experience saw only three cases among sailors; and Mr. A. Copland Hutchison states that, in the British Navy, with a number of patients estimated at 86,000, there were only eight cases of stone encountered in sixteen years, and that three of these were afflicted with this disease before entering the service. The records of the Greenwich Naval Hospital, during a period of twenty-seven years, show that not a single operation for stone had been performed. This is equally true of the American Navy, as there is not a single case reported at our Naval Bureau; and the testimony of Dr. W. S. W. Ruschenberger, Medical Director, U. S. N.,[1] whose experience in this service extends over fifty years, is that he has known of only one operation, and that his personal observation has been confined to but one calculous sailor, who had the disease at the time he enlisted.

A similar exemption seems accorded to soldiers, if we receive the statements of Sir James M'Grigor, which show that out of 340,000 patients, among soldiers serving in the Peninsular Campaign, no case of calculus was met with from 1811 to 1814. Like results are obtained from the French and Russian Army records; and the experience of our late civil war is that there have not been more than two cases of calculus recorded as the result of a systemic condition; although probably the richest collection in existence of stones having bone nuclei, has been collected for the Army Medical Museum, at Washington, through the indefatigable energy of that accomplished gentleman, Dr. Geo. A. Otis, of the Surgeon-General's Office.

Without definite facts to guide us in forming an opinion, we believe that the most probable cause of the infrequency of stone among seamen and soldiers, is that they are generally men in the prime of life, there being comparatively few of either children or old men among them—classes which are considered the most liable to stone; again, the rigid examina-

[1] MS. letters to writer.

tion of recruits for both services, naturally excludes all those suffering from that state of constitution which would favor a formation of such deposits. We cannot, therefore, consider that occupation, *per se*, exerts any protective influence against calculous concretions.

Moral and Physical Emotions.—Very different is it with moral and physical emotions; they evidently exert a powerful causative effect in the production of these affections, and, if time would permit, some very interesting examples could be adduced to prove the effect of the passions—fear, anger, joy, and grief—over the normal secretion of the urine.

Dr. Debout D'Estrées, in his treatise on the "Causes of Gravel and Stone,"[1] narrates quite a number of clinical cases, showing the effect of mental excitement and depression; and M. Bouchardat, in his Memoir upon Gout, gives instance after instance in which violent emotions have produced polyuria; while Civiale himself often remarked how frequently severe exercise and great trouble of mind were followed by the formation of much red sand in the urine.

Foreign Bodies.—We now come to a fertile cause of calculus, which is the presence of foreign bodies introduced into the bladder, either by accident or from design; they irritate and inflame the mucous membrane, thus causing the secretion of an alkaline mucous or purulent fluid by which the urine is rendered alkaline, and hence a precipitation of the phosphates around the foreign body which acts as a nucleus.

Injuries to Spine and Urethral Stricture.—Injuries of the spinal cord produce phosphatic deposits, and thus become one of the causes of vesical calculi; and it has been asserted that stricture of the urethra favors the production of these concretions. In relation to the latter cause, I am inclined to believe that its effect has been over-estimated; for, after having carefully investigated this point, I am struck with the fact that authentic cases of calculus, resulting from urethral stricture, are exceedingly rare. My friend, Prof. Fessenden N. Otis, of New York, says, in a MS. letter on this subject, "in over five hundred cases of stricture, to which I now refer, I have met with but (3) three cases in which stone was coincident with stricture." In my own experience with stricture of the urethra, I find, among the notes of over three hundred cases, that there are but (2) two in which calculus and stricture have been associated; one of them occurred in an old man sixty years of age, suffering from enlargement of the prostate; the other in a youth in whose urethra a small uric acid calculus was lodged in a pouch posterior to the stricture.

It is not improbable, however, that, in very close strictures of long duration, an arrest to the passage of urine may produce vesical irritation and inflammation, causing the secretion of a muco-purulent discharge from the bladder, which will favor such deposits.

Colloids.—Besides the presence of urinary salts, there is something more required for the formation of calculi, for in their natural state the salts are not capable of entering into the agglomerated condition essential to such formations; nor should we consider calculi as mere precipitates of ordinary crystalline and amorphous deposits held together by plain mucus. Some other agency is necessary, and this will most probably be found in the presence of the colloids—certain glutinous products, the result of subacute inflammation of the urinary passages, the kidneys, the ureters, and the bladder itself; they evidently exert an influence over precipitated crystals, changing their forms into amorphous, or, as

[1] Les Causes de la Gravelle et de la Pierre, par le Dr. Debout d'Estrées; Paris, 1876.

Dr. Vandyke Carter[1] designates them, "submorphous structures," which, in the presence of an animal basis, become embedded in the colloid matrix, and thus form calculous masses.

The experiments of Mr. Ord[2] prove that crystalline forms, when associated with a colloid, are altered by two processes—first, by modification of surfaces and angles; and secondly, by a corresponding rearrangement of the molecules themselves. He proved that the strength of a urate solution, with a concentration and excess of the acid supplied, controlled the form of the resulting crystals, and that uric acid, in many of its aspects, verged upon the nature of a colloid. He found that albumen exercised a more powerful influence than any other substance in producing a change of crystalline form, and a disposition to assume an agglomerated condition. Still, we find it difficult to say what is the chemical nature of the animal basis which, in the living being, causes uric acid to change its pure crystalline form to some other, more prone to agglomeration. But, judging from the experiments of Mr. Ord, we conclude that it is albuminous, and, as "mucin" is derived from these formations, probably mucin is the cement.

The general conclusions which we deduce from these experiments are, first, that albumen and the other colloids, when placed in the presence of uric acid, cause its crystals to be deposited in the shape of small, thick, sub-cuboidal bodies, without curved faces, and sometimes as dumb-bells or spheroids; secondly, that grape sugar and other crystalloids cause the acid to be thrown down in long tabulated or foliaceous crystals, with flat sides and sharp angles and edges—forms essential to the process of agglomeration into masses.

Oxalate of lime crystals are in like manner altered in form by the presence of a colloid; but it seems to be requisite that the colloid should be denser than for the same change in uric acid. This fact led Mr. Ord to draw the inference that, as small calculi which pass from the kidney in nephralgic colic are generally of the oxalic acid variety, they are doubtless formed in the recesses of the kidney, where the colloid medium is denser than in other portions of the urinary tract.

The triple phosphates are less frequently influenced by the presence of the colloids than the other varieties, and consequently they are less disposed to form spherical or agglomerated masses.

In the urine we find four groups of substances which are capable of moulding urinary salts into conglomerate masses, and thus of favoring the formation of calculi, viz., mucus, urea, coloring matter, and salts. Standing first among them is mucus, a colloid prone to decomposition, and active as a ferment in promoting putrefaction of organic substances when associated with them in solution; now "an excess of this mucus, altered in character in the urinary passages, or an effusion of albumen, fibrin, or blood, as often results from congestion of the kidney, or from irritation of the urinary tract, will furnish a colloid medium with which uric acid, the urates, or oxalates (themselves perhaps in excess), will combine in the manner before described and thus form a calculous mass."

The microscopic examinations of urinary calculi, by Dr. Vandyke Carter, sustain the views advanced by Mr. Ord, that the spheroidal forms of crystalline masses are not entirely due to the mechanical influences at

[1] Microscopic Structure and Mode of Formation of Urinary Calculi, by H. Vandyke Carter, M.D.; London, 1873.

[2] W. M. Ord, M.B., London, M.R.C.P., St. Thomas Hospital Reports, vol. i., 1870; also Medico-Chirurgical Transactions, vol. lviii., 1875.

work in their formation, and show that there has been a molecule disturbing power acting in the presence of the colloid medium; and also that the form and cohesion of the crystalloids in the formation of calculi may be accounted for by Mr. Rainey's theory of molecular coalescence in the production of shells.

From the microscopic examinations of urinary calculi by Dr. Carter, and the experiments upon urinary salts by Mr. Ord, we are led to the following conclusions: All crystals of urinary salts are modified in shape when they are deposited in a colloid medium; the animal basis of uric acid tends to produce this change; and like causes bring forth the same alterations in the crystals of the other salts. The necessary conditions favorable for the operation of molecular coalescence may at any time be brought into action; and wherever there is an excess of mucus which has been altered in character in the urinary passages, or an effusion of albumen, fibrin, or blood, from congestion of the kidneys, or from irritation along the urinary tract, we have furnished a colloid with which uric acid or the other salts will combine in the manner described. Such a state of system is found during certain fevers, inflammations, etc., where the urine becomes loaded with sediment, and the first nucleus of a gravel begins; this being accomplished, the little nucleus becomes surrounded by a thin layer of protective mucus, in which continued layers in molecular coalescence are deposited, and a calculus is formed.

We do not desire to be understood to say that calculi are accidental formations, dependent wholly upon bare physical causes, but rather that they depend for their production upon an association of hereditary influences with certain physical causes; and that the defects of structural development which we find in certain hereditary conditions probably produce certain elements which are formed into calculi through the agency of the colloids, these being themselves the result of such an hereditary state or condition. It seems necessary for the formation of calculi that these conditions should remain constant and unvarying for a period of weeks, months, or years, thus furnishing time for the slow growth of these concretions.

Time will not permit me to refer to the changes which take place in a calculus after the nucleus is formed in the kidney, and during its passage through the urethra and its lodgment in the bladder; I will, therefore, close with a brief notice of the geographical distribution of calculous affections in North America.

Geographical Distribution of Calculi.—Calculus is not universally found over the Western Continent, there being certain localities where it is met with much more frequently than in others; and then again there are sections where it is almost entirely unknown. The central portions of the United States have furnished more cases than any other parts, whilst the extreme Southern and Northern States rarely produce a case. Along the Gulf coast, the Canada line, and the Pacific shores, stone is very uncommon; as it is also in the Canadas, the British possessions, and Mexico.

In the United States, we find it more frequently in an area of country comprised within the boundaries of Tennessee, Kentucky, Ohio, Indiana, Missouri, Western Pennsylvania, Virginia, North Carolina, and Georgia, than elsewhere; there is also a tract of country adjacent to and including Salt Lake City, Utah, where the disease is said to be remarkably common. On the other hand, stone is comparatively infrequent in Alabama, New York, Maryland, and Illinois; whilst in New Jersey, Delaware, and the New England States, it is so rare that there are very few surgeons who have operated for stone in those States. The Warrens, of

Boston, have performed the greater number of all the operations for this affection which have been done in New England; and that number is very small when we take into consideration the density of the population, and the lapse of time since the settlement of the country. The States bordering on the Gulf of Mexico—Florida, South Alabama, Mississippi, Louisiana, and Texas—are almost entirely exempt from calculous troubles: not more than two or three cases have occurred in Florida; scarcely more in either Southern Alabama, Mississippi, or Louisiana; although in Southwestern Texas, along the Mexican line, quite a number of stone cases have occurred, and have been taken to San Antonio for treatment; but in all other sections of the State the affection appears to be remarkably infrequent. It is a strange circumstance that in the State of Minnesota calculous affections, in the form of nephralgic colic, are exceedingly frequent, whilst stone in the bladder is so very rare; yet this is true, and after a very extended correspondence with the leading physicians and surgeons of that State, I have been able to hear of only three operations for stone upon persons who have resided there. The same may be said of Arkansas, with the difference that gout and calculus are so very seldom seen that they have not been classed among the diseases of the State.

Taking into consideration the recent settlement of Iowa, and its scattered population, it is a little remarkable that stone has been met with so frequently as is shown to be the case by the number of operations reported. To this date I have been unable to gather any reliable information as to the frequency of calculous affections in the far West; but, from the data before me, I am led to infer that they are uncommon. Some few cases of stone have been operated upon at San Francisco, but it has, so far, not been ascertained whether they were of foreign or domestic origin.

In closing, it may be interesting to notice that almost the entire tract of country in which stone has been found to abound, belongs to the carboniferous and sub-carboniferous formations, in which gypsum, limestone, marble, and cretaceous deposits abound; regions the most productive of any others of the temperate zone, furnishing all the varieties of nitrogenous and non-nitrogenous materials of food, both animal and vegetable, the very kind of aliment most calculated to derange the digestive organs.

In summing together the *probable* causes of calculous affections, we place—

I. Hereditary influences which control a diathesis.

II. Digestive troubles produced by an excess or deficiency of proper diet.

III. Sedentary life, with indulgence in stimulating food, by which healthy nutrition and assimilation are altered to mal-assimilation and mal-excretion.

IV. Climatic changes, deficiency of clothing for the proper protection of the body, and an arrest of the healthy function of the dermoid tissue.

V. Want of harmony between the great secreting and excreting organs—the liver, skin, and kidneys—with catarrhal affections of the uropoetic viscera, which favor the formation of a colloid medium.

VI. Injuries of the spinal cord, by which a proper nervous influence over the mucous membrane of the urinary organs is lost.

VII. Foreign bodies present in the bladder, producing cystitis with its consequent muco-purulent discharge, and becoming nuclei upon which the phosphates are precipitated.

SUBCUTANEOUS DIVISION OF THE NECK OF THE THIGH-BONE FOR ANCHYLOSIS AT THE HIP-JOINT.

BY

WILLIAM ADAMS, F.R.C.S.,

PRESIDENT OF THE MEDICAL SOCIETY OF LONDON, ETC.

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Mr. President and Gentlemen:

The observations which I propose to offer to-day for the consideration of the Surgical Section of the International Medical Congress, refer to the operation of subcutaneous division of the neck of the thigh-bone for anchylosis of the hip-joint, with malposition of the limb; an operation which was first performed by myself in London at the Great Northern Hospital on the first of December, 1869. I propose to refer to the experience which we have gained during the past six years, since its first performance, as far as I have been able to collect records of the cases operated upon; and to offer some observations on the mode of performing the operation, the selection of appropriate cases, and the results to be obtained as to the permanent condition of the limb, *i. e.*, whether anchylosis in the straight position is to be the result, or whether we are to endeavor not only to rectify the deformity, but to obtain a false joint at the seat of operation with free voluntary motion of the limb.

Anchylosis of the hip-joint may result from any inflammatory affection of the joint, and formerly, or I would rather say in our own time, under the teaching of Sir Benjamin Brodie, and the generation of surgeons now just passing away, this was the result which all surgeons desired to see as the termination of hip-joint disease, as well as other joint affections. Their treatment was directed to the production of this result by long-continued and absolute rest, with the application of straight and immovable splints. In connection with the subject of anchylosis, it is important that it should be borne in mind that its prevention in a large number of cases, and prevention is always better than cure, is due to the advance of American Surgery; and that Professor Sayre, by disproving one of the old surgical maxims of the English school, that absolute and long-continued rest, and recumbency with a long straight splint or some other form of splint to maintain absolute immobility of the joint, are essential to the cure of the disease—and by substituting his apparatus acting on the principle of maintaining motion during the treatment of the affection—has added a great practical improvement to the modern treatment of hip-joint disease.

As the result of the more severe forms of hip-joint disease, however, anchylosis must necessarily take place, unless resection has been performed, and must be regarded as the best possible compromise, and the best termination of a very formidable disease which threatened even the life of the patient, provided always that the anchylosis has taken place with the limb in a natural and useful position. In the hip-joint, the best position for the patient in which anchylosis can take place, is with the thigh slightly flexed, without any adduction or abduction of the limb;

a little shortening is easily compensated for, and the patient can sit down without much inconvenience in a chair. Anchylosis with the thigh in a perfectly straight position, *i. e.*, so straight that, when the patient is lying on his back on the ground, there is no arching whatever of the spine in the lumbar regions, is attended with very serious inconveniences; the patient cannot sit on a chair except when resting on one buttock on the edge of the chair, the anchylosed limb being stretched out with the heel resting on the floor, and as a result of this habit, in cases of long standing, the knee-joint bends preternaturally backwards; this is increased by a habit sometimes acquired of crossing the sound leg over the anchylosed limb. This condition of straight, it might almost be called over-straight, anchylosis is rarely met with, but I have been consulted by two patients, one a married lady, and the other a gentleman about forty years of age, who suffered so much from this condition of straight anchylosis that they were both anxious to submit to the operation of dividing the neck of the thigh-bone, if motion could be obtained, but as I was unable to promise this, and they had become habituated to the inconveniences, nothing was done. I may mention that it was not only the inability to sit on a chair, which annoyed these patients, but they told me that the bowels could only be relieved in the standing position, so that they were anxious to submit to any operation promising relief if motion could be obtained.

Anchylosis with the limb in a deformed position is, however, the condition for which surgical aid is generally sought. In the great majority of cases of anchylosis of the hip-joint, contraction of the joint with the limb in a deformed position is found to exist; in some cases, simple flexion of the thigh having occurred with very little adduction or abduction; in other cases, severe adduction with a comparatively small amount of flexion, and again, in others, the distortion will be found to depend upon flexion with abduction, or adduction, of the thigh, in about equal proportions. The inconveniences will vary according to the extent and the direction of the contraction, being greatest in those cases in which flexion and adduction are combined, so that the knee of the anchylosed limb is drawn across the opposite thigh; more especially when this occurs in females.

There can be no doubt that an urgent necessity for surgical interference exists in many of these cases, and it was in the city in which we are now assembled that the first operation, having for its object not only that of rectifying the deformity, but of obtaining motion by the establishment of a false joint, was performed by Dr. J. Rhea Barton in the year 1826.[1] A large external incision was made, and the bone sawn through between the two trochanters. The deformity was rectified, and the case proceeded favorably. It is said that useful motion was obtained, but that seven years afterwards anchylosis took place, and the man died of phthisis nine years after the operation. This proceeding, somewhat modified, was afterwards repeated by J. K. Rodgers and others, but the next operation of importance for this class of cases was one also performed by a distinguished American Surgeon, Dr. L. A. Sayre, of New York, who removed a transverse section of the femur, of elliptical form, just above the trochanter minor, by means of a chain saw, the

[1] "On the Treatment of Anchylosis by the Formation of Artificial Joints," in the North American Medical and Surgical Journal, April, 1827, with further remarks in the American Journal of the Medical Seieuces, vol. xxi.

external incision being necessarily of large size. This operation Dr. Sayre successfully performed on June 11, 1862, and as late as April, 1868, the patient is reported as able to walk with free motion of the limb. Dr. Sayre repeated this operation on November 6, 1862, and the case proceeded favorably at first, but abscess with some necrosis of bone occurred, and the patient died of tubercular deposits in the lungs with pneumonia on May, 17, 1863. It was found, however, that a very perfect artificial joint had been formed, and that so far the object of the operation had been accomplished.

I shall next refer to the case in which I divided the neck of the femur subcutaneously at the Great Northern Hospital in London, on December 1, 1869.[1] A case of bony anchylosis of the hip-joint with the leg in an extremely deformed position came under my care at that time, the leg being flexed at a right angle, and abducted, in a man aged twenty-four. It occurred to me that the neck of the thigh-bone was the spot to which attention should be directed, and that if I could divide the neck of the bone by means of a very small saw passed through a wound not much larger than that made by an ordinary tenotomy-knife, and thus preserve the true subcutaneous puncture, I should be able immediately to cure the deformity, and might, perhaps, succeed in obtaining motion. The instruments used were a long tenotomy-knife, and a very small saw three-eighths of an inch in width, and with a cutting edge an inch and a half in length, made by Mr. Blaise, of 67 St. James Street, London. I entered the tenotomy-knife a little above the top of the great trochanter, and, carrying it straight down to the neck of the femur, divided the muscles and opened the capsular ligament freely. Withdrawing the knife, I carried the small saw along the track made—preserving this by pressure of the fingers—straight down to the bone, and then across the anterior surface of the neck, the flat side, and not the edge, of the saw being applied to the bone as it was gently carried along its anterior surface; the edge of the saw was now turned upon the bone, and by a very wiggling sawing movement the section of the bone was accomplished in five minutes. No hemorrhage followed, and I immediately applied a compress of dry lint, retained in position by strips of adhesive plaster and a bandage.

As soon as the bone was cut through, the leg moved freely in all directions; but, before it could be brought into a straight position, it was necessary to divide the tendons of the long head of the rectus, and of the adductor longus muscles, and to cut through the tensor vaginæ femoris muscle. The limb was fixed in a straight position, and bandaged to a long interrupted Liston's splint. No inflammation whatever followed the operation; no swelling, or redness of the skin, or any deep suppuration, but the wound healed slowly, two or three drops of pus in the day oozing from the track of the wound or the granulations at its orifice for three weeks. This did not prevent my applying weight extension on the fourth day, and commencing passive motion. At first the progress encouraged the hope that a false joint would be established and motion retained. After a few weeks, however, the limb began to stiffen at the hip, and all attempts at movement were so painful that I abandoned the idea of obtaining motion, and kept the limb in a straight or nearly straight position for bony anchylosis, which took place, the man obtaining a very useful limb. At the present time this man is able to walk any reason-

[1] British Medical Journal, December 24, 1870; and also a pamphlet, "A New Operation for Bony Anchylosis of the Hip-joint;" London, 1871.

able distance without stick or crutch, and earns his living by an active occupation. I have mentioned the details of this, the first case, because it very fairly represents the general course of events in others.

Deviations in several respects will have to be made in particular cases; for instance, the exact point of puncture and the direction of the same will require to be varied in reference to the altered relation of the thigh-bone to the pelvis, *i. e.*, whether the thigh is simply flexed upon the pelvis, or flexed and adducted, or abducted. The altered direction of the neck of the bone, thus produced, must be very carefully calculated by the surgeon before making the section, which should be at right angles to the long axis of the neck; great care must be taken to avoid cutting obliquely through the neck, or in a direction parallel with the shaft of the bone, a mistake easily committed if the altered direction of the neck and shaft of the bone be not carefully considered. A good general direction in the majority of cases seems to be to place the saw, just before commencing the section, parallel with Poupart's ligament, and from three-quarters of an inch to an inch below it. I have found in the dead-house that this direction will generally enable the operator to cut through the neck a little below the articular margin of the head of the bone.

Then with regard to the number of cases which have been operated upon up to the present time, and their results: I possess the records of twenty-four cases, including five operated upon by myself; four by Mr. Bryant, of Guy's Hospital; two by Mr. Sydney Jones, of St. Thomas's, and one by Mr. Croft, of the same hospital; two by Mr. Jessop, of Leeds; two by Mr. Lund, of Manchester; one by Mr. Hardie, of Manchester; one by Mr. Maunder, of the London Hospital; one by Mr. Willett, at St. Bartholomew's; one by Mr. Jowers, of Brighton; one by Mr. Furneaux Jordan, of Birmingham; one by Mr. Jonathan Hutchinson, of the London Hospital; one by Mr. Brodhurst, at St. George's, and last, but not least, one of the best and most successful, in which motion has been retained, by Dr. Sands, of New York. Out of these twenty-four cases only one death, from pyæmia, has occurred, and that in an extremely unfavorable case, in a very strumous boy, aged eight years, operated upon by Mr. Croft, at St. Thomas's Hospital. In another case, the last operated upon by myself, deep-seated, chronic suppuration followed and gradually exhausted the powers of the patient, a very strumous girl, aged eighteen years, who died eight months afterwards, having suffered from albuminuria and phthisis.

Such a result bears good testimony to the general safety of the operation, and although the dangers even of subcutaneous sections much increase with the magnitude of the operation, these may be kept within their narrowest possible limits by the judgment of the operator in selecting his cases, and the skill shown in the performance of the operation.

With regard to the selection of cases there can be no doubt that the most favorable are (1) Cases of rheumatic anchylosis, because, in rheumatism, no destruction of bone ever exists, and the head and neck of the bone always remain of their full natural size. (2) Cases of anchylosis after pyæmic inflammation, more especially in its subacute form, from which the patient generally recovers. In these cases destruction of bone rarely if ever exists, the cartilage only being more or less destroyed. (3) Cases of anchylosis after traumatic inflammation, in healthy adults, such as follows wounds of the joint, and gunshot wounds in the neighborhood of the joint. As a general rule, in these cases, little or no destruction of bone occurs even after acute suppurative inflammation. (4) The most

unfavorable cases for operation are those belonging to the strumous class, because, as a general rule in these cases, disease of the bone, with destruction of the head and neck of the thigh-bone, to a greater or less extent, takes place; and consequently, in the worst forms of this class, the operation either cannot be performed, or would be attended with such difficulties that it should not be attempted. In many cases of anchylosis after hip-joint disease in children, however, especially those in which there is little or no evidence of a strumous constitution, and in which the disease has been arrested in an early stage, without the occurrence of suppuration, not more than superficial caries exists, and sometimes the mischief is limited to the articular cartilage. In these cases the head and neck of the femur may be but little altered in form and direction, and the operation can generally be performed; and Mr. Bryant, of Guy's Hospital, has especially extended the operation with the most marked success to this class of cases.

Originally my idea was that the operation should be limited to cases of bony anchylosis, generally occurring in the adult; but the ease and safety with which Mr. Bryant has performed the operation in children from eight to fifteen years of age, will induce me also to operate in similar cases.

When the operation of dividing the neck of the femur cannot be performed in consequence of extensive destruction of the head and neck of the bone, then the operation first performed by Mr. Gant at the Royal Free Hospital in London, on December 10, 1872, should be adopted. It consists in dividing subcutaneously the shaft of the femur just below the small trochanter. The bone is cut from without inwards, and not always completely through, a portion of the femur on the inner side being broken. Instruments similar to my own are used. Mr. Gant has twice performed this operation with success.

Mr. Maunder, of the London Hospital, has also operated subcutaneously, or nearly so, in the same situation, but has substituted a chisel for the saw—as, it appears to me, without any advantage—the small saw being the more manageable instrument, and never more than one introduction being required. Mr. Maunder has, however, very successfully employed this method in several cases.

The conclusions to which, I believe, we are justified in arriving from what has been stated are—

I. That bones can be divided subcutaneously like tendons; and that the operation of completely dividing the neck of the thigh-bone by a small saw, introduced through a small subcutaneous puncture, is a well-established surgical operation attended with very little risk.

II. That the long bones can be completely divided by the same method in any part of their length with very little risk.

III. That in a large proportion of these cases the healing of the wound takes place by the first intention, and that no swelling, redness, or inflammation follows. These cases of subcutaneous osteotomy proceed as favorably as subcutaneous tenotomy.

IV. That in some cases a little suppuration from the track of the wound, amounting only to a few drops in the day, does occur for a week or more after the operation.

V. That, in a very few cases, deep-seated suppuration occurs; but in only one of these, so far as at present known, has death resulted from pyæmia out of the twenty-four recorded cases. In one other case death

was accelerated by the operation, or rather by the prolonged suppuration which followed. The patient died eight months afterwards from albuminuria and phthisis.

VI. That the permanent result of the operation has hitherto generally been to correct the deformity, and obtain bony anchylosis with the limb in a straight position; but in several instances free motion has been obtained, and remained only for a few months, when it has been gradually lost. In a few cases free motion has remained for about a year, and we hope will be persistent through life; but time and further experience are necessary before this can confidently be stated to be a reliable result of the operation.

Discussion on Mr. Adams's Paper.

After the reading of the preceding paper, Dr. Alfred C. Post, of New York said:—Many years ago I performed Dr. Barton's operation upon a young man who had had coxalgia resulting in anchylosis in such an abducted and flexed position that the genitals were hidden from view. I divided the neck of the femur, according to Barton's plan, but on the second or third day the limb became emphysematous, gangrene followed, and the patient died. At the post-mortem examination, the femoral artery and vein were found hooked over the sawn end of the bone. Such an accidental complication would not be likely to occur again, but Mr. Adams's plan of dividing the bone within the capsule would certainly avoid it.

Dr. Leopold Servais, of Belgium said:—Anchylosis may result from various causes, such as rheumatism, scrofula, syphilis, wounds, etc. The use of an anæsthetic is necessary to distinguish between true and false anchylosis. I am of the opinion that true anchylosis does not occur save in cases in which suppuration has taken place. When it is necessary to divide the femur below the trochanters, it is, I think, better to employ brute force, and break the bone, as we then have but a simple fracture to treat. From experiments lately performed in France, it has been proved that the femur is weakest at this point, and consequently it will be likely to give way there. The breaking is not difficult, as I have ascertained by fracturing the femur accidentally in other operations.

Dr. Lewis A. Sayre, of New York, said:—I would ask Mr. Adams when he begins extension, and how long he continues it?

Mr. Adams said:—I have heretofore applied extension on the fourth day, but shall in future be disposed to employ it at an earlier period. It should be continued about as long as for an ordinary fracture, but, after a few weeks, passive motion should be practised, and this should be continued once a week, under chloroform, for a long time, in order to prevent the anchylosis which is so liable to follow.

Dr. Sayre said:—The cases upon which I operated are fully reported in my book. One was the case of a female whose legs were so crossed as to interfere with micturition. I took out a section of the bone, rounding off the surfaces, and allowing the psoas and iliacus muscles to retain their attachment to the lesser trochanter. The recovery was perfect, and, when the patient died from other causes, a ligamentous connection was found to have been formed between the resected parts, and there was also a Y-shaped ligament and a form of membrane resembling the synovial. The results of my cases have been good, but I am decidedly in favor of Mr. Adams's operation, as the more simple and safe one, provided anchylosis does not subsequently occur. Mr. Adams's statement confirms my opinion that this result is quite probable, yet the cases in which it does not take place are sufficiently numerous to make the operation a desirable one. I shall in future use this method, but shall apply

extension at once, and make passive motion frequently. A divided bone, tendon, or fascia, should be at once put in proper position, as the effused lymph, being poorly vitalized, does not bear the process of subsequent stretching at all well. The danger of extending the muscles beyond their original length, and consequently separating the bones, although possible, yet will not deter me from putting on extension at once.

Dr. T. G. Richardson, of New Orleans, said:—I recall a case in which I fractured the neck of the humerus in trying to reduce an axillary dislocation of long standing. I at once pushed the broken bone up into the glenoid cavity, bound it firmly in place, commenced motion on the third day, and secured a good and movable joint.

Dr. E. M. Moore, of Rochester, said:—Dr. Bly, of Rochester, has made some experiments upon the extensibility of living muscles. He has found that the force required to stretch a muscle is very great, but that a weight of 300 lbs. will cause decided lengthening. The practical inference is that too great extension should not be used after Mr. Adams's operation, lest the ends of the bone be drawn too far apart.

REMARKS UPON PENETRATING WOUNDS OF THE ABDOMEN, WITH THE SUGGESTION OF A CHANGE OF PRACTICE IN SUCH CASES.

BY

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PENETRATING wounds of the abdomen are full of interest to the surgeon, not only because of their frequency, but also because of their extreme fatality. The marked susceptibility of the lining membrane of the abdominal cavity to morbid action, and especially to the absorption of deleterious agents, together with the importance of the organs contained, will account satisfactorily for the gravity of these wounds.

But the danger of these injuries varies according to their character and to the organ implicated. It is, therefore, not a matter of indifference whether the injury be a puncture, an incision, or a gunshot wound; for, while punctured and incised wounds are more liable to induce hemorrhage, they heal more readily by first intention; whereas gunshot wounds are less apt to be attended with serious loss of blood, but will almost invariably be followed by suppuration. In penetrating wounds of the abdomen, our first concern then is with regard to hemorrhage, which itself will depend upon whether a large vessel be opened or a parenchymatous organ be injured; for punctured and incised wounds of large vessels, and of the liver, spleen, and kidneys, are usually attended with rapidly exhausting loss of blood. If the patient escape this first danger, the question presents itself as to what we should apprehend next; and the determination of this question is of the utmost importance to the patient, as his life may depend upon it.

Upon this point there exists among written authorities the most striking uniformity in the belief that *peritonitis* is the condition most to be dreaded when primary hemorrhage has not carried off the patient, and that the extraordinary fatality of these wounds is the necessary consequence of this inflammatory action, whether induced by the mechanical injury, or by the contact of atmospheric air, of effused blood, or of fecal extravasation. And yet, when we come to examine into facts, we find that the great majority of deaths occurs so early after the injury as to render such an explanation exceedingly improbable and unsatisfactory. Death, unless from hemorrhage, usually takes place within from twenty-four to thirty-six, or at most forty-eight hours after the injury; a rapidity entirely too great to be accounted for by any process of inflammation, properly so called. No true inflammation ever runs its course to a fatal termination in so short a time. Moreover, the peritoneum is not an organ of such vital importance as to render its inflammation *necessarily* fatal; and when death does attend this inflammation from other than traumatic causes, dissolution rarely takes place in less than a week.

Again, it may be doubted that the peritoneum is as susceptible to inflammation as is generally supposed. Indeed, with the light thrown

upon this subject by the multiplied operations for ovarian disease, is it not evident that the danger of a traumatic injury, *per se*, of the peritoneum has been enormously exaggerated? Is it not also true, on the other hand, that the mortality resulting from these operations has gradually lessened with the additional care taken by the surgeon to prevent the retention of blood or other septicæmic materials in contact with the peritoneum?

With these preliminary considerations we now feel prepared to advocate the doctrine that the frightful mortality of penetrating wounds of the abdomen, and the rapidity with which the fatal result is brought about, point directly and immediately to septicæmia or blood-poisoning, as the only condition at all adequate to the production of such effects; and that the blood-poisoning is the almost necessary consequence of the plan of treatment recommended by standard authorities and carried out in daily practice.

If the intestines protrude and are found to be wounded, we are advised to cleanse, stitch, and return them into the cavity; otherwise the case is to be trusted to nature, and perhaps treated by antiphlogistics with a view to prevent or to subdue peritoneal inflammation. In the mean time the blood necessarily effused, whether much or little, is left in contact with the peritoneum, rapidly undergoes putrefactive decomposition, and is as promptly absorbed. Magendie long since demonstrated the wonderful celerity with which agents were carried into the circulation through the serous membranes. But if the system may be contaminated by the retention of blood alone in contact with the peritoneum, what should we not expect from the addition of fecal extravasation. Assuredly not *inflammation*, but most rapid septicæmic poisoning. In view of the rapidity with which toxic agents reach the circulation through the peritoneum and other serous membranes, we may safely infer that the fecal poisoning begins very soon after the extravasation, and that it must have much to do with the rapid prostration so uniformly observed in such cases, and usually attributed to nervous shock.

The awful fatality of penetrating wounds of the abdomen revealed by statistical observations would seem to indicate the necessity for a change of practice, and I have accordingly been teaching for a number of years to the classes of the Medical College of Georgia the propriety of looking upon septicæmia as the paramount danger of such wounds, and of taking such steps as would be best calculated to avert this poisoning. The recent occurrence of several fatal cases in my neighborhood prompts me now to a more formal appeal to the profession for a reconsideration of the principles by which we have been governed in such cases, and to suggest that in all instances of penetrating wounds of the abdomen, in which there may be any suspicion of the existence of effusion of blood or feces into the peritoneal cavity, the surgeon should proceed immediately as follows:—

(1) Induce anæsthesia.

(2) Lay open the abdomen along the linea alba, freely enough to make a thorough inspection of the parts contained.

(3) Ligate all bleeding vessels as far as possible.

(4) Examine carefully the whole length of the alimentary canal, in order to detect any wounds, and to stitch them. If the intestinal wound be ragged, it should be trimmed down to a straight edge.

(5) In gunshot wounds reduce the channels of entrance as well as of exit through the abdominal walls to the form of incised wounds, so that

these may be closed and left to adhere by first intention. In some cases it may be found necessary for this purpose to remove the uneven track by a double elliptic incision.

(6) Cleanse the wounds and peritoneum of all extraneous materials, and close the abdominal walls with suitable sutures and adhesive plasters.

(7) Finally, control the peristaltic action with opium, and administer such nourishment as will leave least fecal residuum, as milk, eggs, etc.

As supplementary to these suggestions it may not be out of place to add that for anæsthesia I would prefer ether, as being less enervating and safer than chloroform; and that, for stitching the intestines, animal ligatures, of catgut or violin-strings, wet with an antiseptic solution, should be preferred.

These principles have been most successfully carried out by the writer and others in cases in which the wounded intestines have protruded; and yet we have allowed other cases to die simply because the abdominal wound has not been sufficiently large to permit the bowels to come into view.

If the practice now suggested be considered harsh and hazardous, I would ask what can be worse than the authorized plan, which makes *death* the rule and recovery the rarest exception? Is it not time that we should discard as groundless the fears heretofore entertained with regard to the danger of opening the abdominal cavity? No change of practice in the class of wounds under consideration can make the chances of recovery less than they are now; and I feel confident that by adopting the plan proposed we would so alter the results as to make *recovery* the rule and death the exception.

ON OPENING THE SAC IN HERNIOTOMY.

BY

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Both Paré and Franco, when performing Herniotomy, avoided opening the hernial sac when its contents could be reduced without its division. Their practice, however, continued to be exceptional until Petit, in cases of large and adherent hernia, left the sac entire, and till Key and Preiss adopted the method as a rule in all cases. It has been held by some surgeons that Sir Astley Cooper's plan was to leave the sac unopened in all cases. We think it will be found, from his recorded observations, that the examples in which he practised and advised leaving the sac entire, were such, in the main, as he termed, "old large ruptures." In his Lectures on the Principles and Practice of Surgery, edited by Frederick Tyrrell, will be found, in his terse description of the operation of incising the stricture in the inguinal form of the disease, all the details of opening the sac to the extent of "freely exposing its contents." He says, "if the stricture be seated in the neck of the hernial sac itself, of course the division of the part exterior to it will not relieve the strangulation; in this case, the sac must be opened carefully at the upper part, only so as to allow a division of the stricture." He also alludes to the advantage of "dilating the stricture without cutting the sac itself, as it exempts the intestine from all danger of injury from the edge of the knife." He advises, too, that the sac should not be divided higher than an inch below the abdominal ring, as its division near the abdomen makes the wound more difficult to close, and exposes the patient to greater danger from peritoneal inflammation. In his description of the operation for femoral hernia, Sir Astley concludes his directions for opening the sac in these words: "We should further divide the anterior part of the sac upwards and downwards." In strangulated umbilical hernia, he says that the peritoneal covering should be cut into, and divided in the same way as when operating for other herniæ. "In very large umbilical herniæ, when strangulated, the umbilical opening should be dilated upwards without dividing the sac."

From this brief reference to the doctrines and practice of this eminent surgeon on the question at issue, it will be seen that he generally opened the sac, and that while he expressed the belief, at least on one occasion, that herniotomy without its division might become the more successful and general operation, yet through his long and distinguished professional life, it was with him an exceptional rather than a general practice.

Petit's operation, called the minor operation, was adopted by Key in its most extended application. In discussing its merits, he says that in the majority of cases, in which patients die after this mode of operation, the death is caused by peritoneal inflammation consequent upon the exposure of an inflamed or strangulated portion of bowel. He says, too, that it is not so much the wound in the peritoneal sac that disposes to inflammation, as the exposure of the bowel to conditions to which it has

hitherto been unaccustomed. He also affirms that the inflammation following the operation for hernia spreads from that part of the bowel that has been strangulated, and does not have its origin from the incision of the sac, even if there are two wounds inflicted upon it, one to expose its contents, and another, higher up, to cut the stricture. Sir William Lawrence dissents from the opinion of Key, that recorded cases are "so largely caused by the operation of exposing the intestine, and its attendant exposure to air and light, change of temperature, and handling," and says that it is the presence of the stricture which affects the parts, not like the slight violence of the operation for a few minutes only, but uninterruptedly for hours, and sometimes days, disturbing the circulation, making an impression on the intestine as if it had been tied with a string, and sometimes causing ulceration of the internal tunics, or of the bowel in its whole thickness. Here, then, is the chief issue between Key and Lawrence; the former objecting to opening the sac, not so much from the fear of wounding it, as of the exposure of its contents to new circumstances—as air and light, change of temperature and manipulation—the latter fearing the effects of the stricture, acting continuously for a longer or shorter period, with efforts at taxis, and not the exposure of the contents of a divided sac for a few moments; and declaring that under all circumstances he is still disposed to continue the practice of opening the sac, believing it to be the most safe.

Luke, who advocates Petit's, or the minor operation, in describing it, refers to the different sites and textures in which the stricture may be; gives some rules to govern the diagnosis, admitting that the girding of the parts may be found, in the inguinal hernia of the male, at any point from the internal abdominal ring to the scrotum; and adds, finally, with regard to the necessity of opening the sac, when the stricture is caused by its thick and indurated neck, that, instead of a complete opening, only the exterior tissues of the thickened, hardened neck should be incised; when, he asserts, the neck of the sac is so far dilatable that the anterior pressure of the hernial contents will allow their return by the taxis, and directs that, in case the first efforts are not successful, this partial division is to be renewed, when by repetition of the process the strangulation may be relieved without entire opening of the sac.

Chelius, after having divided the covering down to the sac, directs that it should be "cut into, and the opening of the sac increased upwards and outwards throughout its entire length." He also says that the mode of operating in which the hernial sac is not opened at all, but only the abdominal ring dilated, so as to return the hernial sac together with the intestine, is in general to be rejected, and that the not opening of the sac is to be most especially confined to those cases in which it is certain that in a recently produced, or extraordinarily large rupture, or in a rupture entirely adhering to the neck of the sac, the strangulation is seated in the abdominal ring. Twenty-five years ago, Skey, in describing the operation of herniotomy, used these words: "By proceeding slowly through the various coverings, we reach the sac of the peritoneum. Sometimes the sac is drawn so tightly over the contents as to be separated from them with difficulty. We select for this purpose the lower part, which is usually somewhat distended by gravitating fluid, and here it may be opened without danger." In the fourth American edition of Fergusson's Practical Surgery, in the account of the operation for strangulated inguinal hernia, it is directed that, after having divided all the overlying tissues, so as to lay bare the outer surface of the sac,

"a small aperture should be cautiously made in this membrane, which should then be more fully laid open." Fergusson also says that, "in large protrusions, it would unquestionably be highly desirable to return the contents without exposing the peritoneal surface. In the majority of such cases the stricture may be divided without penetrating the sac, but in some instances, even after this has been done, the protrusion cannot be returned in consequence of adhesions."

Prof. Gibson, in the sixth edition of his "Surgery" describes the manner of opening the sac as "an important part of the operation of herniotomy." He refers to the operation of dividing the stricture outside of the sac, with his objections, and directs that the sac should be opened in inguinal, femoral, and umbilical herniæ. Miller, in discussing the question of opening the sac, says, "were it applied indiscriminately, nothing could well be conceived more pernicious." In the operation for femoral hernia, he says, "usually the opening cannot be avoided." In Erichsen's description of the general operation of herniotomy, he says, "the sac, having been exposed, must be carefully opened towards its anterior aspect, and if it be a small one, at its lower part." When operating for the inguinal form, he says, "if it be found after the division of the fascia that the stricture has been removed, and the hernia can be reduced, it would, of course, be unnecessary to lay open the sac, and the safety of the patient will be considerably enhanced, and particularly if the operation is performed for an old scrotal hernia of large size, by not doing so. . . . If, however," he adds, "as will happen in the majority of instances, in inguinal hernia, it be found that the stricture is in the neck of the sac itself, it must be carefully opened." In concluding his description of the more common operation for femoral hernia, he says: "When the sac has been reached, it must be carefully opened." In umbilical hernia, the stricture should, if possible, be divided without opening the sac. He further says that the operation without opening the sac may be practised in all forms of hernia, but is much more readily done in some varieties of the disease than others. It is especially applicable in cases of femoral hernia, in which the stricture is commonly outside the sac. In inguinal hernia, it is not so easy to perform Petit's operation; indeed in the majority of cases, the surgeon will fail to remove the stricture in this way.

Thomas Bryant says: "When the hernia cannot be reduced without opening the sac, the whole must necessarily be explored." It will be thus seen that the question of opening or not opening the sac, turns upon the necessities of the individual case. After some arguments against opening the sac, he adduces some statistics in favor of the minor operation, but adds that it must be remembered that, as a rule, the cases in which the sac was not opened were of a more favorable kind than those in which it was opened. In describing his plan of operating to cut the stricture outside the sac, he says that if no indications of reduction show themselves, or if part of the contents of the sac have disappeared, but not with the characteristic jerk, or if some portion has been left behind, the sac must be opened. "In all operations for femoral hernia, the reduction of the hernia without opening the sac should be preferred, and in recent herniæ this minor operation is generally successful. Should it fail, and the sac have to be opened, no harm can possibly have been caused by the proceeding, for the incision can readily be enlarged if necessary, the sac opened, and its neck divided." In describing the procedures of herniotomy, Gross directs how to recognize the sac, and then

adds, "An opening just large enough to admit the point of the director is now made, when, the instrument being carried upwards and then downwards, the sac is divided to the required extent." Referring to the advantages and disadvantages of division of the stricture without the sac, he concludes in these words: "It would not be proper in the existing state of our knowledge to pronounce a decisive judgment upon a subject of so much importance as this unquestionably is; we must wait for further data, and in the mean time adopt the rule to lay open the proper hernial sac whenever there is any reasonable doubt in regard to the site of the stricture. Having divided all the fascial layers down to the sac, in femoral hernia, it may happen that the stricture is seated within the sac, particularly if the hernia be large and old, and, when this is the case, the sac must of course be laid open, its division being effected in the same manner as in inguinal hernia."

Hamilton writes: "The sac being fairly exposed, a small fold may be raised at its most depending point, and an opening made large enough to introduce a director, and upon this the incision may be enlarged." Division of the stricture outside of the sac, if practised, should be reserved for exceptional cases. In femoral hernia, the sac being reached, opened, and the contents examined, then follows the treatment of the stricture." He adds that division of the stricture outside of the sac is probably more often applicable to femoral hernia than to either of the forms of the inguinal variety. "In umbilical hernia," he says, "when it has been necessary to open the sac, the result has generally been fatal. It is advisable, therefore, always to attempt the reduction by division of the stricture outside of the sac."

Our object thus far has been to place, side by side, the opinions and practice of the eminent surgeons to whose names we have referred, without a recapitulation of the arguments by which their conclusions are reached upon the questions at issue. The external operation has been before the profession, at least since its earnest espousal by Petit, in 1718. It found strong advocates in Monro, Key, Luke, and some others, but had not occupied the attention of surgeons generally until near 1833. It is true that, by the English surgeons, the minor operation was first most thoroughly tested, but for a considerable period it has been more or less under consideration by surgeons in all countries. It is now more than a century and a half since it became a favorite operation with some British surgeons, and for the last thirty or forty years it has been practised in Great Britain, on the continent of Europe, and in the United States. Has the minor operation acquired for itself that reputation as an exclusive mode of treatment for strangulation which its earliest or later strong advocates predicted for it?

We think that it will be found that a majority of surgeons who advise non-opening of the sac, reserve certain conditions as incompatible with the minor operation, and admit that in those cases division of the sac is positively required. Then, again, believing the case one favorable to leaving the sac entire, but finding as the operation progresses that reduction cannot be accomplished, they finally resort to cutting the sac. Among all the surgeons who prefer the minor operation, we do not call to mind one who denies the necessity for opening the sac when the stricture exists in the latter structure.

In the statistics on this question, in which the results appear more favorable for the non-opening of the sac, it is conceded that the cases

were of a more promising character for either mode of procedure; this, we think, raises the question, whether the conditions of a case which entitle the minor operation to be preferred, can be recognized before the operation. While some of the average conditions of strictured inguinal and femoral herniæ may be pretty well determined before exposure of the interior textures, the writer's experience compels him to say that more of the important relations of the structures, as well as their pathological conditions, cannot be so well known while in this occult state. If a century and a half, or more, of well-directed surgical acumen had determined the fixed, uniform, pathological conditions in the disease under review, then could our diagnosis enable us much better to say when a strictured protrusion could be successfully reduced with its sac intact. The majority of examples of hernia, it is true, embrace primarily only definite anatomical structures; but from the first day of their history their relations are liable to change, and, with these, occur functional aberrations and structural alterations. The age of the patient; how far the disease is congenital or acquired; whether it is indebted in any way, and if so how far, to strictly traumatic causes; the site of the rupture; the contents of the protrusion; the distance it has gone from its normal home and the direction taken; the volume of the mass; accidents to which it may have been subjected; the varying of the sac from extreme tenuity and pliancy to great thickening and induration; its range of contiguous relations to either or both its surfaces; the more or less frequent transit of the contents through the abdominal apertures in either direction; and occasionally the permanent extra-abdominal situation—are some of the circumstances which will, to more or less extent, influence the case when strangulation exists, bearing directly upon its treatment, both by the taxis and herniotomy, and which will determine largely the results of the latter. The history of all forms of hernia is made up of varying conditions which we believe must contribute to pathological changes, and aid in individualizing cases to an extent to entitle each and every one to its own consideration.

These are some of the reasons why the operator cannot predict positively what are the morbid changes in a given case of the disease, as long as these are hidden from his vision. With his increased facilities for observation he is all the more certain of disappointment if he expects to find marked sameness in the pathological appearances in a number of examples of either of the leading forms of the disease, but more especially of inguinal hernia. What surgeon, before he commences the operation of herniotomy, is quite willing to specify what will be the departures from normal anatomy, either in tissue or relation, until disclosed by his incisions? While he is positive enough concerning the presence of some of the conditions, he is obliged to admit that he is doubtful of others, and these are most likely to be the important factors in the results. These ever-varying phases, sure to be present to a greater or less degree in a great majority of the examples in every variety of hernia, are never to be ignored, but carefully analyzed and considered, whatever treatment is chosen; and until we can make our diagnosis more satisfactory touching the interior state of the sac and its contents, including their true condition and relation, we shall be compelled to accept the major operation instead of leaving the sac unopened. It will be conceded by all authorities, we think, that no part of a hernial tumor or swelling is of such high importance as the contents of the sac, and that these, more than all others, are the textures both primarily and secondarily imperiled.

when strictured. These are the parts which our treatment seeks to relieve as early as possible when in this state. The taxis having failed, the girding point or points are found and divided. Has this been done outside and the protrusion returned, with or without manipulation, it may be well, it may be otherwise. This will depend wholly upon the fitness or not of the returned parts to occupy their original place in the abdomen. How is this to be known? It may be said, because the hernia was reduced so readily after the stricture was relieved. Does it follow certainly, from this ready return of the mass into the abdomen, that it was free from changes that would not permit it to remain there safely, or that it might not in this returned state prove more serious to the patient than if allowed a place outside the abdomen after the stricture had been divided? Our own observation has taught us that the readiness with which the protrusion is reduced after the minor operation is by no means reliable proof that the returned parts are in fit condition to allow recovery, as proved by inspection after death. But what does it mean when all stricturing tissues exterior to the sac have been severed, and the hernia cannot be reduced? We know no way to answer this inquiry but to open the sac and expose its contents, when, if there be any form of internal binding, it may be readily reached and overcome, and reduction accomplished or not, as the state of the strangulated parts may warrant. In the operation of herniotomy, when the sac is made bare, how far will the vision or tactile ability of the surgeon enable him to say what is the real state and relation of its contained textures? We answer for ourselves that it is scarcely more than inferred, and when we have succeeded in reducing an inguinal or femoral hernia in this way, our solicitude remains at high pitch until sufficient time has elapsed to prove that all signs of strangulation are well over.

What are the objections to opening the sac? Perhaps the one most commonly urged is, that it increases the liability to inflammation in the sac itself, and its extension along the peritoneal tissue. Allowing that this texture is readily inflamed from slight causes, we cannot see as a rule how the dangers incident to a small incised wound in the sac should be as great as those incurred by returning the bowel in its average condition, in this kind of cases. In the minor operation, provided there be no other strictured point beyond that outside of the sac, how readily are the protruded parts restored to the abdomen? Granting the usual proportion of examples of adhesions of the sac more or less to structures extra-abdominal, and that the contents are in a state to move freely within the sac, then we think that the manipulations requisite for reduction having to be made through the walls of the sac, the whole depending upon the touch, are so much embarrassed that much greater risk of injury is incurred than by the wound necessary to bring at once into view the true state and relation of the already, it may be, highly sensitive contents. Again, when we think we succeed well in returning the contents wholly into the abdomen, in cases of adherent and hypertrophied sac walls, by careful watching, we may well doubt whether there may not be some portion of intestine not quite within the abdomen. We remember that more or less exudation has occurred during the period of strangulation, and that a fold or process of intestine or mesentery, or even a portion of omentum, may easily enough, in the midst of the more or less plastic effusion, adhere both just below and quite within the upper aperture, and so cause an incomplete reduction. In these examples much of the fulness may disappear, leaving only so much as can well be explained by

the firm thickened sac, with often a marked feeling of relief so expressed by the patient, but soon to be followed by an exacerbation of the symptoms of strangulation. Under most circumstances, our choice would be to accept the risk of an incised wound in the hernial sac, rather than too long trial of the taxis before the coverings have been divided, or its application in the average state of cases after the stricture has been cut without dividing the sac. We cannot see how a wound in the hernial sac is likely to be followed by more inflammation, or any more serious results, than a wound of the same tissue in any other place, beyond what would be inevitable from the already morbid state of the within and adjacent structures. When we call to mind how rare an event it has been, in the clinical history of the operation in question, that incising the hernial sac has added to the inflammation, or contributed in any possible way to the untoward results of the case, we declare our conviction that in a large majority of examples of inguinal and femoral strangulation, the major operation is the safest. We put the question to all surgeons, whether their cases of herniotomy which have been fatal, have not been so from morbid changes already present before the division of the sac and stricture?

Another objection to this operation of opening the sac is, that it exposes the contents to accession of air and light, with change of temperature, suddenly creating new relations which cannot be tolerated. We cannot understand why the peril should be any more by the exposure in this operation than in others in which a much larger portion of the peritoneal surface, with a far greater amount of visceral texture, is suddenly brought in contact with the atmosphere, as amply illustrated in the operation of ovariotomy, and other constantly occurring examples of traumatic lesion of the abdominal textures. We believe that in a large majority of instances, neither the liability of adding an increase of inflammation by opening the sac, nor that of multiplying dangers by the exposure to the air and light for a few moments, of the interior textures, will add as much to the dangers as the untoward manner of applying the taxis with the sac-wall entire, together with the great liability to unfitness of the contents for a safe restoration. If it be said that the views inculcated in this paper are at variance with a commendable spirit of surgical conservatism, then we affirm our belief that it is a much more conserving practice to expose by a well made incision the real state and relations of the suffering tissues, by which every subsequent step for relief can be aided by direct vision, than to adopt a procedure which must be mainly directed without the aid of the surgeon's eye, and commonly, if it effect reduction, requiring a more protracted tactile labor, and always involving the risks of unrelieved strictures, and too often of structures returned to the abdomen, which should have been left outside. The writer, restricting himself to only the authentic clinical histories of a considerably large number of cases, with their results, in which both non-opening and opening the sac were practised, and to the pathological facts furnished by autopsies in a majority of the fatal cases, cannot truthfully arrive at any other opinion than the one already expressed. It must be admitted that there are many surgeons whose opinions are entitled to the highest respect, who would not adopt the major operation as exclusively as we have indicated. The writer regrets to say that he has met with more hindrance than he had anticipated in obtaining from surgical practitioners their individual experience upon the respective merits of the operations in question. It is the honest difference of senti-

ment among surgeons which has chiefly led the writer to call attention to this very important practical question, so that if he cannot reflect new light upon his theme, other surgeons may be induced to make a more free declaration of the results of their experience upon this important subject through the legitimate channels of communicating professional truth.

From what has been said, we conclude—

I. That the examples in which the minor operation is most successful are those but slightly removed from a condition permitting reduction by the taxis, and that the length of time during which strangulation has existed is not of itself the true measure of the condition of the strictured parts, these often presenting more morbid changes when strangulated but a few hours, than in other instances of days' duration.

II. That cases which furnish the most favorable results from the minor operation, are also the most promising for opening the sac.

III. That there are many cases of strangulation in which the stricture cannot be relieved without division of the sac.

IV. That our means of diagnosis are insufficient to enable us to say, in a vast number of cases, whether the strictured parts are in a suitable condition to be returned to the abdomen while the sac is entire.

V. That a due regard to the credit of herniotomy, when every variety of inguinal and femoral strangulation is included, requires that the major operation should be adopted in a large majority of patients in this condition.

VI. That opening the sac in strangulated umbilical hernia is not to be practised if reduction of the protrusion can be accomplished without it.

SOME COMMENTS ON THE HISTORY OF NITROUS OXIDE GAS AS AN ANÆSTHETIC, AND ON THE ANALGESIC EFFECTS OF RAPID BREATHING.

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It would seem impossible that a suggestion of such a practical character, of such immense value to mankind, and made by such authority, as that proposed by Davy of using inhalations of a gas to destroy pain in surgical operations, should go absolutely unheeded for nearly half a century, and yet such was really the case; for Sir Humphry Davy, in a publication, in 1800, of "Researches, Chemical and Physical, chiefly concerning Nitrous Oxide and its Inspirations," suggested from inhalations of that gas made by himself and others that "as nitrous oxide in its extensive operations appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place." This suggestion he never recalled or condemned during his subsequent brilliant career, when everything emanating from his fertile brain was sought after and followed with avidity and zeal. There was no pretext of want of desire for such a means; the practice was almost universal of resorting to the effects of alcohol and opiates to deaden and as far as possible destroy the sensibility to pain during operations. The evil effects of the latter practice were also fully recognized, and yet, as I have said, this proposal of Davy was absolutely allowed to go unheeded and to become forgotten, for, when Dr. Horace Wells, a dentist of Hartford, Connecticut, in 1844, thought of using nitrous oxide gas in extracting teeth, no one was then ready to condemn it on the ground of previous failures, or because it had been proposed so long before.

Wells's idea would seem to have arisen in his own mind on witnessing an effect of the inhalation at a public exhibition given by a travelling lecturer. On this occasion, one of the persons to whom the gas had been administered, whilst under its influence, struck his foot, and on recovering his consciousness it was discovered, by Wells's inquiries, that he had sustained a severe bruise without being aware of it from any pain at the time. This fact led Wells to the inference that the gas could produce insensibility to pain, and he determined to test this by having a tooth extracted while under its influence. The lecturer was invited to his office to administer the gas, and during this administration Wells had the tooth drawn without experiencing any more pain than is produced on a person in a state of perfect consciousness by the prick of a pin. This result led Wells to pursue the subject further; he tried the effect of the gas in a number of cases of tooth extraction (about fifteen), but with varying success; he then went to Boston and got permission from Dr. Warren to make a demonstration of the process in tooth-pulling before the class of the Massachusetts General Hospital. This attempt was an

utter failure, and was called "a humbug affair." Wells went home, told his friends that the gas would not operate as he had hoped, "and wholly ceased to experiment from the date of his failure in Boston (in December, 1844), until the spring of 1847."[1]

In the interval between these two periods, complete demonstration had been made by Morton, his former partner in dentistry, of the power of the vapor of ether to produce the desired insensibility. Morton had no doubt had his zeal to discover some such agent excited by Wells's previous successes; he would appear to have been impressed with the idea that these were essentially the effects of imagination on the part of the patient, for on September 30, 1846, he went to the laboratory of Dr. Jackson, whom he knew well, having been a student under him and recently in his house, and took from a closet an India rubber gas bag. In reply to an inquiry of that gentleman, he said in substance "that he meant to fill the bag with air, and by its aid extract the tooth of a refractory patient." A conversation ensued upon the effects of the imagination and, among other things, of nitrous oxide, in producing insensibility.[2] Jackson treated Morton's proposition lightly; he told him to go to an apothecary and procure sulphuric ether—the stronger the better—which would produce the insensibility he desired. The ether was to be spattered on a handkerchief and inhaled; in a moment or two perfect insensibility would be produced. "Sulphuric ether," said Morton (so goes Jackson's account), "what is that; is it a gas? Show it to me." Jackson said he showed him some ether, and, after further conversation, Morton went to procure it.[3] Morton and his friends subsequently claimed that his expressed ignorance of ether at this interview was merely assumed, to prevent Jackson from setting up any claim to participation in his own search for painless dentistry. He showed by the testimony of Dr. Gould and of Messrs. Weightman and Metcalf, that he was before this time a dentist who was experimenting upon the relief of pain in dental operations, and that he was familiar with the properties of ether. Not only so, but by subsequent experiments he proved that to be certain and safe which may have been thought by others previously to be a possibility.

Morton possessed traits of character essential for success: he was earnest and persevering beyond conception, as we are told by Professor Bigelow, who was personally acquainted with him, a participator in many of the steps in the early progress of the discovery, and who did much to bring it before the medical public of this country and of Europe, to assert its real value, and to point out the best methods of utilizing it.[4] Morton possessed these traits to an extent evidently far greater than his former partner, Wells, for, as I have before stated, the latter, disheartened and discouraged by his failure at the Massachusetts General Hospital, abandoned his investigations and searches "for more promising enterprises." When Morton had got sufficient confidence by his trials with ether, he went, like his partner, Wells, to the Massachusetts General Hospital, in October, 1846, and made a demonstration there of his process. This demonstration was an eminently successful one. Within two weeks the whole world was ringing with wonder at the discovery.

[1] Bigelow's History of the Discovery of Modern Anæsthesia, American Journ. of Med. Sciences, Jan. 1876.
[2] Bigelow, loc. cit., p. 171. [3] Ibid.
[4] Gross, Century of American Medicine, Am. Journ. of Med. Sciences, April, 1876.

By many its possibility was denied; by others it was ridiculed; by others its claim to originality was attempted to be ignored; and, finally, it was preached against from the pulpit, as I heard myself, as being, in one of its proposed applications, viz., to midwifery, an attempt to defy the orderings of the Almighty.

Morton, at once, on his first success at the hospital, seeing in the thing a source of great profit, wrote to Wells stating that he was about sending out agents to dispose of the right to use his discovery, and offered him an agency on shares. Wells, in his answer to this letter, says, "If the operation of administering the gas is not attended with too much trouble and will produce the effect you state, it will undoubtedly be a fortune to you, provided it is rightly managed."[1] Here the question may very naturally arise: Have we not in this answer of Wells a clue to the causes of his abandoning his searches after anæsthesia? The administration of nitrous oxide was "attended with too much trouble," and he had not rightly managed it to make it "a fortune" to himself; for our (the medical) profession must not in judging of the conduct of two members of another (the dental) profession overlook the fact that the latter consideration can be entertained by them and not by us. The former consideration, that of "too much trouble," had no doubt always been in the way of using nitrous oxide, from the day of Humphry Davy's proposal to that of Wells's abandonment of the idea. It was indeed an obstacle to the general use of ether, at first, for then it was supposed that it could not be confided to any but an experienced administrator for whose service an extra charge had to be made.

Morton's demonstration that the secret of success with ether lay in the full quantity of the vapor used was slow in being recognized, and the dread of fatal asphyxia was so predominant in the minds of the majority of practitioners, that for many years after the introduction of anæsthesia the fact of its being unsuited to a large number of cases (other than those of heart and lung disease) was considered to be beyond all question. The partial recognition of this secret with ether, led many (and possibly Wells amongst them) to the idea that the failures with nitrous oxide had been from the insufficiency of the amount used. The "traditional exhilarating gas bag and Davy's exhilarating dose were what had heretofore been used, and they were inadequate to produce anæsthesia with any certainty."[2] The first demonstration of this fact was in a breast excision performed by Prof. Bigelow at the Massachusetts General Hospital, in April, 1848, by means of about sixty gallons of gas.[3]

This, however, did not give nitrous oxide its true position as a so-called anæsthetic agent, as for years afterwards it was ignored entirely. This may have been from the enthusiasm which existed for ether and chloroform (for the latter was proposed by Prof. Simpson, of Edinburgh, in 1840), and yet the "too much trouble" in its administration also undoubtedly exerted an influence against nitrous oxide. It was in fact nearly nineteen years from the time of Wells's first experiment until G. Q. Colton, the same lecturer who had administered the gas to Wells on that occasion, recognizing its real value and that the secret of its success lay in an abundant use of it, secured the aid of a competent extractor of teeth and then made it known as a safe and popular agent in

[1] See Morton's letter of Oct. 19, and Wells's answer of Oct. 20, 1846.
[2] Bigelow, loc. cit.
[3] Ibid., p. 168.

painless dentistry.[1] Before that time dentists advertised frequently "to extract teeth under ether," or "to administer sulphuric ether," or "chloroform," but no one thought of or used nitrous oxide. It was said "not to be as successful as ether;" "not to be a safe agent;" "to be more troublesome;" but since Colton's successes we have "nitrous oxide gas administered" advertised on a host of dentists' windows, and those dentists who do not administer it themselves, do not hesitate to send their patients to establishments gotten up exclusively for that purpose—many of them designated as "Colton Institutes" in recognition of what Colton did to popularize the matter. More than this, since then, the surgical profession has recognized the advantage of the use of nitrous oxide gas over ether in a great variety of the minor operations of surgery, notwithstanding the inconvenience from so much trouble in its administration. Hosts of surgeons have experienced the annoyances and inconveniences of using ether or chloroform in their office practice, from the "post prandial" effects of those agents, and recognizing the absence of such with nitrous oxide gas, have since then availed themselves with avidity of the proximity to their own offices of a "Colton Institute," or of an administrator of nitrous oxide gas, to employ the gas in many cases of ophthalmic surgery or of operations of short duration which they could perform in their offices without detriment or inconvenience to their patients, provided that they could do them in a painless way.

I confess myself to have been a full participator in this feeling, and yet this was from no want of appreciation of the value of ether or chloroform; all who have known me have always been aware of my being a most enthusiastic friend to these agents, and especially to ether. I was as a student a witness with my father of the first public demonstrations of the use of ether in Philadelphia. He was at that time the honored president of our most venerable medical society; he had held that position for over twelve years, and was over seventy-three years of age, and yet he was a most zealous student, always ready to adopt any measure which held out a prospect of alleviation to the suffering of his patients; but he was also possessed with the prudence and caution common to such advanced years, and of this I had the full advantage in hearing his comments after each clinic, and his discussions with his friends at home. He had been educated in England before Davy's proposal was made, when the practice of resorting to intoxication by alcohol and opium, as a preliminary to major operations, was very common, and he was not slow at the time to which I have referred to perceive the relations of these with ether and chloroform in the cyanosed or carbonized state of the blood produced by them; and he considered the state caused by the inhalation of ether as one of greater inebriation than that produced by alcohol. He therefore argued that the vapor of the former should be applied persistently until that extreme state was brought about, but not so abundantly or exclusively as to induce a state of asphyxia.

Such a view then (my father died in 1848) was far in advance of that held by the practitioners of that day. The cyanosing of, or retention of carbonic acid in, the blood, was then looked upon as an accidental and unfortunate circumstance in a case, and I therefore refer to his view as an evidence of the advanced schooling I had on the subject. My zeal for the use of anæsthesia whilst a student on one occasion subjected me to what was meant to be a severe censure before a class of fellow students. It

[1] See Thomas's Manual on Nitrous Oxide, p. 17.

was on the occasion of prolonged efforts of over two hours to reduce a dislocated hip-joint at the clinic of one of our most conservative institutions, and when whiskey and laudanum had been most freely used; several of the class, myself amongst them, had been asked to lend a hand at the pulleys, and we had all failed. I, on failing, said *sotto voce*, to the surgeon, "Doctor, why don't you give him ether?" And he turned round in a most excited manner to the class of nearly two hundred students, and exclaimed, "Gentlemen, one of your number asks me why I won't give this man ether? I would as soon cut his throat." Within twelve hours after this, the ether was given to the man, and his hip reduced, apparently in a moment of time; but I, as an interloper, was there amongst a few to witness the result.

For the past twenty-four years, as a hospital surgeon in Philadelphia (at the Hospital of the Protestant Episcopal Church, at Wills Hospital, and at the Pennsylvania Hospital), I have never lost an opportunity of using anæsthetics, and no one can, therefore, even insinuate that I have been wanting in appreciation of their value. In private practice I have used freely ether, chloroform, nitrous oxide gas, and local anæsthesia (by the atomizer, and by ice and salt and kindred articles), and yet I am free to confess, as I said before, that I have longed for some easier method, or some method free from after annoyances, with which I could save my patients, especially in minor operations or physical explorations, from pain. It is, therefore, very certain that I most gladly availed myself of Dr. Wm. G. A. Bonwill's first suggestion to me that simple rapid respiration would produce insensibility to pain, if prolonged for from three to five minutes, and carried on in as rapid a manner as possible during that short space of time. I tried it first in a case of necrosed femur, where there was a state of great hyperæsthesia, and where I was particularly desirous of avoiding the production of pain; and when (in my office) I did not want to resort to ether or the like. Here I had most perfect success, using the probe freely with the sounding-board appended to it, and still causing my patient not a particle of suffering. I repeated the experiments with like cases in the wards of the Pennsylvania Hospital, then tried the procedure in other minor cases, but not always with the satisfactory results that I obtained in the first of them; the failures, however, were not such as to deter me from the further pursuit of the subject.

My former experiences, with the first attempts at anæsthesia with ether, were such as not to suffer me to become disheartened here. Who is there, who went through the early history of ether, who cannot recall, as I before intimated, the many failures to produce complete insensibility with that agent then, which would not be suffered to occur at the present day? It was then a novelty, and those who used it were ignorant of all the essentials for its invariable success. Such I felt was the case with my experience with rapid respiration; I had a good deal to learn even in what looked like a very simple affair, and seemed to require no experience or even judgment in its use. My first attempt to demonstrate its value before the class at the Pennsylvania Hospital showed this very clearly. I then had brought into the clinic room two boys for explorations with the probe, on whom I had tried the process in the ward with most satisfactory results. The first one of these was remarked by me at the time before the class as "a bright little fellow, who looked at me very cheerfully on that day, and in this respect was very different from what he had been at the beginning of our interviews

some days before;" he then had dreaded the probing. He had been before the class more than once, and had had on one of those occasions ether administered for an exploration by my predecessor, who then uncovered the tibia by a free incision, and, demonstrating most clearly its necrotic state, removed a large portion of it by the chisel and forceps, aided by the trephine. The little fellow, on the occasion of my lecture, espied, as he came into the room, the ether bottle and a lot of instruments on a side-table, and nothing could induce him to believe that it was not my purpose to deceive him, and to perform some operation with the instruments when he should become insensible. He, however, started to breathe rapidly, but kept glancing constantly at the table till I had it removed out of his sight; but his dread was too predominant; he began to cry, and finally positively refused at all to breathe with any exertion or rapidity. I left him and turned to the other boy, who had never been in a clinic before; but he, participating in his companion's dread, followed his example, evidently for the purpose of making a like escape from having anything done with his case.

The reporter of my lecture on this occasion, Dr. T. H. Bradford, one of the residents of the Pennsylvania Hospital, then volunteered to try the process before the class. "It was his first attempt, and was made sitting erect, with his right hand resting upon a table. Breathing rapidly was attended first with a tingling sensation of the surface, especially of the fingers, and a feeling as though the surface was all swelling. Then (in about three minutes of time from the beginning) there followed a dizziness or compression in the head, with consciousness well preserved, but with a feeling of inability to resist, or to act in an independent way." I had directed this gentleman, before he showed the least sign of loss of sensation or consciousness, to raise his left hand as soon as he felt me touching him on the right. At this point, when he began to become cyanosed, he raised the left hand, and, continuing the motion, got to swinging the limb from the shoulder, much to the amusement of the class, and so afforded a very positive proof that he was not perfectly himself. In the continuation of his report,[1] he says: "He remembered well being frequently asked by [me] the doctor, if he was hurting him, but had no recollection afterwards of the pin sticking him, much less of its having been firmly imbedded in his flesh, as he found it when he had ceased the rapid respirations, and the anæsthetic effect had passed off."

The failure with the two boys was clearly the result of emotional excitement and want of will on their part; obstacles to success readily anticipated in any such attempt, but which do not present themselves now in the use of ether, chloroform, or the like, for with them we do not hesitate to, and can, compel the patient to take the anæsthetic; we are independent of his will; but such was not considered to be the case with those agents when they were first used. It was then thought necessary to have co-operation from the patient. I saw many a case in the early period of anæsthesia, where the attempt was abandoned for want of this. It is not at all possible that we shall ever be so independent with rapid respiration. Confidence in its success will, however, as it has done already with me and my patients, diminish the frequency of such a cause for failure; I have profited well from this part of the lesson of that day. But the failures then also gave me, by the opportunity with my assistant, to see that *complete anæsthesia* was not to be sought

[1] Medical Times, March 4, 1876.

by such a procedure, and likewise, very positively, that it was not essential for our purposes, for it must be remembered that I was not in search of an *anæsthetic* agent to supersede those like ether and chloroform, but one most convenient to destroy the sensibility to pain, and that alone.

The destroying of pain merely was certainly more desirable than the state of full anæsthesia from ether, with its accompanying loss of all consciousness and will. Appreciating this advantage, then, from the rapid respiration, I had the opportunity at the following clinic of demonstrating it in a most satisfactory manner. During that clinic it was announced to me that a man was in the receiving ward—had just come in—with, it was supposed by the nurse, a dislocation of his shoulder. Without going to make any preliminary examination, I directed him to be brought before the class. As he came into the clinic room, I called the attention of the class to his carriage; to the manner in which he supported the injured limb at the elbow, by the other hand; to how he leaned forward on that side; and to how the shoulder was depressed and the elbow projected away from the body—all affording very good grounds for the supposition that his shoulder was dislocated. When he was stripped, these appearances were still more marked, and during the undressing he seemed in great pain. Then I announced to the class that I would proceed to demonstrate the existence or non-existence of dislocation without inflicting any further pain on the patient, and without touching him. I then directed him to breathe rapidly, assuring him that it would remove his pains. He did it promptly, and in the usual time began to show its effects. I then told him to raise the hand of the injured limb and place it on his head. This he did without any signs of suffering. Then, whilst he was still urged to keep up the rapid breathing, he was directed to place the same hand on the opposite shoulder; then down by his injured side; and finally behind his back. All these movements he made, sluggishly it is true, but with an absence of all indications of pain. I, of course, then made free comment on the advantage we could have in all such cases with this mode of destroying pain, over that by ether or the like.

This condition of "insensibility to pain with conservation of intelligence, of consciousness, and of voluntary movement," recognized by recent authors, by the term of *Analgesia* (from α, privative, and αλγεω, I feel pain), in contradistinction to the loss of all these conditions to be designated by the generic term of *Anæsthesia*, or what I called a few moments ago complete anæsthesia, was shown at the French Academy of Sciences, in March, 1872, by M. Guibert, to follow the *partial* use of chloroform inhalations, some time after a hypodermic injection of morphia; and he then advocated the state as sufficing for obstetrics and minor surgery. The non-recognition of the occurrence of such a state, or the want of appreciation of its value for such purposes, has, I know, led many to condemn a resort to rapid respiration; and then again, many perceiving the preservation of intelligence, of consciousness, or of voluntary motion, have condemned the process as incapable of producing insensibility to pain. They have evidently not been aware of the researches of Bernard, Guibert, and others in England, as well as in France, on this subject. The fortuitous experiment with my assistant, Dr. Bradford, as I intimated before, was the first circumstance to open my eyes to the value of the distinction. With the use of ether, and the like, I was always accustomed to look for perfect insensibility of the conjunctiva as evidence of my patient being fitted for me to begin any operative

procedure. With that condition we all know there is always the perfect loss of those mental and voluntary conditions which belong to complete anæsthesia. (The combination of a previous hypodermic injection of morphia with the anæsthetic, as suggested first by Bernard, holds out no inducement for a resort to such in the cases to which I have been referring.)

In my earliest trials of insensibility by rapid respiration I had not thought of Guibert's distinction of the two states. In many of these the object was to avoid suffering in trifling operations—operations strictly of minor surgery; but, after Dr. Bradford's trial, I could not refrain from seeing its value beyond cases of dislocation, even in really major operations about the pharynx, on the tonsils, uvula, posterior nares, and Eustachian tube, as well as on the tongue and maxillary bones; for in these the anæsthesia from ether and chloroform, when attempted to be used, has always to be abandoned (particularly in the most extensive of them) on account of danger of strangulation from the absence of any volition by which the patient can expel the blood from the mouth. The mode of effecting insensibility to pain by rapid breathing is also not at all in our way as we proceed in these operations, which cannot be said when ether is used. In such cases we can, as I know from experience, also keep up the insensibility produced by rapid breathing to the end of the operation, even when there are frequent and prolonged interruptions from hemorrhage, by urging the patient, immediately after or during such, to blow out as rapidly as possible. In some such instances I have noticed the patient, either impressed with the advantages or acting mechanically as some would suppose, carry on this breathing most earnestly until assured there was no further necessity for it. Here we have also another advantage: we can always stop the procedure. I have never seen a case in which I could not, and have, therefore, in instances where it became desirable to interrupt the insensibility, always effected it very readily; and this advantage is a very great one even in other cases than those of operation about the air-passages. Thus, for instance, as Guibert says, analgesia has advantages for obstetrical cases. I have in my own experience seen the advantage of this breathing process, not only in the insensibility with co-operative action on the part of the patient in ordinary labor cases, but also in the facility of terminating it, as, for instance, in the supervention of flooding where it becomes desirable to remove all causes of muscular relaxation, which cannot be done immediately where other processes of insensibility have been used. Here I know that I have been able to save patients' lives, where I feel, in these special cases, it would have been impossible to do so if they had been under the influence of ether or chloroform. All instrumental labor, as I know, can be carried on with perfect satisfaction under this process.

Most operations about the uterus, the vagina, the bladder, and the rectum, are also disarmed by this process of one of their terrors to the female, as they cannot be as surely in the case of ether or the like, viz., that of unnecessary exposure. I have frequently, in the past year, dilated the uterus by an Atlee's or Ellinger's dilator, straightened and replaced it, explored it by a Simpson's sound and other means, dilated the vagina by a trivalve speculum, and dilated the urethra and rectum, in the former of these last two instances to the extent that I could pass my index finger into the bladder, and in the latter so that I could pass three or more fingers into the bowel, with my patient lying quietly on her left side (in the ordinary obstetric position), evidently satisfied that she was not being

exposed, and with friends about her able to assure her that the operation was performed in such a manner. All such operations I have been able to perform in this way without pain, and many of them are as severe tests as could be required of the *analgesic* effect of rapid breathing.

It has been universally recognized that the relaxation of the sphincters is one of the best evidences of *anæsthesia*, and experienced surgeons early taught that it required more thorough etherization for operations in these parts than in others on account of their hyperæsthetic condition. Any one who has attempted the rapid dilatation of the urethra—the recently proposed cure for irritability there in females suffering with many forms of womb trouble, which Professor Charles D. Meigs used to describe as a pure *sphincterismus*—can sustain that view. On one day, in February last, I performed this operation most successfully in three different cases, the patients living ten miles apart. In two of them I tested the rapid breathing, and in the other I used no means whatever, on account of disinclination on the part of the patient, to prevent the pain. The former have no recollections of having gone through a severe operation, and have frequently expressed themselves as being most willing to undergo the procedure again should it be required, whereas the last declares that nothing could ever induce her to submit to such pain again, although the operation gave her the desired relief.

Amongst my operations for fistula in ano, under this means, I had one case which had been operated on once before by the bistoury, and without any anæsthetic. This patient actually laughed when I told him the operation was done, and at first he was disposed not to believe me. In his case I used the old-fashioned instrument for the purpose, a *Syringotome*, one that has been used for this purpose by my ancestors for over a hundred years. It is nothing but a steel, sickle-shaped blade, a quadrant of a circle of two and a half inches radius, with a long and very flexible, silver probe-end. It renders the operation a very simple and quickly performed one. My operation was not through the track of the former fistula—it had been well cured—but for one on the other side of the anal orifice, and which seemed like a very much less serious affair than the first evidently had been. All these circumstances are worthy of being noted in a report of this man's comparison of the two operations. In my operation he had no pain; in the other, he said, he had had a great deal.

I could prolong this paper, already sufficiently long for the character of such communications, by entering into details of a variety of cases other than those to which I have alluded; but I have certainly given enough of facts to induce others to give the method a fair trial, and if I shall have succeeded so far, I shall be perfectly satisfied; my purpose has been to earnestly fix the attention of the profession to this subject, with the conviction on my own part that the plan will prove a *desideratum*, even with the many means of anæsthesia which we possess.[1] It is to be earnestly hoped that none will show themselves too ready to condemn or

[1] Here I would again most distinctly repudiate all idea or purpose of even proposing to substitute this for ether or chloroform in major operations (save those about the mouth). I have used it in some such where it was not convenient or expedient to use either of those means, or where, I will confess, it was my object to test this in a severe manner; and the results, as I have mentioned, have been satisfactory, but not such as to demand its use in the place of those means which can produce a more thorough insensibility or complete anæsthesia than I have claimed for this.

ignore this process, as was the case in the early history of ether and chloroform, from failures in the first attempts, much less from the inability, as some say, to see any philosophy in it, or, as some have expressed themselves, from its being too simple a measure not to have been discovered and used with satisfaction before this late date if there had been any merit in it. We have seen the full value of such objections in our retrospect of the history of nitrous oxide and of ether, and surely no one at the present day is going to suffer himself to be misguided by want of zeal or by excessive conservatism in his profession. To prevent the occurrence of these, I will now give minutiæ, as far as my experience will allow me, as to the way of bringing about this insensibility to pain, and also what facts I possess towards the modus operandi of the process, or showing any rationale for it.

In operations about the air-passages, I may also say about the face and scalp, as well as about the ear, and even about the eye and its appendages, the sitting posture is the one to be preferred. This, however, is not, according to my experience, the most advantageous one for the induction of the analgesic state by this method. In all instances where I can, I now adopt the recumbent posture on one side, as in such a position the desired state is I find more promptly induced. To prevent the diversion of the patient's mind from his part in the matter, it is frequently beneficial to cover the face by a handkerchief: this may, in some instances, prove disadvantageous, from the idea the patient may take up that he is really to be etherized; in other instances, it may be in that way of service.

Following strictly Dr. Bonwill's directions, I always at first told my patients "to breathe as deeply and as rapidly" as they could;[1] but studying out their cases I was led to see that the rapidity of breathing was the most essential part, and that its rate of frequency was unavoidably at the expense of depth; a deep or full breath of course requires much longer time than one of an opposite character, and it is also essentially an inspiration; whereas a rapid breath partakes more and more, as it is increased in frequency, of the characters of *expiration*. This is not to be wondered at; it is clearly due to the difference of circumstances of the two movements. All the circumstances concerned in respiration favor the act of expiration. Both the elasticity of the lungs themselves, and that of the walls of the thoracic cavity, aid the expiratory movement, whilst they offer a corresponding resistance to the inspiratory; and, besides these, we have also to take into consideration the atmospheric pressure over the surface of the chest which is not only favorable to expiration, but which increases the resistance to inspiration as the depth of the latter is made greater, and, according to Dr. Hutchinson's researches,[2] this is in an arithmetical progression. These conditions, it would seem from Dr. Carpenter's calculations,[3] require a muscular power in an adult man to overcome them in an inspiration made to its full length equivalent to "not less than 1000 lbs." All which is correspondingly favorable to an expiration begun at the end of such an inspiration.

Even where my patients were progressing well in their efforts, and I urged them "to breathe deeper and faster," the effort for depth would

[1] See Dr. Bradford's report of my Clinical Lecture, in Philadelphia Medical Times, March 4, 1876.

[2] Cyclopædia of Anatomy and Physiology, art. Thorax, p. 1056.

[3] See his Physiology, p. 512.

always produce an interruption of rhythm of the velocity, and I then got to urging them "to breathe faster and deeper;" then "to breathe as fast" as they could; and, finally, notwithstanding I had had the importance of the *depth* of the breathing impressed on my mind, I recurred to my experiences with ether, chloroform, and other vapor, of the advantage of urging the patient to blow out, and adopted with great satisfaction here the plan of making my patients to expire as fast as they could. The fact of the very circumstances of a deep inspiration making an increase in the power to expire, became then very evident; the patient would always, after blowing as fast as he could several times, be compelled to take a deep inspiration; here we demonstrated the French "*besoin de respirer*," against which the patient would show his inability to exert any excess of counteraction, and found a satisfactory proof that no effort of the patient's will could carry the process to a point of serious suspension of respiration. These advantages of expiration over inspiration also come into play when we try to have efforts made to exaggerate either for any length of time, especially if the patient has become stupefied by such; he then requires to be constantly urged to go on with the *breathing in* as fast as possible, whereas, once started to blowing out rapidly, he will continue the blowing when he becomes stupefied, although not with the same earnestness, yet with the same rapidity. From recent conversations with Dr. Bonwill, I find that he has been led to adopt the same directions to his patients.

This altering, by an effort of the will on the part of a patient, the frequency and character of the respiration in the manner described, has an effect of course on the frequency of the heart's action, but this is, I may say, not directly related to the frequency of breathing, but corresponds rather with the exertion made by the patient. The force of the pulse does, however, here, as with ether, chloroform, etc., furnish evidence of the impression being made on the nervous system, never, however, to an extent like that of either of those agents, and to a decided extent only when the utmost impression to be made has been effected. This change is one of *diminution* in the force of the heart, and is essentially like that of a natural sleep, but is as much more marked than the latter as that by chloroform is over that by ether. It never can become excessive for the reasons we have given. Bourdon long since[1] declared that no one can by any exercise of the will over the breath destroy the heart's action. One can therefore continue, without any fear or hesitation, to urge the patient to keep up this breathing process. It generally requires from three to five minutes of constant urging to breathe in this manner, at a rate of from forty to fifty or more inspirations in a minute, to bring about the desired condition.

What will strike everybody, I am sure, as remarkable, anticipating the influence of dread, is, that females always require less time for the purpose than males, no matter what may be their age relatively. Those of middle life always require more time than either the young or aged. The very young, those under ten years, are the most difficult to manage, chiefly, I think, through their fear; more time is also, I have always noticed, required in a cold clear atmosphere, than in an opposite one. The same class of patients, as a rule, takes five minutes in a cool room of a winter's day to induce the same state of analgesia which would need only three of a close summer's day. All these facts had been care-

[1] Recherches sur la Méchanisme de la Respiration.

fully observed by myself before I had given the *rationale* of the process any serious thought or consideration.

The first effect of this breathing, appreciable to an observer, is a change in the color of the face; the lips, the cheeks, and the tint of the face, at first brightened, generally become pallid, then leaden or bluish; and with these latter changes the expression is altered and wanting in animation. If the eyes are open, they have a vacant look, and their lids begin to droop; the patient assumes the look of one much overcome by fatigue, and then of one on the verge of an ordinary sleep. Then often ensues, especially in an hysterical young girl, a state of very positive muscular rigidity; before this however ensues, the state of analgesia has been brought about, and is indicated by want of recognition of the prickings of pins or the like, for the patient will always feel the abrupt contact of obtuse bodies—even the touch of your hand—but will not now experience any pain even from a bistoury or scalpel. The state of rigidity, which I have just referred to, resembles much that of catalepsy, and what is frequently noticeable in cases where anæsthetics are being administered. The power to move is embarrassed by this; the will, however, would seem to have persisted, as the patients tell you afterwards, and may even show you during this state, by their efforts without full ability to follow your directions in moving or suffering you to move a limb. This rigidity of the muscular system is by no means of constant occurrence, is often very slight and transient, and gives way. It is clearly from the semi-asphyxiated state induced by the patient's breathing, for on ceasing to urge on him the rapid rate, he relapses into a quiet, normal rate, and so, soon allowing his blood to recover from its intensely purple hue, he comes out of this state, and gradually recovers his ability to appreciate and remember the contact of foreign bodies and to move away from them.

The subjective phenomena of this process are, as far as they go, essentially like those from nitrous oxide, ether, and even chloroform. There is, first, the swimming or confusion in the head, and the loss of perfect or acute consciousness, attended with a sensation of tingling and distension of the surface—this latter beginning at the sentient extremities, and the hands especially, and passing upwards, and closely following these sensations comes the condition of analgesia, absence of appreciation of painful impressions on those sentient nerves. It is, as I have before remarked, to be particularly noticed that there is in this state the want of appreciation of ordinarily painful impressions, and in this respect we see the resemblance most marked to nitrous oxide; it will be remembered that it was this effect of the laughing gas which attracted the attention of Wells, and he showed that the analgesia may be there most perfect, without the concurrence of the state of anæsthesia, although the distinction between the two states was not then known. These same subjective phenomena, as I have intimated, occur in the administration of ether and chloroform; but they occur then in such rapid succession, and are so soon followed by a profounder impression on the cerebral centre, that there are no recollections of them afterwards, and this was more generally the case in the early history of the use of anæsthetics than it is now, from the fact that there is not the care taken now to secure a free dilution of the vapor by atmospheric air that there was then; the anæsthetic state is now hastened on as rapidly as possible by the mode of administering the vapor, by giving it as undiluted as possible. In the administration of nitrous oxide, as originally proposed by Sir H. Davy,

the air expired into the bag at the close of each breath is charged with carbonic acid and carbonic oxide gases, and, as the nitrous oxide becomes exhausted from the bag, these same gases are breathed in and breathed over, again and again; and how much, it may be asked, does the insensibility from this administration depend upon these very gases of expired air? Who indeed, on careful consideration of the prodromes of *all* anæsthetics, will not be disposed to attribute those very prodromes to the same physiological conditions? All modern anæsthetics, so called, owe their property to their mode of impressing the nervous system, and that essentially through the respiratory organs, intoxicating the blood, and this acting secondarily through the circulating organs by which it is conveyed to the nerve centres. The prodromes in all of them are most marvellously alike in their individual features as well as in their order of succession.

Ether and chloroform are hydro-carbons, so called, and their vapors not only furnish those elements proportional to the specific gravity of said vapors, but so relatively cut off the supply of oxygen by which those same elements, hydrogen and carbon, already in, are to be elaborated from, the venous blood of the lungs. Before they even impregnate the blood of the lungs, they thus arrest or interfere with its means of purification, and this they do clearly by cutting off the oxygen of atmospheric air, for the moment you let that air in a state of purity take their place, you have that elaboration resumed, and, more than that, you have all the abnormal impressions on the brain made to disappear. Let the patient, however, breathe an impure air, one charged with so-called carbonic acid gas, for instance, and the difficulty of breathing continues, and with it all the phenomena now recognized as belonging to the asphyxiated state show themselves in regular succession, proportionate, however, as Felix Leblanc has shown, more especially to the quantity of carbonic oxide so thrown into the circulating fluid of the brain and spinal marrow from the lungs by way of the heart, thus affecting essentially the blood corpuscles and making them inert; nay more, they are then no longer, as Bernard has shown, carriers or capable of being carriers of oxygen, the great vivifying as well as purifying element of the fresh air.

In 1854, Mons. Chenot[1] gave a chemical explanation of this action of the oxide of carbon on the system. He there admits two consequences of the action of the gas; he says when it penetrates the blood by the lungs, it takes the oxygen of the blood and passes into carbonic acid gas; and secondly it produces an exaltation of temperature which results in a sort of inflammation of all the organs and tissues. But M. Chenot, as Bernard remarks, is a metallurgist, and his theory is purely a chemical one. Bernard had, prior to the date of Chenot's communication, in fact as early as 1842, observed that animals, asphyxiated by the oxide of carbon, had their *venous* blood made red like arterial blood, and Troja had observed this same thing before Bernard. The last-named authority, Claude Bernard, and we have no better on the subject, insists upon this change of color of the blood, the peculiar reddening of the blood corpuscles, and a reduction of the temperature, as pathognomonic effects of carbonic oxide, as contrasted with those of carbonic acid intoxication of the blood, where on the contrary the color of the blood is blue, and the temperature increased. The hæmoglobin which constitutes the

[1] Chenot, Note sur l'Oxide de Carbon, Comptes Rendus de l'Académie des Sciences, t. xxxviii. p. 735 et seq.

coloring substance of the blood corpuscles, plays in them a purely chemical part, and we are no longer allowed to believe that the corpuscles act mechanically, dissolving the oxygen; on the contrary, we have the best proof that this substance (hæmoglobin) forms a true combination with the oxygen. Furthermore we now know that this substance will combine likewise with other gases. The distinguished German physiological chemist, Hoppe-Seyler, has studied such crystalline combinations of it with carbonic oxide, nitrous oxide, bromide of nitrogen, the vapors of ether, chloroform, and the like, as well as those with oyxgen; and has shown that they can all displace the oxygen in its combination. When we have ether used, it has been proved to act in this way likewise, and as promptly whether the hæmoglobin has been previously in combination with oxygen, carbonic acid, or carbonic oxide gases. The combination of hæmoglobin with oxygen is less stable than that with carbonic oxide, and the latter less so than that with bromide of nitrogen.

The phenomena attending these combinations in *arterial* blood, at the expense of its oxygen, are, Bernard remarks,[1] attended by symptoms constant in their character, and the first with them all is loss of sensibility; this is so marked where carbonic oxide is the combining agent, that Gabriel Tourdes,[2] of Strasbourg, has compared its action to that of chloroform and proposed it as an anæsthetic agent. Speaking of this proposal of Mons. Tourdes, Bernard says "it is true that carbonic oxide can be an anæsthetic, but in this way carbonic acid can also be one. But the word anæsthetic, thus generalized, could be applied to a crowd of different articles." "Indeed," he adds, "hemorrhage is thus an anæsthetic, since its first effect is to destroy sensibility. But I believe," he further says, "we ought to reserve the name for substances which produce anæsthesia without causing any great danger to existence."[3] In his study of asphyxia from the fumes of charcoal, this same eminent experimentalist (Bernard), besides pointing out the phenomena characterizing the action of carbonic oxide and carbonic acid, has shown that the latter is not destruction of the blood corpuscles as the former is, that it is displaced in them by oxygen, and that the last-named gas converts the lymph or white corpuscles into red corpuscles, in the lungs, by this very action with hæmoglobin to which we have been referring. By means of the spectroscope, Bernard has been able to follow the action of carbonic oxide in all its stages, and has shown that it is only when the blood seems completely imbued by this gas that death takes place, and that, when it is gradually introduced, it produces an anæmic state by this destruction of the corpuscles being more rapid than their removal by the oxygen of the air through the lungs. Death occurs from the damage done directly to the heart. Death from carbonic acid can ensue only when the supply of oxygen has been entirely cut off.

With such facts before us, we can have no difficulty in recognizing that all agents which produce insensibility to pain, when applied through the lungs, act essentially as before declared in the same way; and though complete anæsthesia is not a state of asphyxia, we can see how the two states may occur together; how the latter, occurring in attempts to induce the former, may bring about a fatal result; and how the former, carried to an extreme point of saturation, may in itself cause death. We

[1] Cinquième Leçon.
[2] Gazette Médicale de Strasbourg, 1857.
[3] Op. cit. p. 420.

have also no difficulty in comprehending how the state of analgesia always occurs from *all* these agents *prior* to any interference by them with reflex actions.

There is therefore no impropriety in turning our attention to the mode in which rapid breathing can bring about this state of analgesia, although the process is an extreme novelty. From what I have related as to my own experiences of the facilities for inducing this state, it would seem that it is not a condition produced by increasing the essential respiratory function, viz., that of introducing oxygen into the blood and taking carbonic acid from it. The position in which I have found the patient most readily to be affected, that of reclining and resting on one side of the chest, and the best manner for him to breathe, that by exaggerating his efforts at *expiration*, are against such an idea. These rather imitate the conditions consequent on a pleurisy or pneumonia, where we have a diminution of the oxidation and of the decarbonization of the blood, and where we have, to sustain this analogy, not only a diminution in the depth of the inspirations, but an abnormal increase in their relative frequency over that of the pulse, as likewise the peculiar buccal expiration, so marked in them, and which the patient readily lapses into when we urge him on in his rapid breathing. Starting out to blow fast, each inspiration gets less and less in depth, and the patient while increasing by his will that part of the action called supracostal, and which is so characteristic of woman's breathing, involuntarily diminishes that of the lower part of the chest. The greater familiarity of females with the essentials of this mode of breathing no doubt, in part at least, will explain their greater facility in being inducted into the state of analgesia by the process.

We have also as evidence against any increase properly of the respiratory function occurring, that the blood is positively cyanosed. This is not only readily appreciated in the patient's hue of surface, but can always, according to my experience, be detected in the venous hue of the blood which may escape in operations under this state, and also in that of a new-born child when the process has been used in the birth. In all instances where I have been able to test the point, I have likewise detected a positive reduction of temperature occurring in patients whilst in this analgesic state.

Again, we have the most definite proof that increase of frequency of respiration positively diminishes the amount of carbonic acid thrown out. Thus Vierodt[1] found that, if he respired only six times in a minute, the quantity of carbonic acid was 5.5 per cent. of the whole air exhaled; with twelve respirations, it was 4.2; with twenty-four, it was 3.2; with forty-eight, it was 3.0, and with ninety-six, it was 2.6 per cent.[2] According to experiments of Letellier, quoted by Kirkes,[3] about 100 per cent. more of carbonic acid is exhaled by warm-blooded animals at 0° than at 104° Fahr., and, if we take the mean results of Crawford and Lavoisier, it may be found by an easy calculation that 20 per cent. more of oxygen is consumed at 50° than at 82° Fahr. Such facts as these tend very clearly to explain the difference I have noticed, as stated before, in *temperature* and *humidity* as favoring the induction of this state on the idea that it is rather from a *diminution* in the respiratory function, a cut-

[1] Physiologie des Athmens, p. 102.
[2] Carpenter's Physiology, p. 524.
[3] Physiology, p. 135.

ting off of the normal supply of *oxygen* and retaining the *carbonic acid* in the blood to be sent to the nerve centres.

The variations which have been noted by numerous observers in the exhalation of carbonic acid by the lungs as produced by age, sex, development of body, states of repose, sleep, and watchfulness, likewise correspond with the effects of those considerations whenever I have noted them in my cases.

Every circumstance would therefore seem to indicate that this process of inducing insensibility to pain is one essentially of diminished oxidation and decarbonization of the blood, and recognizing such a state as belonging to the initiation stage of all anæsthetics, when *insensibility to pain* is positively marked, we have no necessity for begging any special theory for this process, as in its action it readily comes under the category of such agents, and is thus not either an *absurdity* or an *impossibility* from a scientific point of view.

OBSERVATIONS ON THE TEMPERATURE OF OSTEOSARCOMA.

BY

PROF. J. A. ESTLANDER,

OF HELSINGFORS, FINLAND.

Among the many different species in the great group of diseases called tumors, the sarcoma has always attracted special attention. During the time that it was considered a form of cancer, it differed from the other forms of this disease by its appearance in younger persons, and, since it has received its present name, it has continued to make itself conspicuous by its not very rare occurrence, the rapidity of its growth, and particularly by the most awful speed with which it often destroys the organism. Its history is therefore comparatively well elaborated, but, in the systems of surgery which I have had occasion to consult, I have not found any attention paid to one of its symptoms which seems to me to be very constant, and to offer a considerable interest on account of its being intimately connected with the nature of the growth ; I refer to the high temperature which it shows when it has attained a certain size, and is rapidly increasing. The following accounts of some cases will furnish a more complete description of this symptom.

Case I.—Matilda Holm, a workingman's wife, 28 years old, had always enjoyed good health until towards the end of her last pregnancy, when she began to feel pain in her right hip when walking. Three months previous to her admission she was confined, and afterwards this pain became very severe, and, three weeks prior to her entering the hospital, she began to feel a continuous and intense aching which she sometimes felt down to the knee and foot. At the same time she also observed a swelling around the crista ilii, and was troubled with a sensation alternately of heat and cold in the body. In other respects her state of health was normal. When the pain by degrees increased more and more, she sought and obtained admittance at the Surgical Clinique on Feb. 20, 1873.

At the time of her admission, her condition was as follows : Body well built, strong, and not lean ; the internal organs normal. Around the whole crista ilii on the right side, as well towards the inside of the belly as outwardly, a large immobile tumor with lobulated surface, solid consistency, but indistinct limits. The skin over the tumor was free, movable, and not tight, but traversed by large subcutaneous veins. The tumor itself, but not the surrounding parts, very tender to the touch. Crista ilii, not only the parts covered by the tumor, but also the parts adjacent to it, easily permeable to the acupuncture needle. A thermometer placed on the tumor and covered with cotton shows 37.8° centigrade, while the same instrument on the corresponding place of the left side, and on the inner side of the thigh, shows only 36.8°, and in the axilla 37.2°, a result which reiterated measurings on the following days showed to be constant.

As there could naturally be no question of any operation, the treatment was only symptomatic. The tumor increased rapidly, and the patient's strength sank in the same proportion. On May 11, the right leg and foot began to get œdematous, and shortly afterwards also the other. May 24, a severe diar-

rhœa appeared, and the patient died on the 28th of the same month. On post-mortem examination, the sarcoma was found to extend backwards to the os sacrum, and downwards to a little below Poupart's ligament. Half of the ilium was destroyed, and the rest rarefied and friable; metastatic nodules were found in the lungs and kidneys. The microscope showed the growth to be a round-celled sarcoma.

Case II.—Erland Engström, a farmer's son, 19 years old, had two years before, noticed a small knob, hard as bone, on the right tibia somewhere about the middle. In the succeeding spring it was as large as a hen's egg, and the following Christmas as large as a man's fist, but after that time it began to grow more rapidly, and moreover began to be painful. The movements of the leg were not much impaired, for on May 11, 1875, the day he was received at the Surgical Clinique, he had walked a distance of nearly seven English miles.

On his admission, the following was his condition: The patient, otherwise in complete health, had on his right leg a tumor with lobulated surface, and a consistency at some places more soft, at others more solid. The tumor extended from within an inch below the patella, close to the ankle-joint, and occupied nearly the whole circumference of the leg, in such a way that it was most developed in front, and behind had pressed the muscles from the bone to which it was solidly attached. From above downwards it measured twenty-five centimetres, from before backwards eighteen, and the circumference was seventy-five. The skin was very highly distended, but everywhere free and movable. The mobility in the ankle-joint considerably diminished, the foot œdematous, the inguinal glands on this side slightly swollen. A thermometer placed on the tumor showed 37.2° centigrade, but on the corresponding part of the left leg, or on the thigh, only 36°. Reiterated observations on the following days gave nearly the same results.

On May 21, I amputated the thigh, and the patient left the hospital on August 16, with the swelling of the inguinal glands considerably diminished.

The tumor consisted of a compound mixture of osteoma, enchondroma, and sarcoma, with here and there some small cysts. These three tissues were generally so distributed that the first was in connection with and close to the bone, the third more on the surface, and the second, which showed an exquisite areolar structure, seemed to follow no rule, and had probably in the beginning formed the whole substance of the tumor. The muscles of the leg, in form of highly distended thick fasciæ, covered the surface of the growth.

Case III.—Johan Kujola, a farmer's son, 32 years old, had noticed about two years before that his right knee began to swell and get stiff, and, as during the last months this swelling had rapidly increased, he applied and was admitted to the Surgical Clinique on Feb. 22, 1876.

The patient's condition at that time was as follows: His constitution was rather weak, but all the internal organs were healthy. Over the right knee-joint, on the anterior and inner side, was a tumor closely connected with the bone, as large as a child's head, and with no well-defined limits. In front it had a lobulated surface, but on the inner side was covered with a lamella of bone so thin that it broke into two pieces, on the first examination. The tumor was pulsating, but the circumference of the bone, together with the tumor, could neither by manual compression nor by Esmarch's bandage be reduced more than about one centimetre. The movements of the knee, although somewhat restricted, were not accompanied with any pain, and there was no collection of fluid in the joint. The glands beneath Poupart's ligament were somewhat swollen. The thermometer on the same day showed 39° centigrade in the axilla, 38.3° on the tumor, and 37° on the corresponding part of the other thigh; and the next day, in the axilla 38°, on the tumor 38°, and on the corresponding part of the other thigh 36.5°.

As there could be no doubt of the diagnosis of strongly vascularized osteosarcoma, and as the swollen glands could easily have been extirpated, I pro-

posed amputation of the thigh, but the patient refused to submit to the operation, and left the hospital on March 3.

Case IV.—Fredrika Petterson, a sailor's widow, 60 years old, had seven or eight years before noticed, on the front side of the right leg, a small knob which was immovably fixed to the bones. As she felt no inconvenience, she paid no attention to this growth until last September, when it began to pain her. She then went to a physician, who found the tumor as large as a small apple, with a scarcely perceptible swelling of the inguinal glands. On Feb. 10, of this year, she came to the Surgical Polyclinique, and as I found the size of the tumor as last mentioned, but the glands considerably more swollen, I proposed to extirpate both. The patient did not consent, but when the tumor shortly afterwards increased with great rapidity, the skin ulcerated, and the pains became much worse, she returned and was admitted to the hospital on May 11, 1876.

On her entrance, she presented a tumor firmly attached to the front part of the right tibia, as large as a child's head; its consistence was soft, and at certain points nearly fluctuating. The skin over the base of the tumor was tightly stretched, and had on the top begun to ulcerate. Beneath Poupart's ligament there was a solid tumour as large as an egg, and, around, several lymphatic glands considerably swollen. The thermometer on the tumor constantly showed 38.1°; on the corresponding part of the other leg 37°; and in the axilla 37.1° centigrade.

As I supposed that the tibia was so much involved that an extirpation was no longer advisable, on the 15th of the same month I performed Gritti's exarticulation at the knee-joint, and, besides, extirpation of the inguinal glands; and when I left home, on August 14, the patient was ready to leave the hospital. By examination of the tumor I found it to be a spindle-celled sarcoma which had destroyed the front part of the bone and entered into the medullary canal. Between the skin and the glands there was no trace of suppuration.

Case V.—Anders Mäkiavütala, farmer, thirty-nine years old, had when a child of two years been burnt by hot water on the right side of the front part of the chest. At the age of twenty and some years, the cicatrix began to grow out in the form of a red knob, which increased so slowly that by last autumn it had only attained the size of a walnut. From that time onward it had begun to grow more rapidly, and, after the top of it had sloughed off, the patient had repeatedly had bleedings from it.

On his admission, on June 21, 1876, the state of the patient was as follows: He was strongly built and well fed. The entire right pectoral region was occupied by a cylindric tumor, nine and a half centimetres high; its sides were covered with a distended, red, and thin skin, and its top formed an ulcer thirteen centimetres in diameter. The tumor was movable and not connected with the pectoral muscle, and felt at some points very soft, at others more solid. No swollen glands were to be found either in the axilla or the supraclavicular region. A thermometer on the ulceration at the top of the tumor showed 38.8° centigrade; on the corresponding part of the left side of the chest, 38°; and in the axilla, 38.7°. After extirpation, on June 23, the tumor was found to be a round-celled sarcoma. By means of skin-grafting the wound was nearly healed when I left home on August 14.

The temperature was measured in the following manner: The quicksilver bulb of the thermometer was fastened to the skin with strips of sticking-plaster, and afterwards the instrument and the whole part of the body covered with a thick layer of cotton. This manner of measuring could not, of course, pretend to be very exact, if the question were to ascertain the absolute temperature of any part of the body, but as the sole question here is about the difference between two parts measured by the same instrument and in the same manner, the result ought to be very nearly true. The above related records show, then, that the temperature

of a rapidly-increasing sarcoma is commonly one and often one and a half degrees of Celsius's scale, higher than that of the corresponding region on the other side of the body.

The high temperature in the tumor might, perhaps, be supposed to be a symptom of inflammation in the distended skin, but Case V., where the skin to a large extent had ulcerated away and the thermometer was placed on this spot, showed that it depended on the tumor itself, and in the other cases, if there had been any inflammation in the skin, there ought to have been perceived, somewhere under it, an abscess; which, however, was not the case with any of the patients, although special attention was paid to this point.

The above-described cases show that all the tumors were rapidly increasing, and, as it may generally be considered a rule that a tumor contains in the same proportion the more arterial blood the more rapidly it grows, it might, therefore, be supposed that the high temperature in these cases depended only on their rich supply of blood, particularly since the case of pulsating tumor presented the highest temperature among them. Without wishing to deny the importance of the arterial blood in this respect, I think we can, perhaps, show the probable presence of yet another source of this temperature. When we consider that even the best thermometers always show the temperature several tenths of a degree lower than it really is, and when we add this difference to the resulting temperature found with the tumors, we get as result a sum considerably higher than the temperature of the arterial blood. This authorizes the conclusion that this surplus is produced in the tumor itself, and that thus the high temperature of a sarcoma is the effect not only of its richness in bloodvessels, but likewise of the active processes going on in the elements of its tissues. It may be that this demonstration, as supported only by a few observations, has not much more value than a common hypothesis; but at all events it explains the relation between the rapid growth of the tumor and its high temperature more completely than the presence of a great quantity of arterial blood does. A teleangiectasis contains relatively more arterial blood than any other tumor, and nevertheless it sometimes disappears by itself.

If we, following the most recent opinions, consider the fungus hæmatodes in Case III. (which was not operated upon) as a sarcoma and not as a cancer, then all the narrated cases have that feature in common that they were all sarcomatous growths, for even the tumor in Case II. contained this form of growth, although the greater part consisted of cartilaginous and osseous tissue. But naturally this is not yet a proof that only the round, or spindle, or giant-cells possess this peculiarity, for the second of the reported cases seemingly leads to hint that on the contrary even the cartilaginous tissue has the same; and it seems probable that it belongs to every tumor whose elements are capable of a rapid increase in number and development.

Of all the different parts of surgery which, during the last decade, have been the object of scientific investigations, perhaps none has been so greatly developed and changed as that which deals with tumors. For at times previous it consisted only of a confusion of special observations, but now it is a well-arranged field where every growth, no matter how varying its form may be, has its given place. But this change for the better mostly regards their patho-anatomical structure, for their clinical history is pretty nearly the same as before. I hope, therefore, that this small contribution in the latter respect will not be entirely wanting in interest.

ON STONE IN THE BLADDER IN FINLAND.

BY

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To facilitate the explanation of the peculiar conditions, with respect to stone in the bladder, which exist in my native country, Finland, I will divide all cases of this disease with respect to their etiology into two groups. The first where there is no disease in the urinary organs and the stone seems to be of constitutional origin, and the second where the calculus clearly depends on a local disorder. When cases of stone belonging to the first group frequently occur in one country, and are, on the other hand, very rare or altogether wanting in another country, then the endemic causes are clearly perceptible. But when the calculus originates in diseases of the kidneys or in the many causes which prevent the bladder from completely emptying its contents—as for instance paresis of the muscles of the bladder, or obstruction of any kind in the urethra, etc.—then the question arises, in what relation do these cases stand to the general endemic causes? Are they absolutely independent of them or not? For my part I am inclined to answer this question to a certain extent in the negative. It is true that the rich or so-called upper classes of the different countries all over the world live pretty nearly in the same manner, and are precisely by their manner of living on one side more exposed to calculus, and on the other side more independent of the endemic causes of the disorder, than the lower classes. But still, as the influence of climate, water, temperature, etc., is never completely to be avoided, the rich must, at least to a certain degree, be subject to the same epidemic laws as the other classes of the population. If I then suppose a country where no endemic causes of stone in the bladder exist, there we should, as a general rule, entirely miss all the cases of constitutional origin, and find the cases of local origin principally amongst the upper classes, and that less frequently than elsewhere. This is just the case with Finland.

Some statistical figures, collected from the Case Book of the Surgical Clinique, at the University of Finland, for the last forty-four years, will show the truth of this in respect to the first group of calculous cases. During this time of nearly half a century, only one case has presented itself, that being in a farmer of forty-one years, whom, in 1863, I operated upon for a phosphatic stone in the bladder. This isolated fact is the more remarkable, as several such operations have been performed in the Clinique, but the patients always belonged to the Russian nation, particularly from the province of Jaroslaw. As Finland, with a population, on December 31, 1875, of 1,829,919, has a surface of not less than 6783 Swedish square miles (Lapland included), and the means of communication between the different parts of the country have to within the last ten years remained undeveloped, it might be supposed that cases of this disease had occurred in any of the eight provinces into which the Grand Duchy is divided, without presenting themselves at the Surgical Clinique

of the University, although it is the only special hospital in the country. In order to gain a decided conviction that this circumstance had no determining influence, I communicated by letter with the doctors superintending the hospitals erected by the government in each of those provinces, and they each and all returned the same answer: that in the last ten or fifteen years during which they had been at the hospitals, they had never met with a case of stone in the bladder.

Cases belonging to the second group of calculus have been exceedingly rare amongst that part of the population which frequents the hospitals, and have been principally limited to stones depending on paralysis of the bladder. Not one single case of renal calculus is recorded in the Case Book. But in my private practice I have had occasion to treat a great many patients of that kind; all from the richer classes. Among cases of vesical calculus, most of them depending on hypertrophy of the prostate, about twelve may have occurred among my countrymen. I have no precise data to judge of the frequency of stone in the upper classes, but it seems to me that this number is remarkably small to correspond with a practice of seventeen years, during which time I have been the only *ex-professo* surgeon in the country.

From the facts I have just now stated, I think I am justified in concluding that stone in the bladder occurs in Finland almost exclusively among the upper classes, and originates most frequently in disorders of the urinary organs. It seems as if the causes of so pronounced an immunity for a whole country ought to be easily discovered, but such is not the case. Here just as well as anywhere, where we have to do with the etiology of diseases, the facts we observe are the result of many co-operating circumstances. As it would be out of time and place here to enter into any long discussion of the different influences which c uld possibly be considered as contributing to this immunity, I will only point out the circumstances which seem to me to merit special attention in this respect. The quality of the water is generally considered very important, and to this matter we ought to assign the first place. The land consists principally of granite mountains (ground even during the glacial period), vast sandhills, and barren heaths, which the vegetation of many thousand years has covered with a scanty crust of earth. The water of springs, rivers, and lakes, therefore, contains only a very small quantity of alkaline salts. As an instance I will quote an analysis made by Prof. J. Chydenius of the water in the Wanda River, which supplies the waterworks of Helsingfors.

100,000 grammes contained 7 grammes of solid substance, whereof—

Chloride of sodium	0.63 grammes.
Carbonate of lime	1.88 "
Carbonate of magnesia	0.81 "
Silicic acid	0.48 "
Organic humus-like substance	2.55 "

Thus the purity of the water is very remarkable, and, as water forms the principal part of the nourishment of a people, we certainly must consider this quality as the main cause of immunity. Another cause might be found in the food of the Finnish people, which generally consists of cereals, fish, potatoes, and milk (nearly always sour), and very seldom meat; in fact I do not think any other people consumes so much lactic acid and so little meat. It would not perhaps be difficult to find out at least the relation between meat as food and the uric acid which forms the greater part of the calculi of constitutional origin, but I content

myself with simply pointing out the most striking features of the Finnish people's manner of living, hoping that a comparison with that of other peoples enjoying the same immunity will, better than any hypothesis, show whether those characteristic points exercise any influence in that respect or not. There would yet remain some circumstances worthy of notice, as for instance the habit of taking once a week a bath in a room filled with hot steam—a habit which is as old as the people itself, and so common that the poorest man never neglects it—but that I think I have already too long claimed your attention for this subject.

SECTION ON DERMATOLOGY AND SYPHILOGRAPHY.

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VARIATIONS IN TYPE AND IN PREVALENCE OF DISEASES OF THE SKIN IN DIFFERENT COUNTRIES OF EQUAL CIVILIZATION.

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It has long been well known that some diseases of the skin are more or less strictly confined to certain distinct geographical or anthropological districts, and that others assume marked variations of type in different countries. The number of such affections, however, is small. The impression has been gradually taking form, of late, that similar variations in type and prevalence exist much more generally among skin diseases than has been hitherto recognized, nor is it strange that such a fact, if fact it be, should have remained so long undetermined. The slow growth of dermatology prior to the last quarter of a century; the babel-like confusion in nomenclature which still hampers its various schools and writers; and the lack of competent observers, except in a few of the principal centres of the most civilized populations, have prevented the collection and comparison of the necessary data from all parts of the world, upon which the establishment of such facts must rest. It is not my intention to consider this subject now under as broad a scope as the title of this paper would suggest, but rather to present my views and the facts which I have been able to gather relating to the differences between these affections, as they occur here at home and in the countries which afford means of comparative observation. In this narrower compass we may, perhaps, wisely essay the first step towards the complete solution of the question.

As just stated, this question of variation has only slowly forced itself upon the perception of dermatologists. Owing to the entire absence in America of any regular instruction in dermatology in our medical

schools, and of opportunity of clinical observation of skin diseases in our hospitals and dispensaries, until within the last few years, those of us here who have labored to supply such deficiencies, and have devoted ourselves specially or exclusively to the study of dermatology, got our first systematic and practical conceptions of skin diseases in the schools of Europe; conceptions no doubt often tinged over-strongly in this or that direction by the individual opinions and theories of some eminent teacher, and which we have slowly more or less modified since then by more extended and broader personal observation; still it was in foreign schools that we first familiarized ourselves with the types of cutaneous disease, and the impressions there gained we have retained as permanent standards for comparison with the results of our later and more practical experience at home. Gradually, perhaps too slowly in the case of those of us whose reverence for a master's precepts has been strongly inherent, we have become aware of differences of various kinds in some of the diseases here and the same affections as seen abroad, differences in course, in severity, in prevalence, in behavior under treatment, etc. These later impressions have been almost wholly the growth of individual observation, and with the exception of a limited mutual interchange of opinion on the subject in conversation or correspondence, and brief allusion to some of the more noticeable variations in occasional papers, the question of such differences has not been presented or discussed in a general way by any of our dermatologists. Nor is this strange: we have been more wisely spending our time, first, in creating opportunities of study, too often in the face of indifference or positive opposition, until we may justly claim to have established dermatology upon a firm basis; and, secondly, in faithful observation, rather than in the preparation of crude publications, so that we have been largely ignorant of each other's opinions upon the subject. But now that the mature results of careful and extensive home observation are, of late, more generally given to the world; now that we have in our American Archives a special channel of publication, such as only one other country can boast of; and now that original and independent works on general dermatology, of which we may justly be proud, have been written by members of this Section; now is it for the first time possible to compare the results of our enlightened and independent observers with those of our foreign brethren, and to learn how far our previous vague and individual impressions are supported by facts drawn from wide fields of observation. And yet we should not over-estimate the opportunities we even now possess for the study of this question. It is to be regretted that they exist only in a few centres of observation, separated but a short distance, and in the midst of large cities with mixed populations; nor until trained dermatologists are more widely and uniformly distributed than at present, will it be possible to form a reliable estimate of the prevalence and character of skin diseases throughout our vast and varied country, or to study in a satisfactory way the variations which these affections manifest among the many races which inhabit it.

If any one of us who has had much to do with skin diseases in both hospital and private practice in our larger cities, were now asked, what are the most marked differences between these affections here and in foreign countries, he would probably reply that we miss altogether certain diseases which are either very commonly, or occasionally, met with in some parts of Europe, while others are far less prevalent here than there. Moreover, we rarely or never see here such grave forms of some affections

as are of frequent occurrence in the old world. Again, certain diseases are more common at home than abroad, or present certain modifications in type which may be regarded as indigenous. Such, I venture to think, would be the reply of all of us, as the result of entirely independent observation; and yet if we were asked for the data upon which such conclusions were based, we should have to answer that they were largely impressions, but impressions which would prove to be in the main correct, when tested by statistics. It is to determine, therefore, how reliable are our opinions upon these points, as far at least as the test is applicable, that I have brought together as many statistical reports bearing upon the prevalence of cutaneous diseases amongst us as are available, for the purpose of comparing them with well-known foreign tables. In making up this home standard, I have preferred to use materials which are the results of the observations of those of us who have had essentially the same training in dermatology, and whose nomenclature and deductions are, therefore, interchangeable, and if faulty uniformly so. They are drawn, too, from similar sources in the large cities of Philadelphia, New York, and Boston, and are therefore homogeneous in character. The figures are taken from the published reports of free skin dispensaries prepared by Dr. Duhring, in Philadelphia, by Dr. Bulkley, in New York, by Dr. Wigglesworth, in Boston, and by the writer. They represent ten thousand consecutive cases of diseases of the skin, affording thus an opportunity of contrasting the peculiarities of these affections, if such exist, in these cities, and of forming a collection as numerous, I believe, as has been tabulated for similar purposes by any one writer hitherto in other parts of the world. I have not attempted to give the complete lists published by these gentlemen, but have put down in the table only such affections as bear possibly upon the subject before us, those of most common occurrence, and the rarer forms of greatest interest. It is to be regretted that we have not the published observations of dermatologists also from our southern and western cities, so that a more completely national representation might be offered; but until our special branch of medicine attains a more independent position in these parts of our country, we must accept this restricted report as the only one at present practically possible.

In examining this table for the purpose of ascertaining if there are any notable differences in the relative occurrence of the affections therein mentioned in the cities of Philadelphia, New York, and Boston, it must be remembered that we cannot generalize too boldly from the results of such an analysis. The element of variableness in the customs of their respective populations, in professional etiquette even, and other non-apparent circumstances, which all influence the attendance upon dispensary practice, must make no little difference in the character of the diseases treated in these cities, in addition to the more noticeable matters of race, manner of living, location, climate, etc., to which we should naturally refer such variations. On the whole, it will be observed that the affections do not vary greatly in prevalence; and some of the apparent discrepancies may be easily explained. For instance, in Philadelphia, no cases of dermatitis calorica, or dermatitis venenata, are recorded, and yet there can be no question that of the first variety, at least, cases must occur; of dermatitis venenata, Boston has more than its share, perhaps because the skin clinic of the Massachusetts General Hospital draws its patients from a wider rural area than those of her large sisters, and that her encircling flora abounds more than theirs in the poisonous

TABLE I. *Showing the comparative frequency of the more common diseases of the skin in* 10,000 *consecutive cases in American dispensary practice.*

Diseases.	New York. Dr. Bulkley. 1617 cases.	Philadelphia. Dr. Duhring. 1267 cases.	Boston. Dr. Wigglesworth. 1862 cases.	Boston. Dr. White. 5254 cases.	Total.	Percentage.
Acne	154	116	246	378	894	8.9
Alopecia	3	......	19	14	36	.36
Alopecia areata	1	2	6	17	26	.26
Chloasma	6	6	22	16	50	.5
Dermatitis calorica	9	......	5	26	40	.4
Dermatitis venenata	7	......	......	73	80	.8
Ecthyma	7	3	3	58	71	.7
Eczema	520	535	447	2372	3874	38.7
Epithelioma	......	18	17	25	60	.6
Erysipelas	32	3	5	55	95	.95
Erythemata	39	4	24	86	153	1.5
Furunculosis	36	11	23	65	135	1.35
Herpes	16	2	20	29	67	.67
Herpes zoster	22	7	6	77	112	1.12
Ichthyosis	3	12	1	5	21	.2
Impetigo	......	......	18	19	37	.37
Lupus	10	3	10	11	34	.34
Lupus erythematosus	2	8	12	6	28	.28
Pemphigus	3	1	2	15	21	.2
Phthiriasis capitis	69		32	240	341	3.4
Phthiriasis vestiment.	32	25	38	50	120	1.2
Phthiriasis pubis	1		11	7	19	.19
Prurigo						
Pruritus	43	42	36	51	172	1.7
Psoriasis	70	75	71	157	373	3.7
Purpura	9	5	5	20	39	.39
Scabies	62	9	17	145	233	2.3
Scrofuloderma	11	6	31	30	78	.78
Seborrhœa	......	54	48	56	158	1.58
Syphiloderma	137	136	100	340	713	7.1
Tinea tonsurans	47	28	86	185	346	3.46
Tinea versicolor	14	33	27	81	155	1.55
Tinea favosa	7	1	6	17	31	.31
Ulcers	25	23	33	317	398	3.98
Urticaria	59	33	37	136	265	2.65
Xeroderma	1	......	11	13	25	.25

species of rhus. Epithelioma is apparently unknown in New York as a skin disease, because the large surgical hospitals no doubt attract it wholly to themselves. So, too, the table fails, without doubt, to represent the actual or proportionate occurrence of lupus, because it is so commonly regarded as a so-called surgical disease. Again, ichthyosis is apparently much more common in Philadelphia, perhaps because Dr. Duhring may regard the cases placed apart by the other observers under the title Xeroderma, as merely a variety of the former disease. Other differences, however, cannot thus be accounted for. The erythemata, for example, must be less prevalent in Philadelphia, for some inexplicable reason, than in the northern cities, for but 4 cases out of the 153 were there observed. Some small part of this great variation may be owing to difference of opinion in classification regarding some of the varieties of the affection, but a small part only. Erysipelas, too, like these other acute hyperæmic processes, seems to be of less common occurrence in Philadelphia. It is in the parasitic affections, however, that

the greatest differences are to be observed. That scabies forms but .7 per cent. of the Philadelphia cases against nearly 4 per cent. in New York, and 2 per cent. in Boston; and phthiriasis only 2 per cent. in the first against 6 per cent. and 5 per cent. respectively in the two latter cities, can be easily explained on the supposition, which is thus demonstrated to be a fact, that the former's clinic derives its material from a different class than the immigrant populations which make up so largely the patients of the two seaboard cities; but this does not explain the remarkable predominance of one of the vegetable parasites over the others in Philadelphia. Tinea tonsurans is elsewhere in America far more common than the other vegetable parasitic affections, yet the table shows that in Philadelphia tinea versicolor is of more frequent occurrence than the others collectively. In the New York column there are 47 cases of tinea tonsurans, 14 of tinea versicolor, and 7 of tinea favosa; in Boston, 271 of tinea tonsurans, 108 of tinea versicolor, and 48 of tinea favosa; while in Philadelphia there are 28 of tinea tonsurans, 33 of tinea versicolor, and 1 of tinea favosa. We are less acquainted with the natural history of the fungus of tinea versicolor than with those of the other vegetable parasitic affections, and it may be that peculiarities of this sort in its geographical distribution may throw important light upon its nature.

Inasmuch as the statistics of dispensary and hospital practice afford but a partial representation of the prevalence of cutaneous affections in the whole community, I have prepared a table (Table II.) which presents more fairly their distribution amongst all classes. It shows the relative occurrence of 5000 consecutive cases at the out-patient department for skin diseases at the Massachusetts General Hospital in Boston, and of 2000 consecutive cases in my private practice there, and illustrates the two extreme classes of patients, those on the one hand mostly poor and of foreign derivation, with the characteristic affections of poverty and neglect of personal care; and, on the other hand, those who have the means and leisure to seek professional advice for troubles which more affect the looks than the health of the individual. These extremes largely represent, therefore, the affections of squalor and of luxury; but, even when the two classes are placed together, they fail to show fairly the prevalence of cutaneous diseases amongst all classes. Most infantile affections of the skin among the middle and upper classes, for instance, remain under the care of the family physician, and only in exceptional cases are seen by the specialist. This is true, too, with many of the trivial and self-limited diseases among them.

As might be inferred, it is mainly with regard to the affections which characterize the two extremes of the classes which make up this table, that we find the most striking differences. Those diseases which affect the personal appearance of the patient, and do not interfere with health and labor, are naturally in far greater proportion in private than in dispensary practice. Thus we see that acne forms 18 per cent. of the former, and 6.9 of the latter; the various forms of loss of hair, included under alopecia, over 10 per cent. in the former, and less than one-fourth of 1 per cent. in the latter; all the varieties of seborrhœa 10 per cent. of the private cases, and only 1 per cent. of the hospital cases; and yet there are no good reasons for believing that these affections are even as prevalent among the rich and well-cared for as among the poor and less cleanly. On the other hand, it is in the latter class that we find the animal parasitic affections positively more abundant, as likewise those which are

TABLE II. *Showing the relative occurrence of the more common skin diseases in dispensary and in private practice in Boston.*

Diseases.	5000 consecutive cases at Mass. Gen. Hosp.	Per cent.	2000 consecutive cases in private practice.	Per cent.
Acne	348	6.9	360	18.
Alopecia	13	.26	205	10.2
Alopecia areata	17	.34	27	1.3
Chloasma	16	.32	38	1.9
Dermatitis calorica	26	.52	4	.2
Dermatitis venenata	73	1.4	18	.9
Ecthyma	55	1.1	5	.25
Eczema	2242	44.8	507	25.3
Epithelioma	25	.5	5	.25
Erysipelas	52	1.		
Erythemata	81	1.6	33	1.65
Furunculosis	65	1.3	9	.45
Herpes	28	.56	3	.15
Herpes zoster	73	1.4	9	.45
Ichthyosis	5	.1	3	.15
Impetigo	19	.38	2	.1
Lupus	11	.22	6	.3
Lupus erythematosus	6	.12	2	.1
Pemphigus	15	.3	3	.15
Phthiriasis capitis	216	4.3	5	.25
Phthiriasis vestiment.	48	.96		
Phthiriasis pubis	7	.14	1	.05
Prurigo				
Pruritus	50	1.	32	1.6
Psoriasis	152	3.	78	3.9
Purpura	19	.38	4	.2
Scabies	139	2.78	29	1.4
Scrofuloderma	27	.54	3	.15
Seborrhœa	55	1.1	200	10.
Syphiloderma	327	6.5	41	2.05
Tinea tonsurans	180	3.6	113	5.6
Tinea versicolor	81	1.6	20	1.
Tinea favosa	17	.34	4	.2
Ulcers	309	6.18	15	.75
Urticaria	132	2.6	17	.85
Xeroderma	13	.26	17	.85

caused wholly or in part by the action of external irritation, and those which are more or less due to poverty of the blood and unhealthy ways of living. Thus disturbances produced by lice form 5.4 per cent. of the hospital cases, and only .3 per cent. of those in private practice; and scabies 2.8 per cent. in the former, and 1.4 in the latter. The various forms of dermatitis, due to the action of external causes mainly, were twice as common with the former as with the latter. Eczema, which is somewhat under the same exciting influences, formed 44.8 per cent. of the former, and 25.3 of the latter; but it must be remembered, as above explained, that the latter figures represent less fairly the absolute prevalence of eczema among the wealthy classes. Herpes formed 2 per cent. of the hospital cases, and only .6 per cent. of the private. The varieties of cutaneous syphilis, both acquired and congenital, were in the former 6.5 per cent., and in the latter 2.05 per cent. Ulcers 6.18 per cent. of the former, and only .75 per cent. of the latter. Urticaria 2.6 per cent.

of the former, and .85 per cent. of the latter. In other orders of affections, which are apparently little under the influences of class and customs of life, we find a striking equality in prevalence in the two series of cases. The erythemata, mostly forms of E. exudativum multiforme, were alike in both, 1.6 per cent.; ichthyosis .1 per cent. in the hospital cases, .15 per cent. in the private; lupus .34 per cent. in the former, .4 per cent. in the latter; psoriasis 3 per cent. in the former, 3.9. in the latter; and yet even with these affections we cannot accept these comparative results as representing absolutely their relative prevalence amongst all classes.

Let us now compare the combined results of our American table with those of similar statistics in other countries. The most available materials for this purpose are Anderson's 10,000 cases from the Glasgow Dispensary, Purdon's 3000 Irish cases, and 24,000 made up from the annual reports of Hebra's Vienna clinic; and the private cases published by Wilson and Anderson, with 2000 of my own; making 47,000 hospital and 13,000 private cases—in all, 60,000. Dr. Anderson's cases are especially valuable as representing the prevalence of skin diseases in Great Britain, not only because the number is the same as that in our American table, but because his nomenclature and classification are also essentially identical with those of the American reporters. The statistics of Wilson and Purdon are less valuable, because from want of such harmony it is impossible to make a reliable comparison of affections at all complete. On this account I have been obliged, in the following tables, to group several classes of disease under more general heads than in Table I.

Table III. shows the comparative prevalence of the more common affections of the skin in American and European dispensary and hospital practice; and Table IV. shows the same in special private practice here and in Great Britain, as closely as differences in nomenclature and classification will permit the formation of reliable comparisons.

As will be seen, Acne appears to be three times as common with us in dispensary practice as in Scotland, and nine times as much as in Vienna, if the tables are to be believed; or else our population is more sensitive about its "complexion" than foreign nations, even among the lower classes. The former conclusion is sustained in Table IV., which shows that acne forms but 7.5 per cent. of private cases in Glasgow, and 5 per cent. in London, against 18 per cent. in Boston. The Vienna statistics, it must be borne in mind, represent more a resident than an ambulatory class of patients. Alopecia, meaning loss of hair in general, forms .36 per cent. of the dispensary cases in America, .56 per cent. in Scotland, and .26 per cent. in Ireland, whilst it does not enter at all into the Vienna tables, because it is not a sufficiently serious affection for hospital residence; among private cases it forms 10.2 per cent. of the Boston, 1.8 per cent. of the Glasgow, and 4 per cent. of the London cases. Alopecia areata, on the contrary, seems to be of much more common occurrence in Scotland than here, for in the dispensary cases it forms .26 per cent. of the American, and 1.5 per cent. of the Scotch; and among the private cases 1.3 per cent. in Boston, 4.4 per cent. in Glasgow, and 2.7 per cent. in London. Of its prevalence in Austria we can form no estimate. Chloasma (the local distribution of melanoderma upon the face, moth or liver-spot, as popularly called) formed .5 per cent. of the dispensary, and 1.9 per cent. of the private cases here, against .03 per cent. of the former and .3 per cent. of the latter in Glasgow. Ecthyma presented no differences of impor-

TABLE III. *Showing the comparative prevalence of the more common affections of the skin in American and European dispensary and hospital practice.*

Diseases.	Amer'n. 10,000 cases. Boston, Phila., N. York.	Per cent.	Scotch. Anderson, 10,000 cases. Glasgow Hosp.	Per cent.	Irish. Purdon, 3000 cases. Belfast Hosp.	Per cent.	Austr'n. Hebra, 24,000 cases. Vienna Hosp.	Per cent.
Acne	894	8.9	325	3.25	......	......	263	1.
Alopecia	36	.36	56	.56	8	.26		
Alopecia areata	26	.26	153	1.5				
Chloasma	50	.5	3	.03				
Dermatitis	120	1.2	27	.27	......	......	244	1.
Ecthyma	71	.7	97	.97	6	.2	233	.9
Eczema	3874	38.7	2527	25.27	800	26.	2877	11.9
Epithelioma	60	.6	38	.38	14	.46	33	.12
Erysipelas	95	.95	10	.1	22	.7	89	.36
Erythemata	153	1.5	476	4.7	......	......	210	.8
Herpes	179	1.79	32	.3	20	.6	178	.7
Ichthyosis	21	.2	31	.3	1	.03	56	.2
Lupus	62	.6	198	1.98	......	.....	738	3.
Pemphigus	21	.2	13	.13	15	.5	53	.22
Phthiriasis	480	4.8	327	3.27	107	3.5	3186	13.
Prurigo	......	......	1	......	......	......	740	3.
Pruritus	172	1.7	39	.39	......	......	149	.6
Psoriasis	373	3.7	725	7.25	75	2.5	652	2.2
Purpura	39	.39	8	.08	4	.13	95	.3
Scabies	233	2.3	2527	25.27	497	16.	11,284	48.
Scrofuloderma	78	.78	27	.27				
Seborrhœa	158	1.58	13	.13	......	......	25	.1
Syphiloderma	713	7.1	493	4.9	299	9.9		
Tinea tonsurans	346	3.46	142	1.4	53	1.7	80	.33
Tinea versicolor	155	1.55	106	1.06	18	.6	50	.2
Tinea favosa	31	.3	156	1.56	15	.5	105	.45
Ulcers	398	3.98	433	4.3	......	......	190	.79
Urticaria	265	2.65	147	1.47	43	1.4	206	.8
Xeroderma	25	.25	......	......	......	......	1	

tance, although the term is too loosely used for close comparative analysis. Eczema made 38.7 per cent. of the American dispensary cases, 25.57 per cent. of the Scotch, 26 per cent. of the Irish, and 11.9 per cent. of the Austrian; and of the private cases 25.3 per cent. in Boston, 34.8 per cent. in Glasow, and 45 per cent. in London. It does not seem, therefore, that there is any marked difference in its prevalence in Great Britain and here at home, if the figures in both Tables (III. and IV.) be considered together; and the inverse ratio established by a comparison of its prevalence in the poorer and richer classes here and abroad may be explained by simple custom, as governing the relations between general and special practice in the respective countries. The low percentage in the Austrian column may be partly explained by the fact that those affected with the milder forms of the disease do not seek hospital residence for relief, and that cases of infantile eczema are diverted to the children's hospital.

With regard to the next two affections in the tables, Epithelioma and Erysipelas, trustworthy comparison is impossible on account of variation in local custom of referring such cases to the skin or surgical departments of hospitals. The various forms of Erythema would seem to be less prevalent in America than in Europe, for here it occurred in the proportion of 1.5 per cent. in dispensary, and 1.6 per cent. in private practice, to 4.7 per-

TABLE IV. *Showing the comparative prevalence of the more common skin diseases in American and European private practice.*

Diseases.	Boston. 2000 consecutive cases.	Per cent.	Glasgow. 1000 consecutive cases.	Per cent.	London. 10 000 consecutive cases.	Per cent.
Acne	360	18.	75	7.5	508	5.
Alopecia	205	10.2	18	1.8	414	4.
Alopecia areata	27	1.3	44	4.4	276	2.7
Chloasma	38	1.9	3	.3		
Dermatitis	22	1.1	......	......	15	.15
Ecthyma	5	.25	......	......	42	.4
Eczema	507	25.3	348	34.8	4500	45.
Epithelioma	5	.25	9	.9		
Erysipelas	......	......	......	......	12	.12
Erythemata	33	1.6	99	9.9	219	2.19
Herpes	12	.6	1	.1	108	1.
Ichthyosis	3	.15	7	.7	46	.46
Lupus	8	.4	25	2.5	155	1.55
Pemphigus	3	.15	3	.3	19	.19
Phthiriasis	6	.3	7	.7	23	.23
Prurigo	......	......	2	.2		
Pruritus	32	1.6	11	1.1	61	.61
Psoriasis	78	3.9	106	10.6	628	6.28
Purpura	4	.2	1	.1		
Scabies	29	1.4	44	4.4	308	3.
Scrofuloderma	3	.15	6	.6		
Seborrhœa	200	10.				
Syphiloderma	41	2.	57	5.7	350	3.5
Tinea tonsurans	113	5.6	36	3.6	252	2.5
Tinea versicolor	20	1.	15	1.5	131	1.3
Tinea favosa	4	.2	4	.4	3	.03
Ulcers	15	.75	7	.7		
Urticaria	17	.85	5	.5	88	.88
Xeroderma	17	.85				

cent. in Scotland in the former, and 9.9 per cent. in the latter, and of 2.19 per cent. of Mr. Wilson's cases. In Austria, on the other hand, it formed only .8 per cent. of the cases in the hospital. Herpes is considerably more prevalent in America than in foreign dispensary practice, forming 1.8 per cent. of the cases here to .3 per cent. of the Scotch, .6 per cent. of the Irish, and .7 per cent. of the Austrian; but in private practice, as with eczema, an inverse ratio prevails, at least in London, the percentage being in Boston .6, in Glasgow .1, and in London 1. Ichthyosis shows no remarkable variations in the tables, .7 per cent. being the highest ratio presented by any country. Lupus, on the other hand, shows a marked difference in relative occurrence, being much less common here than abroad, a fact which is strongly impressed upon an American in Europe by the chronic nature of the disease and the heroic means employed in its treatment. Of the dispensary cases it forms in America but .6 per cent., in Scotland 2 per cent., in Austria 3 per cent.; and in private practice here .4 per cent., in Scotland 2.5 per cent., and in London 1.5 per cent. The reports do not allow a proper subdivision into its two distinct forms. Pemphigus shows itself about alike in both classes of cases and in all countries. Phthiriasis is naturally everywhere almost exclusively confined to the lower classes, and marks clearly the degree of degradation in habits of cleanliness of a nation. It will be easily understood,

therefore, why Austria with her dirty Slavonic races should present so excessive a percentage of cases, 13 per cent. to 4.8 per cent. in America, 3.3 per cent. in Scotland, and 3.5 per cent. in Ireland, although the Vienna figures comprise also the excoriations produced by fleas and bugs. Scabies, too, affords of course another and much stronger illustration of the same law. Of dispensary cases it forms amongst us only 2.3 per cent., in Scotland 25 per cent., in Ireland 16 per cent., and in Austria 48 per cent.; and in private practice in Boston 1.4 per cent., in Glasgow 4.4 per cent., and in London 3 per cent. Prurigo may be set down as an affection almost wholly absent from this country, and in fact virtually confined exclusively to certain parts of Europe. Not a single instance is recorded among the dispensary cases in America, only a single one in Scotland, whilst it forms 3 per cent. of the Austrian. In my private practice I am not sure that I have seen a single case, whilst Anderson puts down two instances in his list. Mr. Wilson's statistics cannot be considered, as he confounds the disease with simple pruritus. This latter affection seems to be somewhat more prevalent here than in Europe, forming 1.7 per cent. to .4 per cent. of the Scotch, and .6 per cent. of the Austrian dispensary cases; and of the private cases 1.6 per cent. here, to 1.1 per cent. of the Scotch, and .6 per cent. of the English.

Psoriasis is much more common in Scotland than elsewhere, according to the tables, in both classes of patients; in the dispensary cases showing 7.25 per cent. to 3.7 per cent. American, 2.5 per cent. Irish, and 2.2 per cent. Austrian; and in the private cases 10.6 per cent. to 3.9 per cent. American, and 6.3 English. Purpura, if the small number of cases recorded permits comparison, seems to be rather more common here than in Great Britain, both in dispensary and private practice. Scrofuloderma is a term so indefinitely used by different writers that no legitimate conclusions can be drawn from the statistics presented. Seborrhœa, like acne, appears to be much more common amongst us than elsewhere, the term being used to include that scaly condition of the scalp so intimately connected with alopecia. Of the American dispensary cases it forms 1.6 per cent., of the Scotch .13 per cent., and of the Austrian .1 per cent.; whilst in private practice it forms 10 per cent. of all cases. With regard to the syphilodermata, it is clearly impossible to draw any inferences from the figures presented in the tables as to the comparative occurrence of this disease, because the existence of special departments and of advertising quacks draws the cases almost wholly away from skin clinics. In the Vienna column, for instance, the space is not filled, because the number of cases which find their way into the cutaneous wards is so trivial compared with the immense number in the adjoining clinic of Sigmund. In the skin department of the Massachusetts General Hospital, the cases (Table I.) are almost wholly those of the wives and children of diseased men. The Vegetable parasitic affections present some rather surprising results: Tinea tonsurans is more common here than in other countries, forming 3.46 per cent. against 1.4 per cent. of the Scotch, 1.7 per cent. of the Irish, and .33 per cent. of the Austrian dispensary cases; and in private practice 5.6 per cent. against 3.6 per cent. of Anderson's, and of 2.5 per cent. of Wilson's. Hebra's cases hardly represent the frequency of its actual occurrence, because he did not recognize, when they were published, the parasitic character of eczema marginatum, and a portion of his cases of sycosis should possibly be included in the same class. Tinea versicolor does not vary so greatly, but is less common in Ireland and Austria than elsewhere. Tinea favosa is somewhat less common here

than in Europe, and noticeably so than in Scotland. It is in Italy, however, that it especially flourishes. The ratio which these three affections bear to one another in Scotland in dispensary practice is singular by comparison with other countries, for whereas with us tinea tonsurans forms 3.46 per cent., tinea versicolor 1.55 per cent., and tinea favosa .3 per cent., and in Ireland 1.7 per cent., .6 per cent., and .5 per cent., respectively; in Scotland the percentages are 1.4, 1.06, and 1.56, or nearly equal. In Anderson's private cases, however, their relative occurrence is more like that observed elsewhere. Urticaria is somewhat in excess in America, forming 2.65 per cent. of the dispensary cases against 1.47 per cent. in Scotland, 1.4 per cent. in Ireland, and .8 per cent. in Austria; but in private practice the difference is not so marked, the percentage being .85 in Boston, .5 in Glasgow, and .88 in London.

TABLE V. *Showing the relative prevalence of certain rare forms of skin disease in both dispensary and private practice in America and Europe.*

Diseases.	Amer'n. 10,000 dispensary cases.	Per cent.	Scotch. 10,000 dispensary cases.	Per cent.	Irish. 3000 dispensary cases.	Per cent.	Austr'n. 23,944 hospital cases.	Per cent.
Cornu cutaneum . .	1	.01	2	.02	1	.03		
Elephantiasis Arab. .	5	.05	7	.07	2	.06	36	.15
Elephantiasis Græc. .	2	.02	1	.01	......	......	3	.01
Keloid	15	.15	5	.05	......	......	1	
Leucoderma . . .	1	.01	4	.04				
Lichen exudat. rub. .	......	......	......	......	......	......	7	.02
Lichen scrofulosorum .	2	.02	3	.03	......	......	67	.23
Lichen planus . .	1	.01						
Melasma	3	.03	1	.01				
Molluscum contag. . .	13	.13	6	.06	5	.15		
Molluscum fibrosum .	3	.03	1	.01	......	......	5	.02
Morphæa . . .	......	......	......	......	1	.03		
Pellagra								
Scleroderma . . .	7	.07	1	.01				
Xanthoma . . .	4	.04						

Diseases.	American 2000 private cases.	Per cent.	Scotch. 1000 private cases.	Per cent.	English. 10,000 private cases.	Per cent.
Cornu cutaneum . .						
Elephantiasis Arab. . .	2	.1	1	.1	3	.03
Elephantiasis Græc. . .	......	......	1	.1	20	.2
Keloid	2	.1				
Leucoderma . . .	1	.05	......	......	34	.34
Lichen exudat. rub. . .	......	......	1	.1		
Lichen scrofulosorum . .	......	......	2	.2		
Lichen planus . . .	2	.1				
Melasma	5	.25				
Molluscum contag. . .	2	.1	......	......	6	.06
Molluscum fibrosum . .	2	.1	......	......	5	.05
Morphæa	3	.15	......	......	11	.11
Pellagra	......	......	......	......	1	.01
Scleroderma . . .	1	.05	1	.1	2	.02
Xanthoma	4	.2	......	......	8	.08

If we now turn to Table V. we may learn the relative prevalence of some of the rarest forms of disease of the skin in both dispensary and

private practice in America and Europe. The results are interesting mainly because they show on the one hand how extremely rare are certain affections of the most marked type, and on the other hand how very infrequent are other diseases in the countries thus represented compared with their well-known prevalence elsewhere. We have here tabulated, it will be remembered, 60,000 cases of skin disease. Among them in the first category we find only 4 cases of cutaneous horns; 32 cases of molluscum contagiosum, 16 of molluscum fibrosum, 15 of morphæa, 12 of scleroderma, and 16 of xanthoma. In the second class above specified the most notable instances are those affections which assume in certain parts of the globe an endemic character; elephantiasis Arabum occurs but 56 times, 36 of which are found in the Austrian column; elephantiasis Græcorum (leprosy) 27 times, of which 2 were American, 3 Scotch, and 3 Austrian, and the remainder (Mr. Wilson's) nearly all imported cases from colonial leprous districts. Pellagra occurred but in a single instance. Two other affections, outside these two classes, should, perhaps, be referred to specially, inasmuch as they have been so sharply defined as distinct types by Professor Hebra; these are lichen exudativus ruber and lichen scrofulosorum. Of the former not an instance is recorded outside the seven Austrian, except one amongst Anderson's private cases. Lichen scrofulosorum occurs only twice in the American 12,000, and five times in the Scotch 11,000 cases; while 67 instances are recorded in the Austrian statistics.

The conclusions to be drawn from the above brief analysis of these tables, which have a direct bearing upon the question immediately before us, may, perhaps be best arranged as answers to questions, as follows:—

(1) What skin diseases of well-established character and recognized occurrence in Europe are wholly absent in the United States?

(2) What affections are noticeably less prevalent here than in Europe?

(3) What affections are more prevalent amongst us than in Europe?

(4) What modifications from the European types are observed in skin diseases in the United States?

(1) *Wholly absent.*—The most notable example in this class is Prurigo. I mean by this term of course the affection which one sees so frequently in the Vienna clinic, and which is marked by such pronounced features that its individuality is unquestionable. I do not mean that jumble of cutaneous disorders so frequently called by this name, secondary changes in the tissues of the skin as the result of simple pruritus, animal parasites, etc. One feels its absence from his practice after foreign study as a positive void long after his return, but may even come in time to doubt the truthfulness of his recollections and the very existence of the affection. Not a single case is recorded among the 10,000 American cases in Table III., and but a single one among the same number in Scotland, and I am giving here the results of the observations of those only who have had ample opportunity of studying it in Vienna. In the table of statistics of private practice, Anderson records two more cases. In my own, none are set down; yet I have seen one or two patients, whose cases in Vienna might perhaps have passed for doubtful, ill-defined examples of the disease, but none who presented all its well-characterized manifestations. Dr. Wigglesworth, of Boston, has reported a case which he thought must be prurigo. On the other hand, we find in the Vienna column of the table 740 cases, forming in fact 3 per cent. of all cutaneous diseases treated in Hebra's wards. How can we account for this striking

variation in the prevalence of so well-defined an affection; for its entire absence from this country? I can offer no explanation. There is nothing in its pathology, or known etiology, to assist us in this direction. Its prevalence is greatest in the country which, of all Europe, presents the greatest diversity in races. We can only blindly congratulate ourselves on the absence of this life-long and well-nigh incurable disease.

Among the rarer affections of the skin which are tabulated as to their comparative prevalence in Table V., there are two which find no place in the American columns, lichen exudativus ruber, and pellagra. The first named is one of the rarest diseases even in the country where its individuality was first recognized and announced, only about twenty cases in all having fallen under Prof. Hebra's observation, seven of which find place in the Austrian column. Anderson records a single instance among his private cases, making, with Hebra's, only eight among the whole 60,000. We know nothing of the causes, and but little of the nature, of an affection which proved uniformly fatal until brought under subjection by arsenic, but more extended observation may yet give it a place upon our native list.

Pellagra, granting still its specific individuality, follows such sharply defined geographical laws of distribution, that it is not surprising that but a single case is recorded in the table and that under Mr. Wilson's observation. It is improbable, with the marked contrast in respect to the abundance and variety of food which our country offers to the regions infected by this disease, that it should develop amongst us, but not impossible that emigrants from its haunts may bring it to our shores. It would scarcely establish itself here, however, as it has been slowly retreating from some of its oldest footholds in Europe during the last half century.

(2) *Less prevalent.*—Our analysis shows that the following affections have more or less claim to be considered in this class: Erythemata, lichen scrofulosorum, lupus, leprosy, phthiriasis, scabies, tinea favosa, and alopecia areata.

The various forms of erythema seem to prevail less in the United States than in Great Britain, at least. The causes of such disturbances are so various and so often of extraneous character, that it is not strange that considerable diversity in their prevalence, corresponding to variations in modes of living among different nations, should exist. Of lichen scrofulosorum we know so little that it would be useless to conjecture why the skins of strumous patients should be less liable to such disorder here and apparently elsewhere than among the nations of the Austrian empire, but such is notably the fact. Lupus, it will be seen by the tables, forms one of the most striking examples of variation in comparative occurrence upon this list. It is an affection, moreover, about which false conclusions in this respect are least liable to exist; for it is so grave in character, so enduring in course, and so imperative in its demand for heroic means of cure, that scarcely a case is liable to escape the observation of the dermatologist. Lupus is a rare disease in America, although if our figures were drawn from hospital records the statistics might fail to support this opinion, because most chronic ulcerative and destructive processes, especially those seated upon the face, are regarded by surgeons as lupus. It is not only rare but, as we shall see below, modified in type also from that so commonly observed in Europe. The opinion is held, I am aware, by some American dermatologists that lupus vulgaris is even less common than lupus erythematosus amongst us. Dr.

Duhring has found it to be so here, in Philadelphia, and Dr. Geddings, of Aiken, whose valuable studies of this affection are so well known, writes me that it is "much more common at the south than genuine lupus." Yet Tables I. and II. show that whatever be the case in the more southern parts of the United States, such is not the fact when the figures drawn from the three cities, Philadelphia, New York, and Boston, are combined, for of the seventy cases forty were lupus vulgaris and thirty lupus erythematosus.

Leprosy (elephantiasis Græcorum) according to Table V. would seem to fall almost within the class of affections which do not occur amongst us, two cases only being recorded therein, both of which, however, were observed in native Americans. Yet we know that in the neighboring province to the northeast there has long been a community of lepers, and that in our western States among the Norwegian immigrants the disease exists. In our southern States, too, I am informed that it prevails to some extent, and am indebted to my friend Dr. W. H. Geddings for the following notes concerning its occurrence in his native city, Charleston, S. C. The first case of the disease came under the observation of his brother in that city some time between 1846 and 1849, but he is unable to say whether it had occurred before that time. Sixteen well-established cases have come to his knowledge since then, five of them within the last ten years. In none of them was any hereditary taint to be traced, no progenitor having been affected as far as known, except that mother and daughter in one instance were attacked simultaneously and died about the same time. In two other instances also two members of the same family were affected. These certainly are very important data, and worthy of further careful investigation in connection with the question of infection, which can be nowhere so favorably studied as in an isolated region where hereditary taint is excluded. With regard to the race and color of the lepers, four were Jews, eight were white Christians, three were mulattos, and one a full negro. The wide-spread occurrence of the disease outside our limits to the south, in tropical America and its islands, and westward in the Pacific isles, is well known. With such foci of foreign origin all around us, and even within our borders, it is not improbable that the disease may some time become endemic in the United States.

Phthiriasis and Scabies.—In the former, an affection almost wholly confined to the lower classes, we somewhat lead Great Britain, according to the Tables, but as compared with Austria, our cases are only one to three. It is in connection with scabies, however, that the most remarkable difference is to be observed. Since the close of our late war, during which it attained a wonderful growth, it has again dwindled until it has become a somewhat rare affection, even in our seaboard cities where immigration is constantly bringing fresh supplies to sustain it. While with us, as Table III. shows, it forms only 2.3 per cent. of dispensary cases, in Scotland it constitutes 25 per cent., and in Vienna 48 per cent. of the same class, and in the Ospedale di S. Gallicano at Rome, one-half the cases under treatment in 1873 were of scabies. The disease seems to be diminishing also steadily in Austria, for, whereas in the Vienna clinic in the year 1857, that of the first report included in our statistics, there were 1695 cases, in 1873 there were only 322, a diminution due in part to the fact that the various trades in Vienna are employing each its own physician, instead of sending the infected to the general hospital. There

is no reason, however, why, with habits of cleanliness like our own, the disease should not become as rare everywhere as amongst ourselves.

Tinea favosa, as stated in the analysis, is somewhat less common here than in Europe, especially in Scotland, but it is in Italy that it finds apparently its most favorable conditions of development. In the skin hospital at Rome, above mentioned, Dr. Schilling reports 145 cases in the year 1873, 93 in 1874, and 63 in 1875, or nearly 12 per cent. of all the cases there treated. Such extraordinary preponderance implies either the existence there in proportionate abundance of the extraneous sources of contagion, or wonderful carelessness or ignorance concerning its communicability. Tinea tonsurans and tinea versicolor, on the other hand, it may be stated, seem to be almost wholly unknown there. Such local peculiarities in the prevalence of the vegetable parasites, may yet lead to important discoveries in relation to their non-parasitic phases of development.

Alopecia areata.—With no intention of entering upon the disputed question of the nature of this mysterious affection, it is to be noted that it is of less frequent occurrence here than in any of the European countries from which we have drawn our reliable data.

(3) *More prevalent.*—The affections which are shown by the tables to be in any marked degree more prevalent here than in Europe are, seborrhœa, acne, urticaria, pruritus, the peculiar forms of efflorescence due to the poisons of rhus and mosquitos, heat rashes, herpes, alopecia, and tinea tonsurans (sycosis). The affections of the sebaceous glands seem really to be more common with us in all classes than among European nations. I have no explanation of the fact to offer, but I am aware that it will be used as an argument by those who hold the opinion that acne is connected with derangements of the digestive system, inasmuch as Americans are supposed to be more "dyspeptic" than other people. However this may be, for my part I have never been able to find any fault of the internal economy which I could regard as an important primary factor in the etiology of this disease in any general way. No more can I explain the somewhat increased prevalence of seborrhœa of the scalp in private practice here, over its occurrence in the same classes of society abroad, in places where our statistics allow a direct comparison. The more frequent occurrence of alopecia, as shown in Table II., is, perhaps in proportion to, and in direct consequence of, this latter affection.

Urticaria and pruritus are both slightly more prevalent in the United States than in Europe. Both are more or less intimately dependent upon modifications of innervation, and it may be that our drier atmosphere or greater extremes of temperature may account for such excess; certainly the latter affection in some of its forms is closely connected with the winter season. Summer, too, with our intenser heat, it may here be mentioned, produces far more frequently direct disturbances of the general integument than in more temperate Europe. Prickly heat, heat rash, or folliculitis dependent upon overwork of the sweat glands, is universally prevalent, leading up to acute eczematous and furuncular inflammations of the cutaneous tissues, which form no small share of our practice during such a season as the summer just passed. Herpes zoster, too, appears to be more common among dispensary patients here than abroad, possibly through climatic influences, as there are good reasons for believing that in some cases it is directly caused by impressions received through the skin itself. Other forms of dermatitis occur amongst us, which as the result of purely *local* excitants, in both senses

of the word, are not met with in Europe, but which on this very account need only have mention here. They are the peculiar eczematous inflammation produced by the poisonous species of rhus, so widely diffused throughout the United States, and the multiform efflorescences provoked by the stings of mosquitos in persons not previously inoculated with their virus.

The vegetable parasitic affections present, as we have seen, very unexpected variations in comparative prevalence in the countries included in our tables. It is tinea tonsurans which seems to find its most favorable conditions of growth amongst us; and in its various manifestations of ringworm of the scalp, beard, and general surface, of parasitic sycosis, and so-called eczema marginatum, is quite a common affection. Among domestic animals, too, it is not unfrequently observed. Were the facts of such unequal distribution of cutaneous affections more fully recognized, we should not witness controversies between dermatologists of different countries concerning the prevalence or even the very existence of certain diseases, nor have the authority of so eminent a master as Hebra categorically expressed in denial of the occurrence of affections which are almost daily met with here, because they are rarely met with in Vienna. Of such character are his assertions in relation to tinea sycosis, which may well be extremely rare in Vienna, and yet from different habits of barbers in shaving, perhaps, or from other causes, may occur very frequently elsewhere, as it certainly does at least in Boston. It is especially in connection with the vegetable parasitic affections that one's point of view should be very broad before venturing upon such statements.

(4) *Modifications.*—Before discussing this division of our subject, it should be distinctly stated that we have now left our tables, imperfect as they are, behind us, and that our opinions in connection with it rest almost wholly and necessarily upon impressions alone. As individual observers we may naturally differ widely from one another, therefore, upon such points. My own opinions will be stated, briefly and it may be uncertainly, although I doubt not that others will have much more decided and definite views upon the subject to express. I am aware, for example, that some of our best observers believe that many affections of the skin assume a more acute or inflammatory type amongst us than elsewhere, and that others are of the opinion that cutaneous diseases are less tolerant of exciting and severe local medication than in Europe, and that therefore what may be the best methods of treatment there may be injurious even, if strictly followed here. I am not prepared to concede these propositions; at least I do not think that I have sufficient evidence to warrant the statement of any such convictions in this paper, even if they were doubtingly entertained by me. That my judgment with regard to the choice of remedies in skin diseases may differ somewhat from that of some foreign writers and teachers, is indeed true, but I do not on this account feel authorized to infer that the therapeutical or physiological reaction of the American skin differs materially from that of the European, for I know that I differ quite as widely from some of my colleagues here at home upon important matters of treatment, and I must confess too that, now that I come to record formally my opinions regarding this part of our subject, I am not prepared to state my belief in the frequency or degree of such changes of type in these affections as strongly or generally as I thought I might do before considering the question so immediately.

The number of diseases that I can specify as exhibiting such modifications to any noticeable extent is therefore extremely limited. They are, moreover, diseases characterized by varied and extreme structural changes of the cutaneous tissues, so that a comparison of the appearances presented as they occur here and elsewhere, is easy. The most conspicuous example of such is undoubtedly lupus vulgaris. We have already seen that it occurs far less commonly here than in Europe, and it may be that the same law governs its course as well as prevalence. At all events, I think there can be no question that we do not see cases here which approach those so frequently met with in foreign clinics in severity of the destructive process, or in extent of the surface affected. Such at least is my own experience, and I have no reason to offer in explanation of the fact. It might seem that it required several generations of accumulated malnutrition or more positive constitutional evils to effect such eventual disorders of cutaneous structure, and that on this account lupus prevailed more abundantly and in its worst forms where poverty and bodily misery were more common than with us; but I do not think that the history of individual cases amongst us warrants any such conclusion. I wish I could add that I have found a corresponding benignity of type in its disposition to yield more readily to treatment, but corresponding classes of cases have in my experience exhibited the same obduracy to remedies here as in Europe, and perhaps the same tendency to relapse.

The same statements may be made with truth, too, in my opinion, of the cutaneous syphilides, especially their later manifestations. We rarely, if ever, see here cases of so grave a type as in European hospitals. There seems to be a resistance to the destructive force of the disease, on the part of the skin, which tends to prevent the formation of the worst lesions within its tissues.

Leprosy, too, seems to feel the mitigating effect of some local influence within our borders. In Trecadie it has kept up a lingering existence for many years. With regard to its character among our Norwegian immigrants at the West, at present, I regret that I have no definite information to impart; Prof. Boeck expressed the opinion, after his visit to them a few years ago, that the disease seemed to be partially controlled; its course retarded to some extent by their change of residence. Whether the interesting observations made by him while amongst them are best interpreted under the theory of contagion or hereditability, is not within the province of this paper to consider. As to the character of the disease, as it occurs in our southern States, I am not informed.

Indeed, the best use that I can make of my conspicuous want of knowledge upon this and other points which should be considered under this division of our subject, is to exhibit it frankly as an illustration of what, I fear, may be a too common state of ignorance on our part, and to indicate how much definite information is yet to be acquired before we can intelligently discuss it. Finally, I may add that no one can be so fully aware as myself of the incompleteness of this paper in all respects, and to how unsatisfactory an extent it fails to answer the questions it proposes. I offer it merely as an introductory sketch, or skeleton chart, to show in what directions we may best carry out the investigations which may eventually lead to the full solution of the subject.

The following conclusions seem to be warranted by the data above presented:—

I. Certain obscure affections, the etiology of which is little, if at all, understood, even in those parts of Europe to which they are mostly con-

fined, may be regarded as practically non-existent amongst us. Such are prurigo, pellagra, and lichen exudativus ruber.

II. Certain diseases, directly connected with and dependent upon poverty and habits of personal uncleanliness, are less prevalent in the United States than in those parts of Europe of which we have sufficient statistical information for comparison. Examples of this class are the animal parasitic affections especially.

III. Some cutaneous affections of grave character, which are dependent upon or a part of serious constitutional disorders, are of less frequent occurrence and of milder type amongst us than in Europe in general, or those parts of it where they are endemic. Lupus, the syphilodermata (?), and leprosy are the most marked instances of this class.

IV. Certain disorders of the skin, especially those of its glandular systems, and those connected more immediately with its nervous system, are apparently more prevalent with us than in Europe. The most notable examples of the former are seborrhœa, acne, and possibly the heat rashes; of the latter, herpes, urticaria, and pruritus.

Discussion on Dr. James C. White's Paper.

After the reading of the preceding paper, Dr. Louis A. Duhring, of Philadelphia, said:—The subject treated of in the paper just read is one of great interest, and the statistics contained in it show that there is a marked discrepancy in the prevalence of skin diseases in the three great eastern cities of this country, Philadelphia, New York, and Boston. How to account for these differences is a difficult problem, and, on account of these differences, Dr. White's conclusions should, it seems to me, be taken with reserve. The precise meaning of some of the terms employed should also be given with more particularity; for instance, morphæa is a name susceptible of several meanings.

Dr. L. Duncan Bulkley, of New York, said:—American statistics are not as complete as could be desired. Morphæa is undoubtedly more frequent than it is represented to be in the tables of Dr. White's paper. I have myself seen it in a number of cases. I observe that no cases of prurigo are mentioned in the paper. Heretofore, I have only reported cases observed at hospitals and dispensaries, and have not reported those of my private practice. I have had two cases of prurigo under my own observation in this country. I also believe that, in the types of diseases of a congestive character, there is a marked difference between this country and Europe. I have in New York repeatedly tried Hebra's treatment for this class of cases, without success. I am of opinion that in these diseases, as met with in the United States, there is the general characteristic of nervousness, so peculiar to the American people, which requires a treatment very different from that taught in the schools of Europe. American physicians are discovering this every day; they are becoming more familiar with American characteristics as they grow older, and are finding it profitable to forget much that they learned in the old world.

Dr. Duhring said:—I have brought up the subject of morphæa in order to ascertain the views of Dr. White on that disease. Dr. White and Dr. Bulkley evidently understand it as Mr. Wilson has described it; whereas I think that Mr. Wilson has so confounded it that no one could recognize it from his definition as a distinctive disease. Mr. Wilson confounds the old morphæa of leprosy with the modern morphæa. I have discarded the definition of Wilson, and also that of Kaposi, and have from experience adopted the view to which Dr. Fox has inclined. Morphæa should be entirely disconnected from leprosy. I have seen three cases of it, at a clinic in London, and five in this country. As I understand the disease, its manifestations are of two kinds; the first being the large or small yellowish circumscribed patch upon the skin, like

a piece of fat or lard, in appearance resembling the well-known macular form of leprosy, with which, however, it has no connection. Another form entitled to the name of morphæa is the keloid so accurately described by Dr. Addison. I have seen four cases of this disease in this country. It is an extremely complex affection, requiring great detail and particularity in its description. I have seen all the symptoms encountered in both forms of the disease in one patient; I have seen the macular patches from the size of a pin's head to that of a hand with those numerous symptoms of the so-called keloid of Addison. I have also seen the macular manifestations, accompanied by reddish or purplish streaks and striæ atrophicæ, and, moreover, band-like cicatriform lesions. In a paper recently prepared by me, I have tried to give an accurate description of the disease, but to attempt it here would occupy too much time.

Dr. BULKLEY said:—I would designate as morphæa just what Dr. Duhring has described, and, with due deference to Dr. Duhring, would say that Wilson gives a similar description.

Dr. DUHRING said:—Partial atrophy of the face should not be confounded with morphæa, though it closely resembles the cicatriform form of the disease. The subject is a very complicated one, and the clearest distinctions should be made between the old morphæa of leprosy, the macular and cicatriform varieties of the disease now designated morphæa, and unilateral atrophy of the face.

Dr. WHITE said:—I had a case which began with the round form of morphæa. It was sent to England where Mr. Wilson saw it and called it morphæa; then to Vienna where Hebra called it Addison's keloid; and, finally, returned to this country and resulted in time in atrophy of the skin. In the paper before the Section the word has been used in all the looseness of meaning requisite to cover the variety of forms assumed by the disease.

Dr. DUHRING said:—I have seen the case referred to by Dr. White, and I would call it unilateral atrophy of the face. The lesions are not confined to the skin, but extend deeper and affect even the muscular tissues and bone substance. In one case of unilateral atrophy of the face which I have observed for eight years, the tongue and roof of the mouth have dwindled upon one side to only half the normal size.

Dr. BULKLEY said:—I have seen two such cases, in which the patches, or arrests of development, resembled very much morphæa. I have also within a few days seen a patient who has been put under galvanic treatment and is two-thirds or three-fourths cured.

Dr. DUHRING said:—In the reported cases of unilateral atrophy of the face, the disease has tended to increase from year to year, while in those cases of morphæa which have come under my observation, the disease has tended to spontaneous recovery; one young lady who has been the subject of a most extensive morphæa, is now nearly cured, the patches upon her body having undergone spontaneous involution; this never occurs in unilateral atrophy of the face.

Dr. WHITE said:—With regard to prurigo, I am well aware that many English writers do not concede that there is such a disease, maintaining that the cases seen in Vienna are but poorly developed diseases of a secondary character, in which the cutaneous lesions follow, and do not antedate, the subjective features.

Dr. BULKLEY said:—I have seen two well-defined cases of prurigo in native Americans. One occurred in a man who, after terrible suffering, died of the disease. The other was the case of a child.

Dr. S. ENGELSTED, of Copenhagen, said:—Prurigo is very rare in Copenhagen, but is sometimes met with. It sometimes commences in childhood, and is then incurable. It occurs only in the lowest grades of the population.

Dr. DUHRING said:—I agree with Dr. White, that this is a very rare disease. I have seen but one case in this country (at Boston), that I thought was prurigo; I could not positively say that it was so, for I saw the case only once, but if asked to name it I could call it nothing else. I was forcibly struck with its resemblance to the Austrian prurigo. The whole surface was involved.

While attending the clinic at the Hôpital St. Louis, at Paris, I one day saw a boy who had prurigo. Hardy said that he called the disease "strophulus prurigineuse," but it was undoubtedly the prurigo of Hebra. Hardy said it was obstinate but curable. Evidently it did not, in Paris, take on that aggravated form which is commonly seen in Austria. There is at present, in the Philadelphia Hospital, a case very much resembling prurigo, but I would not so call it. I doubt whether lichen ruber is more common abroad than here. Lichen planus and lichen ruber I regard as one and the same disease, the latter being the more advanced stage of the process; I would ask Dr. White whether, in his experience, he has ever known lichen planus to run into lichen ruber?

Dr. WHITE said:—I have not.

Dr. DUHRING said:—I would ask Dr. Engelsted whether he has ever seen in Copenhagen Hebra's lichen ruber?

Dr. ENGELSTED said:—I have not, but I have seen there lupus vulgaris.

Dr. DUHRING said:—Lichen ruber is as rare in Europe as here. I have seen in this city a case of lichen planus which passed into what might be called lichen ruber; it was not so severe as the cases I have seen abroad. Prurigo is a common disease in Austria, but lichen ruber is rare. I must also differ from Dr. White as to the severity of the syphilodermata, which I consider to be as severe in type in this country as in Europe.

Dr. WHITE said:—Upon this point I have written only from my own observations in Boston of the severity of the disease, and have made no comparisons with other cities.

Dr. ARTHUR VAN HARLINGEN, of Philadelphia, said:—I agree with Dr. Duhring that Philadelphia has a pretty fair share of these cases, and that many of them are of a severe type. There are no syphilitic clinics in this city; but, judging from the tables of skin diseases, the proportion of the syphilodermata is large.

Dr. BULKLEY said:—I coincide with Dr. Duhring's opinion that the disease is not less severe here than in Europe.

Dr. DUHRING said:—Many cases have come to my notice undiagnosed and consequently untreated. I can recall half a dozen frightful cases which have come from the country districts untreated. In Vienna the disease is, as a rule, recognized at once and treated, and hence it is checked before it reaches its worst stage. One case which I can recall had been diagnosed as cancer, and so treated for fifteen years.

Dr. BULKLEY said:—I have known five cases of elephantiasis Græcorum; two died within four or five years; another has been progressing eight or nine years, and another four or five years. Two of these cases came from the southern part of this continent. Those which occurred in native Americans were very severe.

Dr. DUHRING said:—I was not aware that leprosy occurred in this country at all unless it was imported. Before being convinced that it exists endemically, I should like to see a case thoroughly studied by a competent observer and have it carefully reported. The disease occurs in Central America and Mexico, in the Sandwich Islands, and also in California; but not, I think, endemically. I have seen but one case in this country, and that was a Cuban, who acquired it probably in Spain.

Dr. WHITE said:—Prof. Boeck thought that the disease, as found in the Norwegian settlement in this country, had been checked by the change of residence. I have known of one other case, one of the tubercular form of the disease, which resulted fatally in Boston.

Dr. VAN HARLINGEN said:—As to lupus, I think that statistics here show a larger proportion of cases of lupus erythematosus than of lupus vulgaris; in fact, the former may be considered a common disease, whereas the latter is not so frequent.

Dr. DUHRING said:—The American type of lupus vulgaris is much milder than the European.

VERRUGAS, A DISEASE PECULIAR TO PERU.

BY

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[COMMUNICATED BY THE SECRETARY.]

IT is, I believe, pretty generally know throughout the world that Peru is engaged in the gigantic enterprise of building a series of railways across the snowy peaks of the Andes, in order to connect the Pacific ports with the head waters of the Amazon; and by this means to establish a shorter communication with Europe, thus avoiding the inconveniences which now result from a long and tiresome sea-voyage, as well as throwing open to the world the untold wealth which exists on the Amazon and its tributaries. This wonderful undertaking, conceded to be the greatest engineering triumph of the century, is due to the energy, skill, and perseverance of Don Enrique Meiggs.

On one of these routes, starting from the Pacific Ocean at the port of Callao; passing through Lima, the capital of Peru; thence up the valley of the river Rimac; and finally crossing the summit of the Andes by means of a tunnel, at an altitude of 15,645 feet above the sea-level, vast obstacles have been encountered, not only in the way of the many natural difficulties to be overcome, but also from many and strange diseases, one of which it is the purpose of this paper to describe; but before doing so, a brief geographical description of the district in which this disease prevails will be proper.

This district lies between altitudes of 3000 and 6500 feet, cases of the disease occasionally originating a few hundred feet higher, though rarely. In and about San Pedro Mamma, 3000 feet above the sea-level, we find arid dry rocks; but on the river banks, wherever water has access, fine pasturage for animals. The banana here grows luxuriantly wherever cultivated; as we gain altitude, we encounter fine groves of oranges, figs, cherimoyer, peaches, guavas, and in fact every variety of tropical fruit except the cocoanut; as well as vegetables familiar to our northern eyes, such as the onion, lettuce, cabbage, etc. At an elevation of 5280 feet, and 46 miles from the sea, the character of the soil changes, as the valley becomes more narrow, the soil sandy and intermixed with granite, the sides of the mountains steep and producing abundantly a variety of cacti, but still susceptible to a high degree of cultivation wherever water can be carried. At an elevation of about 6000 feet, we find the granite in a friable, decomposing condition.

Running through this valley is a rapid mountain stream varying in depth from two to six feet, and fed by springs and the melting snows of the summit. The temperature at the altitude of 3000 feet is mild and equable; as we gain 4000 feet the temperature is increased, and at an altitude of 5280 feet we frequently encounter a temperature of 110° Fahr. in the shade, in the middle of the day, and at night find it sufficiently cool to require a couple of blankets on our beds, the range of

temperature frequently being as great as forty degrees Fahr. between 12 M. and 12 P.M. Approaching an altitude of 6000 feet, the temperature again becomes more equable, and at an altitude of 8000 feet it rarely varies ten degrees the year round.

The name popularly applied to the disease of which I am about to speak is VERRUGAS. It is a disease usually preceded by an initial fever of varying intensity, lasting from ten to thirty days, and accompanied by excruciating pains of a rheumatic character, finally culminating in an eruption of warts over the body; these occasionally proceed to suppuration and ulceration, accompanied at times by profuse hemorrhage.

It is a disease confined to certain valleys in Peru; it has well-defined limits, and is not found outside of those limits. Its range of altitude is between 3000 and 6500 feet, and it is found in the valleys of the Rimac, Pacasmayo, Chimbote, and some of the valleys on the eastern slope of the Andes, and was probably the disease which struck terror into the adventurous hearts of Pizarro's troops on their march into the interior of Peru through the valley of Tumbez. I am informed that this disease prevails on the eastern slope of the Andes, but always under the same conditions of altitude, soil, climate, and natural productions.

It is a disease which affects animals as well as men; it is a disease which recurs, one attack not exempting from a second, a third, or even a fourth, if the person is exposed to the same influences. The eruption, however, in the subsequent attacks, is not as profuse, nor the warts as large as in the first, although no general rule is observed in this respect. The disease obeys none of the known laws of the exanthemata, although it is in many respects closely allied to them. Retrocession of the eruption always produces very grave, and often fatal, symptoms, and usually takes the form of congestion of the liver or lungs. In like manner, should the eruption not make its appearance on the skin, or mucous membrane of the mouth or nose, a fatal termination of the case may be expected. Many have supposed the disease to be allied to syphilis, but I do not so regard it. The disease cannot be communicated by inoculation, for I have repeatedly tried on my own person, as well as on others, the insertion of pus from an ulcerating verruga, but without avail. The only analogy with syphilis is in the nocturnal pains and the occasional persistence of a copper-colored spot on the skin after a large verruga has proceeded to ulceration and cicatrization. The ulcers lack, however, entirely the geometrical forms so characteristic of syphilitic ulcerations, and these copper-colored spots are only left on the parts of the body which are covered by the clothing; no matter how large a crop of verrugas one may have had on the face, scars or discolorations rarely remain.

In those cases in which the disease is heralded by an initial fever, two types are presented; the acute rheumatic, and the low typhoid, often combined with an intermittent element which prevails extensively in the verruga district. Patients thus affected require careful watching from their extreme liability to succumb to the violence of the fever, before the eruption makes its appearance.

In the excruciating pains, particularly affecting the joints, this affection is closely allied to dengue, a disease which occasionally occurs in the Southern States of North America. In contradistinction to dengue, however, one attack does not prevent another; the fever has a longer duration; the eruption is of a different character; hemorrhage only

occurs from the verruga itself; the duration of the disease is much longer; it is not epidemic; and it is an extremely distressing and often fatal disease. It occurs as well among the strong and healthy as among those who are weakened by previous disease, or who are of feeble habit of body.

In regard to the cause, nothing is known. Many have supposed the disease to be caused by the water of the valley; this cannot be the case, as many people have simply passed through the infected district on horseback, or in the train, without drinking one drop of water, and have been as severely affected as those who have passed weeks and months in the valley. Others, in their fear of having the disease, have drunk nothing but wine or brandy during their entire period of residence, even refusing coffee and tea; and have suffered just as severely as others who have followed their ordinary mode of life. Some suppose that it may arise from the turning up the ground along the line of railway, thus bringing certain telluric influences to bear; this, however, cannot be the entire cause, as the disease existed before the ground was ever disturbed. I am inclined to the opinion that the friable, decomposed, and decomposing granite, has an influence in producing this disease as well as the intermittents which here prevail to so great an extent. I have known a nursing infant pass one night in the affected district, and in three weeks break out over the entire body with verrugas. The disease may not show itself for weeks or months after exposure, though a change of climate, and particularly a sea-voyage, is very liable to bring out the eruption.

I remember particularly the case of an Italian, who had made some money on a small contract in the infected district, and, returning to his home in Italy, on the voyage, broke out with verrugas; on arriving at his home he was immediately ordered into quarantine as a leper, and was not allowed to enter the city until he had been pronounced cured by the proper authorities. Many of the gentlemen connected with the railroad, when home on visits to New York, California, or other parts of the world, have been looked upon with very grave suspicion, not only by their friends, but by physicians and surgeons whose word is law in regard to medical matters.

The treatment of the disease is entirely palliative, and will be more particularly illustrated in the typical cases presented. The bowels should be kept open by mild laxatives, of which none are better than the tamarinds taken freely in the form of a drink; the infusion has a grateful, subacid flavor, particularly agreeable in the semi-febrile condition of the patients, and the addition of one of the alcoholic stimulants will usually be advisable. Should there be indications of liver trouble, nothing is better than blue mass or podophyllin. Hot vapor baths, or a resort to any of the various hot springs, will have the effect of accelerating the appearance of the eruption. I have sometimes found good effects from the iodide of potassium, in ten grain doses three times daily, taken in an infusion of sarsaparilla, or, more agreeably still, in the compound syrup of sarsaparilla of the U. S. Pharmacopœia. Should the pain be so great as to prevent sleep, morphia or chloral must be resorted to. The blood is thinned, and deficient in hæmatin; hence the necessity for the use of iron, and, of all the preparations of this metal, I have found none to equal the muriated tincture, given in ten drop doses, three times daily, in a glass of port wine. This course of treatment, with a full generous diet, and a proper allowance of alcoholic stimulants, will usually

in time bring out the much desired crop of verrugas. Exercise in the open air, although exceedingly painful and at times impossible, is of very great importance.

A great variety of native remedies has been used in this disease. I have tried them all and found them all wanting. Of these remedies, the best is an infusion of the Indian corn, taken in large quantity with the native sweet wine of the country. This "agua mote," as it is called, has a diaphoretic and diuretic action, as have in a greater or less degree the other native remedies, "uña de gato," "maguey," "norvillo," etc. All of these remedies are taken in infusion, and with an equal quantity of sweet wine.

Should none of these measures avail to bring out the eruption, a change of climate or a sea-voyage, or a resort to some of the famous warm baths of Chili, will probably have the desired effect.

The literature of this disease is very scanty. Prescott, in his Conquest of Peru, mentions it as one of the diseases encountered by the Spaniards. Von Tschudi, in his work published in 1749, mentions it, and the same author is said to have written an article on the subject in some medical journal of Germany; these, with an article in a Lima newspaper, and an article by Dr. J. M. Browne, U. S. N., in the Contributions to Medical Science, U. S. Navy Department, vol. i., 1872, constitute, I believe, the entire literature of the disease. An idea of verrugas can best be formed by the study of illustrative cases, of which I cite a number, and, as will be observed, the symptoms in the various cases are diametrically opposed to each other, scarcely any two cases presenting the same features.

CASE I.—The first case of verrugas which I ever saw was in the northern part of Peru, on a sugar hacienda. The patient presented himself to me with what I supposed to be a node, just on the point of ulceration. I immediately placed the man upon a course of iodide of potassium and iron, advising rest, and poultices to the tumour which was situated upon the edge of the tibia. This, however, did no good, and the tumour, after ten days' treatment, showed no signs of ulceration or pus. One day the superintendent happened to see it, and without ceremony plunged in a lancet, making a gash about an inch long, and then sent immediately for me to control the hemorrhage which his unlucky thrust had caused. I succeeded in this by using pressure and the perchloride of iron; rapid ulceration then took place, and the man left the hacienda and went to a hospital in Lima. About a year afterwards he came under my observation in one of the hospitals of the line, suffering from caries of the tibia and continued fever, of which he died. Upon questioning him, I learned that before going to the hacienda he had been at work on the Oroya Road, and that he had thus contracted the disease there.

CASE II.—A Chinese had received an injury of the spine, causing paralysis of the lower extremities, and had been in the hospital for a period of about five months, slowly but steadily gaining the use of his legs. One morning he called my attention to some lumps on his legs, about as large as peas. These lumps gradually increased in size and number, without pain, heat, or redness, until many of them reached the size of hens' eggs, and till they were found not only on the legs but on the arms and chest. The patient in the mean time did not suffer the slightest inconvenience whatever, not one of the verrugas proceeding to ulceration, but gradually subsiding as they appeared. His appetite remained good, his urine was free and of natural color, and his bowels acted daily and naturally. The only remedies given were the tinctura ferri chloridi, ten drops three times daily, and a daily subcutaneous injection of the fiftieth part of a grain of strychnia, which remedies were given for the

paralysis. I saw the patient frequently for two years afterwards; he was in good health and condition, and with only a slight degree of paralysis remaining.

CASE III.—This was the case of a Chinese who entered the hospital with all the symptoms of rheumatic fever, complaining of pain in the joints, which were swollen and tender, his tongue being covered with a dirty-white fur, his bowels constipated, and his urine scanty and high colored, at times not more than half an ounce being secreted in the twenty-four hours, and that soon becoming nearly solid from the contained urates. The fever was constant, the pulse ranging from 110 to 120 in the minute, and the temperature from 102° to 104° Fahr. The patient was placed upon the alkaline course of treatment for rheumatic fever, with some slight improvement to the symptoms. Diuretics, however, produced but little effect upon the urine, and after having exhausted all the means at my command, and, at the end of two weeks, the patient's condition continuing much the same, I resorted to a native remedy, which consisted of an infusion of the legs of the common cricket. Four of the legs of the cricket were bruised and infused in half a pint of hot water, and when cold half an ounce of the infusion was given every four hours. This produced immediate relief to the symptoms; the urine began to flow freely, and there was an immediate abatement of the febrile movement, followed by a profuse eruption of verrugas which proceeded rapidly to ulceration accompanied by free hemorrhage. The ulcers finally healed kindly under the use of the carbolated cerate and powdered matico. Iron and alcoholic stimulants were freely used to recuperate the patient's strength, which was much exhausted by his long and painful illness which had extended over a period of three months.

I was led to the use of the infusion of cricket legs by the recommendation of Dr. H. Kinney, former Medical Director of the line, who highly extolled it as a diuretic. I have used the remedy a number of times since, and with uniformly good results. I do not pretend to explain its modus operandi, but presume it to be analogous to that of cantharides, as in one case I observed some tendency to strangury.

CASE IV.—My own case. I was attacked with wandering pains about the body, just one month after taking up my residence in the infected district; these pains particularly affected the joints, sometimes one and sometimes others, the pains simulating those of muscular and articular rheumatism, but without the inflammatory symptoms. These pains were of an intermittent, shifting character; for instance, they would affect one joint, say of the little finger, in the morning, and by noon would be located in the opposite hand, knee, leg, or arm, and as rapidly change to other parts of the body, affecting, however, particularly the legs below the knees. Suddenly all the pain left the extremities and located itself in the right testicle, which became enlarged and exceedingly painful, though without any inflammatory symptoms being developed. All the ordinary remedies useful in swelled testicle, excepting leeches, were used to mitigate the pain and reduce the swelling, but without avail; but the swelling, which had come on rapidly, after continuing for nearly two weeks, subsided within twenty-four hours, leaving no ill effects behind. Immediately, however, the right arm began to swell, and continued to do so until I thought the skin would burst, so great was the tension; the tumefaction extending from the fingers to the shoulder, and lasting for about ten days, then subsiding as rapidly as it had come on.

During this time I was in perfect health, being able to ride about on horseback, eat well, and drink well, wandering pains, however, being felt throughout the body, as well as the steady pain in the arm. After the subsidence of the swelling in the arm, there was a let-up to all the symptoms, but this was only the calm before the storm. Suddenly I was prostrated by congestion of the liver, followed by jaundice and the discharge of a large gall-stone, which brought me so near death's door that my coffin was actually prepared. Fol-

lowing this, appeared the eruption of verrugas over the entire body, and a sea-voyage restored me to health. The first pains were felt on the first of March, and the first verrugas showed themselves about the first of August, when they continued coming and going until December, many of them ulcerating and being the source of free hemorrhage.

The course of treatment which I adopted was simply palliative, treating each symptom as it arose. My diet was of the most generous description, accompanied by the free use of brandy and wine, and I continued to go about and attend to my professional duties, though at times the effort was most agonizing from the intense pain. I gave a thorough trial to the corn-water with sweet wine, but did not perceive that it had much more effect than would naturally be induced by the ingestion of a large amount of fluid.

I have since that time had three other attacks of the disease. In the second attack I suffered no pain; two or three verrugas made their appearance on the skin, and a severe wetting caused their retrocession, producing extreme irritation of the stomach, which was only relieved by a residence at the sea-side and a careful use of iced champagne. In my third attack, three verrugas, the size of pigeons' eggs, made their appearance without any premonitory symptoms, causing no pain or inconvenience. Business compelling me to make a long and tiresome journey on a hard-trotting horse, which, considering that I had been out of the saddle for three months, was anything but agreeable, a violent congestion of the liver and lower portion of the lungs resulted, and was followed by the retrocession of the verrugas and a narrow escape from death. In my fourth attack I suffered but slight pain, the eruption making its appearance over the entire body, but none of the verrugas being larger than a pin's head. I am now in the most perfect state of health imaginable.

This disease is particularly liable to attack the mucous membranes; I have repeatedly seen verrugas coughed from the throat and lungs, and have found them in the intestines after death. Should a verruga locate itself upon the eye, it should by no means be disturbed, and only the simplest remedies should be used to combat any sympathetic irritation, allowing nature to take her own way to cure the disease. Troublesome cases occur occasionally in which the verruga locates itself in the urethra, causing mechanical obstruction to the passage of urine. The treatment is obvious; the patient must urinate, and hence a catheter must be passed, and retained if necessary, with a bandage upon the catheter to restrain hemorrhage.

ARE ECZEMA AND PSORIASIS LOCAL DISEASES OF THE SKIN, OR ARE THEY MANIFESTATIONS OF CONSTITUTIONAL DISORDERS?

BY

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THE suggestion of a local pathology of diseases of the skin is of comparatively recent date, and stands in bold contrast to the older humoralistic doctrine, which ascribed all these diseases to a morbid entity, a *materies morbi*, seeking exit from the system, and whose escape is beneficial; and the positive demonstration within the present century of a local cause of several cutaneous maladies (as dermatitis, the parasitic affections, etc.) which had, for ages, been regarded as expressions of blood states, is an achievement of which modern medicine may justly be proud.

This discovery of a local origin of certain skin lesions, and the fact of the resemblance between some of the forms of dermatitis and the eruptions of impetigo, lichen, eczema, etc., have led some to claim, still further, that most, if not all, of the diseases affecting the skin are either the results of local external causes, or are local diseases originating in idiopathically deranged action of the elements of the skin; a view, the adoption of which has undoubtedly been promoted by a natural reaction from the too exclusive, humoral doctrines previously entertained.

It need hardly be said that in arguing a constitutional origin and nature of eczema and psoriasis, I do not desire at all to return to the older humoralistic doctrine of disease, but seek to keep entirely within the bounds of recent chemical physiology and pathology, while at the same time I by no means ignore the importance of local cell-action, as will appear later. Nor does the recognition of a constitutional nature of eczema and psoriasis, by any means necessitate a belief that it is dangerous to check or remove these diseases by local measures, for experience has proved the contrary; it does, however, demand an investigation of internal causes, and a removal, as far as possible, of systemic disorders, since thereby elements of disease are removed and the general system benefited, while the eczema and psoriasis are cured.

In the further presentation of the subject of the nature and origin of eczema and psoriasis, it will be advisable to separate the two diseases more or less; I will, therefore, in each instance speak first of eczema, and then make the application to psoriasis; because, although they resemble each other in many important particulars, and have many points of contact, their dissimilarities are such as to cause confusion if they are in every instance spoken of together.

The local pathology of eczema and psoriasis has rested largely on three grounds: first, on the results obtained in the local treatment of these and other diseases of the skin, especially in the larger hospitals in Europe, notably those in Vienna; second, upon microscopical researches in histology; and, third, as far as relates to eczema, on a clinical and micro-

scopic study of the artificial eruptions produced on the skin by irritating substances, idiopathically and experimentally. We will dispose of the last of these first.

Now, while there is as yet no absolute proof that the pathological process excited artificially in the skin differs essentially from that taking place in acute eczema, we need not conclude that eczema is of local origin because its eruption resembles artificial dermatitis. If we accept one artificial eruption as eczema, we must accept all as such, from the large blisters following cantharides, heat, and cold, to the discrete, pustular eruption caused by croton oil or tartar emetic, or the slight erythema of the mildest irritant. Moreover, many of these artificial eruptions resemble erysipelas even more than they do eczema, and yet there will be few who will hold that true erysipelas is a local disease of the skin; again, the eruptions of croton oil and tartar emetic resemble those of smallpox and syphilis, without this being any argument against the constitutional nature of these latter, while the eruptions produced by the insect in scabies and by lice are frequently confounded with eczema. These lesions are all purely local, there is simply a local response of the cells and capillaries of the over-stimulated part; they are all but forms and varieties of inflammation of the skin, and are properly termed dermatitis, and bear no more relation to eczema than does the inflammation of a sprained joint to true rheumatism. In a very small proportion of persons, such irritations may become the starting point of true eczema, but in them I believe that the elements of the eczematous diathesis may always be discovered. A demonstration of the essential difference between these artificial eruptions and eczema is shown by the fact that it is not uncommon to induce dermatitis, as by a blister, for the cure of chronic eczema, as also of psoriasis.

It is necessary, therefore, in the present study to remember the distinction between eczema and dermatitis, for undoubtedly much that has been treated for the former is but the latter, and of course no argument can be drawn from therapeutical results obtained in dermatitis as to the local nature of eczema. In regard to psoriasis also, we have to clearly exclude, not only the scaly syphiloderm, but also all syphilitic influence; true psoriasis, the lepra of Willan, has nothing to do with syphilis, not even in the way of a later hereditary transmission, as has been suggested, nor must it be in any way confounded with the dry, scaly forms of eczema, or with the true leprosy, the elephantiasis of the Greeks.

Before proceeding systematically to examine the several points regarding the local or constitutional natures of these diseases, let us consider, for a moment, the question of the possibility of a double origin of the diseases under consideration, whether they can own two natures, being sometimes local and sometimes constitutional. In regard to the skin lesions in the contagious exanthemata and syphilis, no question exists that their origin is solely and always due to the introduction of a poison, which acts through the system, and that the cutaneous manifestations are but one exhibition of their effect. Passing now to such general diseases as gout and rheumatism, whose etiology is more deeply hidden, we do not doubt that the local phenomena observed in each are always the result of the same constitutional condition, some of the earlier links in the chain of cause and effect being recognized in functional derangement of certain organs, and a consequent sub-oxidation or imperfect elaboration of the elements of food, and an absence of healthy disintegration of tissue; and

when we speak of cold or external injury as being the cause of gouty or rheumatic inflammatory action in a part, we understand readily that it is only intended to signify that it was the *exciting cause*, which determined that that particular spot should be affected, or that the disease should develop at that particular time. The same line of thought might be extended to other diseases, showing that it is irrational to deny the constitutional origin of a disease unless its local nature can be established in a manner answering every requirement, as is the case in the parasitic diseases of the skin; quite as impossible is it to exclude local causation, when the constitutional nature fails to account for every condition.

Great error has, therefore, been made, I believe, by those who look only or mainly at the local causes, and argue therefrom a local nature of eczema, they forgetting the established principles in general medicine in regard to predisposing and exciting causes of disease. This is verified by the fact of the difficulty of producing a true eczema artificially, at will, in a person not subject thereto either in self or family (if indeed it can ever be done), and the impossibility of inducing a psoriasis by any local means.

Our conclusions then, thus far, are that eczema and psoriasis cannot be both local and constitutional diseases, that is, either exclusively, according to the case; and that the eruptions resembling eczema, artificially produced, are either ordinary dermatitis, with a strong tendency to spontaneous recovery, or are true eczema in eczematous subjects, in whom the exciting cause, instead of occurring in the ordinary way, has been artificially supplied, just as a gouty person might, by measures voluntarily applied, induce a true gouty inflammation of a joint. As before stated, we know of no claim to the production of psoriasis by local measures alone.

In the present study of the question as to whether eczema and psoriasis are local diseases of the skin or manifestations of constitutional disorders, I propose to develop the following points:—

(1) The nature of the eruption in disorders of the skin which are recognized to be constitutional, as the contagious fevers, syphilis, etc., drawing a comparison between eczema and psoriasis and these affections, and showing their points of difference.

(2) The nature of local diseases of the skin.

(3) The microscopic anatomy of eczema and psoriasis, with a view of comparison with that of the local skin diseases on the one hand, and with that of the constitutional on the other.

(4) The clinical history of eczema and psoriasis, in so far as they bear on the question, the points considered being (*a*) age, (*b*) sex, (*c*) location of eruption, (*d*) relapses, (*e*) hereditary transmission, (*f*) gouty and rheumatic symptoms, (*g*) urinary disturbances, (*h*) bronchitis, etc.

(5) The clinical history of some local diseases, to show the differentiating elements between constitutional disorders and those believed to be local, as epithelioma, verruca, keloid, parasitic and mechanical diseases of the skin, etc.

(6) The effect of local treatment, and how far the success of local measures necessitates a belief in the purely local nature of the disease.

(7) The effect of constitutional treatment, and the internal and general measures of service in eczema and psoriasis, to show how far their effect proves that the diseases which they remove are constitutional.

(1) In comparing eczema and psoriasis to the acknowledged constitutional disorders affecting the skin, contagious fevers, syphilis, etc., we must not, of course, press the similarity too far. We find, however, certain resemblances, as in the symmetry of development of the lesions, for eczema, if uninfluenced by local causes, will, when carefully studied, be found to exhibit this almost as clearly as psoriasis, whose bilateral disposition is so striking; the peripheral mode of spreading of eczema resembles much that of erysipelas, and the scattered eruption of psoriasis corresponds much to the mode of development of other exanthematous diseases, including syphilis. Eczema also is not infrequently attended with fever, in its more acute and general forms, and on the full, recent development of psoriasis there is more or less of malaise and prostration, and probably fever. The characters of the lesions in eczema and psoriasis are not entirely unlike those of constitutional diseases, which are marked by, first, congestion; second, exudation; and, third, if these former have been sufficiently severe, desquamation. The lesions of eczema and psoriasis are also superficial, and rarely, if ever, do they leave cicatrices, when uncomplicated.

(2) In contrast with these stand the characteristics of local diseases of the skin, marked by their utter want of symmetry (unless accidental), their extension depending either on a recognized cause, or being unexplainable, as in keloid, epithelioma, etc.; local diseases very rarely affect the whole integument, or even large portions, unless ichthyosis, of whose true nature, however, we know very little, be granted as such. Local diseases, moreover, acknowledge no constitutional connections nor fever; while, finally, the congestive element of eczema and psoriasis stands in striking contrast to its absence in local diseases, except, of course, where it is called forth by local stimulation. I am well aware that many cases of eczema present a number of the features of local disease, and that psoriasis occasionally is seen quite localized; but this is the exception, whereas it is rare to find exceptions in regard to the local diseases exhibiting any constitutional features; and, as we have concluded that eczema and psoriasis cannot be at one time local and at another constitutional, the weight of evidence, in this comparison, is decidedly in favor of the latter.

(3) The study of the microscopic anatomy of eczema and psoriasis is as yet in its infancy, and throws but little light on the etiology of these diseases; but, for a comprehensive view of the subject, we will briefly recall the main points in their histology, which will be seen to favor the constitutional rather than the local nature of the affections. The first anatomical change in eczema (reasoning from what is observed in artificial eruptions) is capillary congestion, resulting in capillary stasis and exudation in the tissue of the skin, producing more or less œdema. In some instances, not rare with us, the process stops here, or is arrested by treatment, and we have only the erythematous state, followed by moderate desquamation, the result of the impaired nutrition of the outer layers of the skin, a state very similar to the epithelial shedding in scarlatina, erysipelas, or measles.

If the exudation is too great to allow of absorption, it seeks egress through the external layers of the skin, or becomes organized as chronic infiltration. Biesiadecki believes that the fluid of eczema gains exit through certain spindle-shaped cells, which lie between the round and polygonal cells of the rete, these becoming so altered as to convey the fluid, as through direct channels. When the outer surface is still intact,

the hard, horny epidermis resists further progress and a vesicle forms; when the epidermal layer has been dissolved off or ruptured, the fluid oozes from the surface. Charpy[1] gives a somewhat different account of the genesis of eczema; still regarding the beginning of the disease as being in the corium, and consisting in a congestion of the layer just beneath the papillæ, he believes that the exuded fluid is absorbed by the deeper cells of the rete, in the intervals between the papillæ; that these cells become engorged until they rupture, whereby alveoles are formed; and that the increase of these alveoles terminates in the production of vesicles, upon the rupture of which cul-de-sacs are formed, whose walls furnish the further secretion.

In chronic eczema, the exuded plasma has become organized, and is seen as cell infiltration of the corium; the papillæ are much enlarged; the lymphatics, both in the papillæ and corium, are increased in size and dilated (Neumann); the bloodvessels are sometimes obliterated (Rindfleisch); and even the deepest parts of the skin are involved.

The anatomical and other relations of eczema have suggested to some a likeness between this and catarrh of the mucous membranes, and it may be well here to consider for a moment the nature of the mucous affection called catarrh, in reference to the bearing of this resemblance upon the nature of the diseases now under consideration. First, it must be remembered that it is no more proper to call all inflammations of the mucous membranes catarrh, than it is to name all those of the external integument eczema, or even dermatitis; the term catarrh but signifies the exudative feature, and the word eczema implies much the same. We have already separated and clearly differentiated many diseases of the mucous membranes, and it is highly probable that future study will show that there are many very different processes included in what is yet called catarrh, and ultimately their nature and origin will, we suspect, be as clearly defined as are now those of cutaneous diseases, or, it is hoped, far more clearly. That this is true may be judged from our present knowledge; thus, we have simple intestinal catarrh caused by the direct irritation of indigestible substances, a mechanical affair corresponding to ordinary dermatitis; we have an incontrollable diarrhœa, or catarrh, from the irritation caused by the circulation of effete substances in renal disease; there is also a mechanical catarrh from portal congestion in cirrhosis; we have also gouty affections of the various mucous surfaces; likewise the catarrhal complications of measles, those of syphilitic origin, etc., and I have long believed that much of the ordinary bronchial catarrh was truly eczema of that surface. Therefore, in likening eczema to catarrh of the mucous membranes, we by no means simplify matters in regard to its local or constitutional nature. Moreover, the common observation of catarrhal affections following a chilling of the surface, does not indicate necessarily their local nature, due wholly to the cold, for we see great differences in the results produced, dependent evidently upon constitutional states, the congestion due to the action of cold on the integument being only the exciting cause.

We will now consider for a moment some views in regard to the local nature of eczema propounded by Dr. Tilbury Fox, in the Lettsomian Lectures for 1869 and 1870.[2] He suggests that the capillary congestion is a *consequence* of cell-activity, arguing that mere capillary excitement

[1] Annales de Dermatologie et de Syphiligraphie, t. iii, p. 100.
[2] Eczema, its Nature and Treatment; London, 1870.

cannot give rise to eczema, because otherwise the erythemata would constantly overstep their limits. The element of the influence of the nervous system is largely considered by Dr. Fox, who believes that this abnormal state of the cells may be in response to "perverted innervation," as suggested by Hebra some time since; but while Hebra limited the action of the nerve influence to the production of "congestion and other disturbances of the circulation," Fox considers its influence in inducing cell proliferation a very necessary point to be admitted in eczema. That the latter is very true is shown by alterations of tissue plainly following and due to nerve lesions, and the same might be expected from the distribution of the nerve fibres even to the cells of the rete and epidermis.

But while a step in advance is made in recognizing the direct agency of the cell elements of the skin in the production of eczema (and independent cell activity can be no longer denied, as is shown in leucocytes), and while the influence of the nervous system in the production of cell changes is acknowledged, there is no proof that this cell change is really primary in eczema and psoriasis, and independent of constitutional conditions, and that it exists as a local affair; and this view finds little support beyond theory. It is a field, however, in which research is needed, and may perhaps be carried on with advantage, namely, the microscopical investigation of the apparently healthy skin of markedly eczematous subjects, both before and after local disease, as also a study of its nervous relations, both clinically and histologically. Should repeated, careful, and well verified microscopic studies demonstrate cell change in the earliest erythematous stages of eczema, and possibly also in the elements of the skin of eczematous patients before or after disease, it would do much to enlighten what otherwise must remain uncertain, namely, the part that the diseased tissues take in eczema, how far they are primary and how far secondary. The skin is the most favorable, if not the only, portion of the body on which very early pathological changes can be observed and followed, and research in this line may throw light upon the pathological histology of other organs.

That independent cell-action has much to do with inflammatory and other changes in the skin, is evident from the results of mechanical and chemical irritation, where, as in the case of the ordinary blister, the intervention of any blood influence cannot be suggested with reason; and that the cells are under very direct control of the nervous system is also very plain from the lesions in zoster, elephantiasis Græcorum, those occurring after nerve injuries, etc.; here the cells which under normal nerve control absorb just enough nutriment to maintain their proper relations, under perverted nerve control take on morbid action. Anatomical studies of the nerve trunks and branches in eczema might perhaps assist in our knowledge of its nature.

Psoriasis has been less studied microscopically than eczema, and little or nothing has yet been published of its genesis. The most striking histological changes in sections from older patches, as is known, consist in a great papillary enlargement, with thickening of the rete Malpighii to a corresponding extent, or nearly so. The papillæ are crowded with new cells, disposed mostly about the bloodvessels, with some infiltration of the corium; but the infiltration is not nearly as extensive as in chronic eczema, as might be judged from the clinical appearances.

Wertheim, from a microscopical study of psoriasis, concluded that the patches were due to some peculiar change in the capillaries of a circumscribed spot. Neumann considers that what Wertheim thought were

enlarged and blocked-up capillaries were cell groups around them, but offers no suggestion as to the mode of origin of the patches. By the kindness of my friend Dr. A. R. Robinson, of New York City, I have had the opportunity of examining microscopically some sections from psoriasis, which confirm Wertheim's views, and throw some light on the pathological histology of this disease. The earliest specimen was taken from a papule of psoriasis but two weeks old, in which there had been but little scaling. The bloodvessels in the papillæ were very distinctly seen to be greatly enlarged, and in some places appeared to approach the surface of the epidermis very closely; in quite a number of places the bloodvessels had ruptured, and microscopic ecchymoses were plainly visible. In specimens from rather older patches the bloodvessels showed very distinct budding processes, and some of the vessels appeared completely blocked up; there was little or none of the new deposit of cells described by Neumann, and there was no possibility of mistaking the enlarged bloodvessels for cell heaps around them.

It will be thus seen that the microscopic changes in eczema and psoriasis do not differ so very much from those observed microscopically in some other chronic skin diseases, and that histology contributes but little to show the local nature of these diseases, except so far as relates to the studies of Wertheim, further verified by those of Dr. Robinson; and yet here no one can show that even those changes in the bloodvessels are primary. Fox, however, believes that psoriasis "consists primarily and essentially in a misbehavior of the cell-elements themselves—a perversion of the ordinary cell-life of the epidermis—a true tissue disease, in which the trophic nerves probably play the chief part," but, unfortunately, gives no reasons for his interesting supposition.

While this latter view finds little or no support in anything that has yet been reported of the pathological histology of psoriasis, there is a very slight foundation for it in a certain resemblance which can be made out between psoriasis and a disease of well-recognized local nature, namely, epithelioma, to which we would venture to devote a few words.

The elements which have suggested the comparison between the two diseases are: a similarity in some of their clinical features and modes of development, their anatomical disposition, the transformation of patches on the tongue, called psoriatic, into epithelioma, the asserted family relations of the two affections, and the occurrence of epitheliomatous growths on the faces of young persons affected with psoriasis. In regard to a similarity in their mode of development, the increase in both is by peripheral extension, to the extent often of forming a ring, with healing within. Anatomically, epithelioma is an outgrowth, or rather an ingrowth of epithelial elements, whether the more external ones of the skin or those lining glands; we have, as yet, no studies with which I am acquainted to prove it, but the true pathology of psoriasis may be an epithelial hyperplasy of somewhat similar nature, and the lengthened papillæ seen in it may be the result of the ingrowth of the rete. We have the same shedding of the outer epithelial layers in psoriasis as in epithelioma; on scraping both, by no means forcibly, not only are the outer dried layers removed, but the softer and more succulent portion next above the papillæ is also separated, and we have in both the bleeding, familiar to all. The presence of the fibrous papillæ in psoriasis prevents further progress in the scraping, or we might induce a destruction similar to that in epithelioma.

The only real histological support I have to offer to sustain this

theory of the ingrowth of the epithelial layer in psoriasis, other than the well-known increase of the inter-papillary portion of the rete, is found in some of Dr. Robinson's sections, in which, in addition to the capillary change already alluded to, the sheaths of all the hairs showed outgrowths at their lower portions similar to those which have been observed in lichen ruber.

The transformation of what has been called lingual and buccal psoriasis into true epithelioma is now proven beyond doubt; of the true nature of the former affection, and its relations to ordinary psoriasis, however, we have little reliable information; it is certainly rare to find these mouth lesions in psoriatic patients, and many who have them have never had ordinary cutaneous psoriasis, and it is highly questionable if there is the slightest relationship between them.

Gaskoin affirms very confidently the hereditary relations of psoriasis and epithelioma. I have occasionally found both diseases in the same families, in intelligent private patients, but have never been able to satisfy myself of any relationship; the cases, however, have been far too few, and the time which has elapsed since I first investigated the connection, far too short, to form any conclusions therefrom. I have recently treated a young man with well-marked psoriasis, who exhibited perfectly developed epithelioma in front of the left ear. I would suggest this relationship between psoriasis and epithelioma as a profitable field for research, clinically and microscopically. If a relationship can be established, the local nature of the former may be demonstrated; for the present, however, this is not proven, and the constitutional relations of this disease, as well as of eczema, are so abundant and conclusive that we must here grant them the weight of argument and fact.

(4) We will now turn to a consideration of the constitutional relations of eczema and psoriasis, as exhibited in their clinical history. In regard to the age at which they occur, eczema affects all, from the cradle to the grave; none are too young, none too old to suffer from it. This is more the character of constitutional than of local disease; cancerous affections seldom, if ever, are seen in the young; warts (of the ordinary variety) seldom in the old; keloid very generally in middle life, both extremes being spared. The vegetable parasitic diseases rarely occur in the old; whereas in them are found changes in the skin, and diseases therefrom, not seen in young life. Psoriasis may be also said to prevail at all ages, though less frequently very early or very late in life.

Eczema and psoriasis both occur about as frequently in the male as in the female. In location, eczema spares no part of the surface, and, I believe, may affect every portion of the mucous membrane as well, while its development is very commonly symmetrical; these features are true also of psoriasis, and are those of general and constitutional diseases rather than of those of local nature. The tendency to recurrence in the same parts might be claimed as a local feature, but, in regard to eczema, the same exciting cause may be present again and again; and it must be conceded that tissues are undoubtedly weakened by disease, and more likely to be affected the second time than sound tissues; this is observed in gout and rheumatism, as also in affections of the mucous membranes.

But relapses in eczema and psoriasis furnish more indications of a constitutional than of a local nature, for fresh attacks of eczema are very commonly found to follow a depression of vitality; and I believe that this is true of psoriasis far more commonly than is generally conceded. Moreover, these eruptions by no means occupy the same locations each

time. I have frequently observed that the spots of psoriasis fall almost if not quite as often on new tissue as on the sites of a former disease.

There is no doubt as to the hereditary character of eczema and psoriasis. Whether the heredity consists in any general, constitutional habit, or whether it is simply a transmitted tendency to tissue-change, cannot be shown at the present time; in favor of the latter may be cited the transmission of mental and physical characteristics, cancerous disease, and ichthyosis; as instances of the former, we have the inheritance of syphilis, gout, and rheumatism, and the history of the occurrence of gouty, bronchitic, hepatic, and urinary troubles in the family of eczematous and psoriatic patients, in such a way as to show a connection.

If the claim of an inherited, perverted tendency of tissue could be established in eczema and psoriasis, which tendency lies dormant until called into activity by some exciting cause, it would still fail to explain alone why a cause which is at one time inefficacious, will, on another occasion, even in a much less degree, be sufficient to produce the disease.

This brings us to the study of the gouty and rheumatic symptoms, the urinary disturbances, and the bronchitis of eczema and psoriasis. That patients with these affections are the frequent subjects of the systemic derangements connected with or tending toward gout and rheumatism, as well as to the complete manifestations of these diseases, is very certain; this observation is of ancient date, and is strikingly confirmed by recent science. It is not very uncommon to find attacks of gout alternating with those of eczema and psoriasis, and clinical study shows that various disorders which may properly be classed under the name of functional derangements of the liver are very common in the subjects of these two diseases of the skin.

These systemic disorders are shown in the urine, which undergoes functional derangement in a large proportion of the cases of eczema and psoriasis. In a recent contribution to and study of the subject by the writer,[1] the following conclusions were reached: "Eczemic patients seldom pass large amounts of urine, the tendency being to scanty secretion, almost always unnaturally acid, with a specific gravity averaging above normal. Free uric acid, the urates, and oxalate of lime, abound; sometimes oxaluria is very persistent. Albumen is rarely seen. The urea and uric acid are often below the normal standard, although they may be in excess when a large portion of the integument is affected; indican has been found in pathological quantities. When the specific gravity is high, it may be due to an increase in the sulphates. The chlorides are diminished. Psoriasis sometimes alternates with phosphatic urinary deposits in gouty subjects. The fixed salts are increased. The urine shows generally a hyper-acidity, with deposits of uric acid, urates, and oxalate of lime, the contrary being the exception. The specific gravity is liable to great and unaccountable variations."

It may be claimed that these urinary changes are observed in persons also with no cutaneous disease, and that therefore there is no connection between the two. I am not yet in a position to prove exactly what changes in the urine necessarily accompany eczema and psoriasis, but mention those that have been actually observed to show that patients with these diseases are not in the perfect health often claimed for them, and, although a casual examination will often fail to detect any disease other than that of the outer integument, I assert that searching investiga-

[1] Archives of Dermatology, October, 1875, p. 1.

tion will seldom fail to demonstrate elements of disordered health, very commonly shown in the condition of the urine. And as these are removed by appropriate remedies, the eczema and psoriasis improve, and *vice versâ*.

Not less striking, as indicative of the constitutional relations of eczema and psoriasis, is the occurrence of bronchitis and asthma, conjointly or alternately, in the patient, or in his immediate family; the truth of this is well attested by many writers.

The neurotic relations of eczema and psoriasis are also very interesting, and indicate other than a local nature of the disease; but the subject is yet too recent and unsettled for more than a mention in this place: I have elsewhere[1] given a fuller study of it. Recorded facts, based on large clinical experience, are needed greatly for the full and satisfactory determination of many of the points here alluded to.

(5) Enough has perhaps already been said in reference to the clinical history of local diseases, but a few points may be again alluded to with advantage in connection with what has just preceded. No constitutional relations have been recognized, to my knowledge, in any of these; constitutional remedies have no effect upon them. Their unsymmetrical character and irregular development have been mentioned, as also their removal solely by local measures. Local diseases of the skin of external origin are represented by the parasitic affections and dermatitis; idiopathic misbehavior of cells is exemplified by epithelioma and keloid; herpes zoster may be taken as a type of an acute local disease of the skin dependent on nerve elements; and ichthyosis of a chronic, local disturbance of general cell nutrition; the clinical history of each of these is familiar to all, and quite different from those of eczema and psoriasis.

Hebra, as is well known, has always stood as the representative of local pathology and therapeutics, and it may not be uninteresting or unprofitable to refer briefly to his present views in regard to the purely local nature of eczema, as found in the recent, second edition of his work.[2] That I may not be charged with misunderstanding him, I will quote a section from the close of the long chapter on the Etiology of Eczema (p. 462). After repeating what had been said in the former edition to show the local nature of the disease, and after stating his disbelief in any diathetic cause, he says: "While, therefore, we cannot accede to any peculiar or proper dyscrasia, we must, on the other hand, confirm the fact that certain conditions of the human organism, partly transient, partly permanent, at one time increase, and at another time diminish its susceptibility to agencies producing eczema. These physical conditions are called a disposition, or predisposing cause, *momentum disponens*, to distinguish them from the direct exciting cause of irritating agencies, and we are obliged to recognize these elements in the etiology of eczema, because experience confirms it."

"For example, we see an eczema on the hands and forearms of a young girl who has been engaged in washing soiled linen, and we declare that the origin of the eczema is in the action of the lye, soap, hot water, and friction. Now, at the same time with this girl, there are many other females washing in the same lye, using the same soap, and living under the same circumstances, without acquiring eczema. Indeed, this

[1] Archives of Electrology and Neurology, November, 1874, and May, 1875. Reprinted by G. P. Putnam's Sons. New York, 1875.

[2] Lehrbuch der Hautkrankheiten, Zweite Aufl.; Erlangen, 1874.

very girl, who now has eczema, has been exposed to the same influences previously without becoming affected. What is the cause of her present susceptibility? A careful examination of her general condition will give the explanation. The girl who before was healthy, robust, and regular in her menses, has now lost her appetite, has become sluggish and languid, her appearance is pale and bloated, her menstruation is profuse; in a word, she has become chlorotic, and thereby eczematous. The remedies suitable for the chlorosis are now employed; the appetite and power of work return, the menses become regular, and the eczema disappears in spite of the continued influences of the agencies causing it. The same observation is made in reference to pregnant and nursing women; also in those suffering from chronic sexual disturbances. The latter must always be looked upon as favoring elements (a *momentum disponens*, or predisposing cause) which induce a *status minoris resistentiæ*, and allow an otherwise ordinary skin irritant to become an exciting cause, a *momentum excitans*."

After again claiming that we need no blood explanation of eczema, Hebra adds: "In order not to be misunderstood, we will, however, here again state that every eczema is not caused by a local irritation, but that it may be occasioned by diseases of the rest of the system."

Such clear and unqualified statements as to the constitutional nature and origin of eczema are unexpected from one so prominently known in connection with local pathology, and show that his opinions have latterly undergone considerable change. It must be remembered, however, that at the time of the first edition of Hebra's work, upon which his reputation as a local pathologist greatly rests, many of the crude theories of the past had not yet been completely overthrown; and also that within his experience many diseases which had formerly been considered constitutional, had been demonstrated to be purely local, as scabies, the vegetable parasitic diseases, plica polonica, etc., and that the success of local measures seemed to demonstrate to him that other affections were also local; he has, however, the candor to acknowledge his mistake.

(6) We come now to one of the most interesting and at the same time most disputed portions of our investigation, namely the relative effects of local and constitutional treatment as indicating the nature of eczema and psoriasis. Hebra and Neumann declare, in the most positive terms, that it is quite unnecessary to regard the constitutional relations of these diseases, except when they are striking, and those who have followed the Vienna School for any length of time can testify how completely this idea is carried out there. External remedies and measures are used there almost exclusively, it being the greatest exception to have any questions asked in regard to the general health; and internal medication, it may safely be asserted, is very rarely, if ever, employed in the hospital, in eczema and psoriasis, except by way of experiment; and yet these eruptions are removed in a very satisfactory manner. Does this, however, prove the local nature of the disease? In negation of this I need but suggest how the stiffness resulting from gouty and rheumatic inflammation is benefited by external counter-irritants and passive motion, while the origin of the serous inflammation and fibrous thickening is recognized as of constitutional origin. Syphilitic iritis may be subdued by atropine alone, and tubercular laryngitis receive great benefit from topical treatment; the ulcers of dysentery may be cured, and the discharge of leucorrhœa arrested, by local measures; and yet these diseases be manifestations of constitutional states.

But there is not the slightest influence exerted by local treatment in preventing the return of eczema and psoriasis, whereas we know that constitutional measures have considerable power to prevent relapses; this is, as is well known, less true of psoriasis than of eczema, but mainly, I believe, because of the neglect to carry out faithfully the proper measures for a sufficient length of time after the disappearance of the eruption. Again, in this country at least, local measures often fail to accomplish the desired end until constitutional measures are resorted to in addition. In considering the effect of local treatment as an argument for the local nature of eczema, it is well to bear in mind the distinction drawn early in this paper between dermatitis and eczema, for many cases treated as the latter disease are entirely local in character, the result of local irritants, possessing in themselves a strong tendency to heal when the irritating cause is removed and the inflamed surfaces protected, that is, are cases of dermatitis and not of eczema.

I believe true eczema and psoriasis to be constitutional states wherein ordinary irritants give rise to inflammatory changes in the skin, which form an important element of the disease, but which are no more its sole element than are the cutaneous phenomena of the exanthemata, leprosy, syphilis, etc. I believe also that single attacks of eczema, and of psoriasis, too, correspond in nature very much to those of such diseases as gout and rheumatism, in which the local changes have a greater or less tendency to self-limitation, although the effects of the disease may long remain. That is, as arthritic, pulmonary, or cerebral symptoms appear to be the culmination of blood-processes which we know as gout and rheumatism, so eczema and psoriasis are directly dependent upon somewhat similar, although as yet little defined, blood changes, the acute symptoms corresponding to those of rheumatism and gout, while the chronic local alterations in the skin answer to the gouty concretions on and in the synovial membranes, and to the rheumatic thickening of the valves of the heart or around joints long inflamed; and, as in gout and rheumatism, there is a tendency for the attacks to recur again and again, the health in the mean time seeming to be perfect, so in eczema and psoriasis the local phenomena may be manifested from time to time, without any very apparent constitutional disorder between, while the *product* of the disease, the cell infiltration, remains.[1]

Now but little can be done to arrest the acute phenomena of eczema by local means, other than soothing applications, and it is very difficult to check a developing psoriasis by outward measures; but the *results* in both may be removed by local stimulants, which induce cell activity within the bounds of health. I speak more especially of these diseases in adults, for infantile eczema exhibits more plainly the constitutional elements whose great activity renders local measures often very futile.

While, therefore, the arguments are against eczema and psoriasis *being* local diseases of the skin, it must be admitted that they *become* local diseases in their skin lesions, and as such may be amenable very largely to local treatment, as far as relates to the single manifestations of disease at any one time.

(7) Whatever may be the views of those who hold exclusively to the value of local treatment in eczema and psoriasis, no doubt whatever exists in my own mind, nor, as far as I can learn, in the mind of a large

[1] "The Management of Eczema," by L. Duncan Bulkley, M.D., Trans. Am. Med. Association, vol. xxv. 1874, p. 121. Reprinted. G. P. Putnam's Sons, 1875.

share of those who make skin affections a special study, that constitutional measures are both valuable and necessary to the cure of most cases of these diseases. And by constitutional treatment I do not mean arsenic alone, nor any one specific remedy, for truth demands the acknowledgment that specifics for diseases do not exist, unless it be with the two exceptions of mercury for syphilis and quinine for malaria. But by constitutional treatment I understand the employment of every measure for the cure of disease, which cannot truly be called local treatment.

Now my experience has been, as already stated, that most patients with eczema and psoriasis present anomalies in their urinary secretion, and in proportion as this exists have I found the congestive feature of the disease to be marked, and in the same proportion have I seen the diseases benefited by cathartic and diuretic remedies. A not inconsiderable number of patients with eczema and psoriasis are habitually constipated, and the diseases, with me, resist all local and other measures until this is remedied. Many are what is called bilious, and nitric acid will do more to cure their skin diseases than any other means. It is not at all uncommon, in this country, to find eczema and psoriasis in those who are extremely nervous, and cases are constantly met with where an enormous mental strain is keeping up the disease; here the constitutional measure of rest, with perhaps change of scene and climate, is imperatively demanded, together with nerve tonics of various kinds. In dispensary practice in this city a good portion of all eczematous and psoriatic patients exhibit the features known as strumous or scrofulous, and are more quickly and surely cured by the internal administration of cod-liver oil than by any other remedy, local medication either being used not at all, or playing a very unimportant part. Arsenic also is capable of entirely curing, that is, removing, eczema, and that tolerably quickly, *without the use of any local measures whatever*, as I have repeatedly witnessed and elsewhere shown,[1] and its power over psoriasis is acknowledged on all sides. Iron is indicated in a large share of our eczematous and psoriatic patients; others require such remedies as rhubarb and soda; and the vegetable tonics play an important rôle in our therapeusis.

Diet, hygiene, exercise, and bathing, are all essential in my opinion to a successful management of these diseases. I claim that patients who have been thus carefully managed not only recover from the individual attack under proper local measures much more quickly than when the latter alone are used, but that they most certainly are in a better condition to resist future attacks, and that relapses are far less frequent, and I claim that the clinical history of the patients shows this. The action of these measures is not local, is not directed to the skin; but by virtue of the influence exerted on the general economy, as well as on the affected tissues, they cure the diseases in question.

In this study I have not referred much to the opinions of writers on dermatology, because, with the exceptions which I have mentioned from time to time, the opinion is very general that eczema and psoriasis are constitutional disorders. By a constitutional disorder, as we now understand it, is not meant the existence of some peccant material or *materies morbi*, which the system endeavors to throw off; nor that the discharge

[1] Trans. Amer. Med. Assoc., vol. xxvii. 1876, p. 163; New York Med. Journ. Aug. 1876, p. 113; Reprint, "The Use and Value of Arsenic in the Treatment of Diseases of the Skin." New York, Appleton, 1876.

is in any way curative or beneficial to the system; nor that the removal of the cutaneous lesion can in any way or manner act prejudicially to the economy; but the expression, constitutional disorder, is understood in the same sense in which it can be applied to gout, rheumatism, chlorosis, leucocythæmia, scorbutus, etc. And it is used here in especial distinction from local disease—that is a pure and specific disease of the tissues of the skin itself, singly and alone—as the artificial eruptions, parasitic diseases, epithelioma, verruca, keloid, etc.

There are several elements of causation in infantile eczema which have not been touched on, partly because of want of time, and partly because observations are wanting as to their true effects in producing the disease. One is the occurrence of the exanthematous diseases; it has been observed by some that eczema followed soon upon these affections, and it has been thought that they left such an impression upon the cell-elements of the skin that these readily took on eczematous action, simply on the application of an external irritation. Now if they can have any causative effect in eczema, the exanthemata can also induce psoriasis, lichen, urticaria, etc. Whether, however, they act in any other way than as depressants, leaving the body and tissues in an enfeebled state, I cannot tell; it will be remembered that Mr. Hutchinson has recently claimed that the later lesions of syphilis are but the remains of the earlier, general, dermal phenomena. If the exanthemata had any real power to impress any tissue change on the skin, which should afterwards result in eczema, we could hardly conceive why this should happen in such a small proportion of cases; for, of the multitudes who go through these inflammatory skin affections of childhood, but few suffer from eczema afterwards; while many children have eczema long before undergoing one of the exanthemata. We cannot therefore accept any argument as to the local nature of these diseases drawn from the previous occurrence of the exanthemata.

Vaccination is another process which we not rarely see followed by eczema; do we suppose here a local cause (other than exciting), that is, does it induce any change in the cells of the skin whereby they are more prone to eczema? I believe not, because of the very general adoption of vaccination, and the very few cases of eczema immediately following. I need not in this presence state that there can, of course, be no blood-poisoning, no contagion of eczema by means of the lymph, even when taken from a vaccinifer suffering from eczema; we know of no transmission by this means except that of syphilis, unless it be a tendency to carbuncular inflammation, of which I have observed a number of cases, and which my friend, Dr. Frank P. Foster, tells me he occasionally meets with. The only agency which vaccination can exert in calling forth an eczema, is, I believe, simply that of a local irritant, acting in the same way as a burn or other excessive stimulation. I have seen psoriasis of a very marked character develop for the first time immediately after vaccination, but the event must of course be looked upon as a coincidence, or else we must conceive that the local tendency to cell-proliferation has furnished the starting-point, the fire which kindled the train all ready for ignition. The rarity of the observation of psoriasis immediately following vaccination shows the correctness of this view.

Equally useless and uncertain is it, in my judgment, to look upon dentition in any other light than as an exciting cause, not acting locally, it is true, but probably as a reflex irritation, and as such I believe it to be of more or less importance; but in view of the small proportion of teething

infants who have eczema, and the infinitely smaller ratio of those with psoriasis, other elements must be looked to for the real etiology of these diseases; this is shown also by the frequent occurrence of these diseases in those of older years, and before the teeth begin to appear.

Erroneous diet, however, I consider as an all-important and undoubtedly prolific cause of eczema in infants, a point which we cannot here enter upon, except to mention it as of weight in favor of the constitutional origin of the diseases under consideration.

That local causes are of very great importance in the etiology of eczema in infants, as well as in adults, is beyond doubt; such causes are the use of irritating diaper-linen, harsh bandages, too frequent and careless washing and drying, incautious exposure of the face to cold air, hot caps on the head, harsh attempts to remove the dried sebaceous matter from the scalp, etc.; but that the disease very frequently does not get well upon the removal of these irritants, and under local measures alone, is certain, and that a proper study and direction of constitutional measures will cure the disease, if proper local treatment is employed, is equally sure.

Psoriasis is observed so rarely in those of very tender years, that no conclusions can be formed respecting it; if indeed the disease in infants differs in any way from that of adults.

I have said nothing yet in regard to the relations supposed to exist between scrofula and eczema and psoriasis. This is a very difficult subject and one which needs clearing up, there being thus far no sufficient data upon which to base a scientific investigation of the matter; for, although many authorities state that there is a connection, the evidence of any causal effect is far from being established. Neumann states that among 308 eczematous children he found but 30 who were rachitic and 70 who were scrofulous, while among more than 3000 scrofulous and rachitic children he found none with eczema.

For the proper study of this relation, we need to have the true position of scrofula more clearly defined than at present, its causes and symptoms more sharply drawn, and further statistical details, such as those of Neumann, recorded. That quite a share of children with eczema, among the poor, exhibit some of the features called scrofulous, that is pale, pasty skins, light hair, eyes, and complexion, enlarged glands, ophthalmia, otorrhœa, etc., is not to be questioned, but, that this state has any special effect in causing eczema, other than its depressing power, cannot now be proved. It is certain, however, that eczema and psoriasis exhibit somewhat different features when occurring in these subjects; the tendency to pus formation is evident in the secretion from eczema, and in psoriasis the scrofulous state is shown by a thickened and more yellowish condition of the scales.

Although less commonly thought of, there is probably more connection between what is called a scrofulous state and psoriasis than there is between the former and eczema. Gaskoin thinks that phthisis plays an important part in preparing the way for psoriasis, and I have been rather inclined to think the same; investigations are, however, needed here.

The results of this inquiry as to the local or constitutional nature of eczema and psoriasis may be summed up in the following propositions:—

I. Eczema and psoriasis are diseases *sui generis*, and are not to be confounded in any way with other states; as, the former with artificial

dermatitis, and the latter with the eruptions of syphilis, scaly eczema, or leprosy.

II. Eczema and psoriasis cannot own a double, independent causation, or nature, at one time local, and at another constitutional; but, with other diseases, they may have a twofold cause, namely, a predisposing and an exciting.

III. Eczema and psoriasis in many of their features resemble the accepted constitutional diseases more than they do those recognized as local.

IV. The skin lesions of eczema and psoriasis, as also their microscopical characters, more nearly resemble those of the general and constitutional diseases affecting the skin, than they do those of purely local diseases.

V. Eczema is very properly likened to catarrh of the mucous membranes, but no argument as to its local nature can be drawn therefrom; it is very probable that some proportion of the mucous attacks called catarrh are eczema and psoriasis of this tissue.

VI. Both eczema and psoriasis resemble gout and rheumatism, in certain respects, and are dependent upon a somewhat similar, although as yet unknown, constitutional cause; much of the skin lesions, as ordinarily observed, must be looked upon as the *local results* of the diseases, removable by local means.

VII. There as yet exists no microscopical or physiological proof that eczema and psoriasis are the sole result of local cell disorder, either congenital, acquired, or due to perverted nerve action; although from our present knowledge of independent cell activity, and from the intimate connection between nerve elements and the cells composing the skin, it is highly probable that cell action and nerve influence are important factors in eczema and psoriasis.

VIII. Local causes play a very important part in the etiology of eczema; they are probably inoperative in psoriasis.

IX. Certain relationships between psoriasis and epithelioma have been claimed, which require much further investigation; at present they are not established, and are no proof of the local nature of psoriasis.

X. The clinical history of eczema and psoriasis furnishes much evidence of constitutional relations, mainly with states allied to gout and rheumatism; no direct causal connection has yet been demonstrated between the scrofulous state and eczema and psoriasis.

XI. Local treatment alone is often insufficient to remove the lesions of eczema and psoriasis, and cannot prevent or delay relapses; its success does not demonstrate the local nature of these affections.

XII. Constitutional treatment, alone and singly, can cure many cases of eczema and psoriasis, and prevent or delay relapses in a certain proportion of cases; by constitutional treatment is intended every agency not properly placed among local measures.

XIII. The total weight of evidence and argument is that eczema and psoriasis are both manifestations of constitutional disorders, and not local diseases of the skin.

In closing this paper, already far exceeding the limits designed, the writer would beg indulgence for its imperfections, of which he is fully aware; some of these have been necessitated by the vastness of the subject and the small amount of space afforded in which to consider

them. Many points which had been elaborated much more, have been necessarily condensed or omitted, and many fields, an investigation of which would have further developed the subject, have been left entirely untouched; many have been barely alluded to, because of the scantiness of reliable data. It is to be hoped that the investigation will be pursued still further, for the etiology and pathology of these diseases may be considered as at the foundation of all dermatology, and the importance of some of the questions here raised can scarcely be overestimated.

The following are some of the points requiring further study: the mode of the formation of vesicles in eczema, and the way in which the exuded liquid reaches the surface; the state of the skin, histologically, very early in eczema and psoriasis, as also after the disappearance of the disease, and likewise of the skin of those subject to eczema and psoriasis in places where no disease has yet appeared; the nerve relations of these diseases clinically, and the state of the nerves microscopically; the condition of the bloodvessels very early and very late in these diseases, especially in regard to Wertheim's view of a capillary change being the cause of psoriasis; the resemblance between psoriasis and epithelioma, clinically and microscopically, as regards also the development of what are called psoriatic lesions of the mouth into epithelioma, and the true nature of the former and their relation to psoriasis, if any; also the possibility of the origin of psoriasis in hyperplasy of the rete downwards between the papillæ; the urinary relations of eczema and psoriasis, also their heredity, as relates to the diseases themselves, and also to other constitutional manifestations; the asserted connection between the eruptions of the exanthemata and eczema, especially in infants; the influence of dentition in the production of eczema; the effect of vaccination; and, finally, the relations of scrofula to eczema and psoriasis.

THE VIRUS OF VENEREAL SORES, ITS UNITY OR DUALITY.

BY

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THE question, the discussion of which I have the honor to open to-day, is expressed in the following terms:—"THE VIRUS OF VENEREAL SORES, ITS UNITY OR DUALITY."

In presenting this question, the first inquiry that suggests itself to one's mind is whether it is identical with the question of the "Unity or Duality of Syphilis," that, for the last twenty-four years, has excited so much interest and discussion in the medical world. I reply that it *is* essentially the same. To ignore this fact, to attempt to assume a new departure, to consign the question of "unity or duality" to the tombs as having no bearing upon our present views, would be a fraud upon medical history and a source of confusion to the student; since those who have so earnestly advocated the so-called "duality of syphilis," have never for a moment supposed that there existed two kinds of syphilitic virus, but merely that there was more than one poison at work in the etiology of a complex disease known for many years under the name of syphilis; and he who now recognizes a duality of poisons in venereal sores, is a syphilitic "dualist" in the sense in which that term has been employed. It will be seen, however, that the term "duality of syphilis" has always been a misnomer, and the form of the question presented to us to-day is most appropriately substituted for it.

I take it for granted that the question proposed to us is intended to include the poison or poisons of every kind productive of venereal ulcers, and that the term virus is to be used in its broadest sense, viz., that of *contagious principle* or *poison*. When speaking of a special poison supposed to be incapable of generation *de novo*, I shall call it a *specific virus*.

Allow me also to add that I shall use the terms *chancre* and *chancroid* in the sense which is commonly received in this country—chancre as indicating the initial lesion of syphilis, and chancroid as indicating the "soft chancre" of the French school, and the "chancre" of the German school.

Three views as to the origin of venereal sores have been entertained:—

(1) All venereal sores are due to a single, specific virus, the virus of syphilis.

(2) Some venereal sores are due to the syphilitic virus, and others to a distinct specific virus, known as the *chancroidal*.

(3) Some venereal sores are due to the syphilitic virus, and others to the inoculation of the products of simple inflammation, in which latter case no specific virus exists.

The clinical observations and artificial inoculations, which have been brought forward, for and against these several theories, must be familiar

to those here present, and I shall therefore limit myself to reviewing them and drawing my own inferences from them, as a basis for the discussion which is to follow. Indeed, any other course would be impracticable within the limits of this paper.

The view first mentioned, viz., that all venereal sores are due to a single virus, the virus of syphilis, had been the prevailing one for nearly three centuries prior to the year 1852. At the same time, it had not escaped the notice of many observers that the results of contagion were by no means identical; that, in some cases, the persons infected showed no symptoms after the healing of their ulcers, while others developed a train of symptoms, lasting through years, and even transmissible to their children.

In the year 1852, Bassereau claimed a distinct cause or origin for each of these two classes of cases. He founded his claim, first, on the history of venereal sores, whereby he endeavored to show that, although contagious ulcers of the genital organs, communicated in sexual intercourse, had been well known to the ancients, yet that the constitutional disease which we call syphilis made its first appearance in Europe in the latter part of the fifteenth century.

While believing this argument drawn from history to be a valid one, I would not insist upon it in a discussion like the present, which should be based upon scientific data. I will merely remark that this argument will receive additional support, provided that it can be shown that the chancroid is due to a poison generated by simple inflammation, and hence that it must have existed at all times and among all nations.

Bassereau's second argument was based upon the "confrontation" of persons affected with venereal diseases, and he and others were able to prove, in several hundred cases, that when the disease was local in the giver it was also local in the recipient, and that when it was constitutional in the giver it was always constitutional in the recipient; in other words, that the broad line of distinction separating a local disease on the one hand from a constitutional disease on the other, was constant in successive generations without limit.

It will be observed that this proof does not involve any differences real or supposed in venereal ulcers themselves; it may be said to rise above such consideration in that it ascends to the source and origin; and I do not hesitate to say that much of the confusion and contradiction of opinion upon this subject has been due to the fact that observers have confined themselves to investigating symptoms, which, though generally constant, may yet be poorly marked or even wanting, and which often require practised eyes and fingers for their recognition.

Now I maintain that this clinical proof adduced by Bassereau has never been shaken, for, although local ulcers have been produced by the inoculation of matter from syphilitic sores, yet this is susceptible of, and indeed requires, as we shall see hereafter, another explanation than an identity of poisons, and, on the other hand, *there has never been a single authentic case in which syphilis has been produced by the inoculation of chancroidal matter from a person who has had only a chancroid and not syphilis.*

Bassereau does not appear to have speculated on the cause of the difference in venereal ulcers. I do not find in his work the words "Unity or Duality of Syphilis," nor any expression of opinion as to the existence of a specific virus for the local sore. He simply says that he is obliged to recognize a different cause (*une cause différente*) for the local and constitutional diseases.

A school of dualists, however, soon sprang up, who departed from the simple faith of their founder in attaching undue importance to the characteristics of the sores themselves, and who claimed for the local sore a distinct, special virus of its own.

One of the tenets of this school was that the secretion of syphilitic lesions could not be inoculated with success either upon the person bearing them or upon any other person affected with syphilis, and this tenet, in the theory of dualism, was looked upon as vital.

It was not long, however, before it was successfully attacked and overthrown. Clerc, of Paris, Melchior Robert, of Marseilles, and others, succeeded in inoculating the secretion of syphilitic sores upon the bearers, with the result of producing ulcers, without incubation, bearing all the characteristics of the chancroid, and inoculable in successive generations. Mr. Henry Lee, of London, and Köbner and Pick, in Germany, also found that a true chancre would become auto-inoculable, if it was irritated by the application to its surface of powdered savine, or by having a seton passed through its base, so as to render its secretion decidedly purulent. Again, Boeck and Bidenkap, in Christiania, in their later attempts at syphilization, took matter exclusively from true chancres, and obtained the same result as when they had inoculated chancroidal pus. In five cases reported by Bidenkap and Gjör, of Christiania, matter was taken from ulcers obtained in the above manner, and inoculated by patients free from syphilis upon themselves, and in only one instance did any general symptoms ascribable to syphilis follow, and these were of a doubtful character.

Gentlemen, the import of these successful auto-inoculations of syphilitic lesions as regards the question before us, deserves our serious consideration. Let us inquire what they apparently proved, and what they actually did prove. They *apparently* proved the identity of the syphilitic poison with that of the local sore. By their means, it was supposed that the doctrine of duality was demolished, and the advocates of unity were triumphant. Whether this conclusion was not too hasty, we shall presently take occasion to inquire. But these experiments *actually did* prove the absence of any distinct *specific* virus in the chancroid, incapable of generation *de novo;* for here were chancroids artificially produced independently of any descent from chancroids.

You are well aware of the defence adopted by the dualistic school—the mixed chancre, a sore combining both the syphilitic and chancroidal specific poisons—which, it was asserted, would satisfactorily explain all these cases and still leave the tenets of dualism, as at that time understood, intact. This explanation was for a while regarded as satisfactory, but it could no longer be upheld when such experiments had been multiplied indefinitely; when their number was so great that the chances of the commingling of two kinds of specific virus and their simultaneous inoculation was reduced to a homœopathic absurdity; when an indurated syphilitic primary lesion could be taken at random, and, after due irritation, its secretion could be successfully inoculated with the effect of producing pustules and ulcers bearing every characteristic of the chancroid; and when the same result could even be obtained at will by the inoculation of the secretion from a purely secondary lesion, as, for instance, a syphilitic mucous patch! If the chancroid was dependent upon a distinct specific virus, its presence in all these cases was simply impossible, and yet not a single shade of difference could be pointed out between the result produced and that from the most emblematic chan-

croid ever met with in practice. Dualism was indeed henceforth demolished, if by "dualism" be meant that each of the two kinds of venereal sore has a distinct, specific virus of its own. In the face of the experiments referred to, I cannot believe it possible to defend in future any such doctrine of duality.

But the last word had not been spoken in favor of a distinct origin of the chancroid from that of syphilis, nor the last experiment made and recorded which would decide the question before us to-day. Let us examine more carefully those experiments I have just referred to. What was the matter so successfully inoculated? The pure, unmixed virus of syphilis? By no means. It was a compound product, taken, to be sure, from a syphilitic lesion, but a lesion irritated commonly to suppuration by artificial means; containing possibly the germ of syphilis, but containing also, and in fact chiefly composed of, *pus*. Which of these two factors was responsible for the effect produced? The syphilitic virus? In that case this virus should have preserved its power of infecting the constitution, and matter taken from these ulcers, and inoculated upon healthy individuals, should invariably have produced syphilis, which has been shown not to be true. Moreover, if it could be proved that pus alone, free from all suspicion of syphilitic mixture, was capable of producing the same result, then pus was the guilty factor, and there was no such transformation as supposed by the unitists. Such proof we now have as will be seen from the following cases:—

In 1865, Prof. Pick, at the suggestion of Prof. Zeissl, inoculated simple, non-venereal matter of inflammatory origin upon syphilitic subjects. Taking the secretion from pemphigus, acne, scabies, and lupus, he inoculated it upon persons affected with syphilis and produced pustules, not preceded by incubation, and the matter of which was further inoculable through several generations. Counter-inoculations upon the persons free from syphilis who were the bearers of these affections, were without effect. The same result was attained by Kraus and Reder with the pus of scabies, and by Henry Lee with pus from a non-syphilitic child. The late Mr. Morgan, of Dublin, also succeeded in producing pustules and ulcers, identical in appearance with the chancroid and capable of re-inoculation through a number of generations, by inoculating syphilitic women with their vaginal secretions.

It would thus appear that the skin of syphilitic individuals possesses a marked vulnerability, a peculiar aptitude to become inflamed when acted upon by irritants; but this is nothing more than is seen in other non-syphilitic subjects, whose vital powers are impaired by any cause whatever. For instance, it is well known that among medical students engaged in the dissecting-room, it is those who are run down by hard study and overwork, who are most likely to become inoculated by fluids from the dead body. Again, the idea which was entertained by some that there must be a syphilitic soil for such inoculations to succeed upon, is disproved by other experiments which I have to relate.

The earliest of these experiments, as far as I am aware, have never been published, and were performed in the winter of 1867–8 by Dr. Edward Wigglesworth, Jr., of Boston, upon himself, while pursuing his studies at Vienna. He has kindly furnished me with the following history: After stating the grounds which led him to the conclusion—original, it appears, with himself—that "*pus pure and simple might be the cause of the chancroid,*" Dr. W. says:—

"I would state that I was free from all disease either hereditary or acquired; that I had never had a sore of any kind or any local or constitutional lesion of the skin or mucous membranes, and that I was merely a little run down from overwork in the hospital. I took from an acne pustule upon myself, pus, which I inoculated upon myself in three places on the anterior radial aspect of my left forearm at the junction of the middle and upper thirds, first pricking open the apertures of hair follicles and then rubbing the pus into them. The result in the course of three or four days was three well-marked pustules. From each of these I inoculated one new spot upon the same arm nearer the wrist. The result was three new well-marked pustules. From each of the three of the second series I again inoculated fresh spots still nearer the wrist, and again the result was positive. The second series was hardly as well-marked as the first, and the third series was slightly inferior in vigor to the second; still all were well marked, the nine sores being at the same time present upon my arm. On removal of the crusts, perceptible ulceration of the skin was found to exist. Zeissl, with whom I was studying at the time (1867–8), happened to be lecturing upon dualism, and requested me to show my arm to the class to prove the production of ulceration from properly inoculated, simple, healthy pus. There were no buboes in my case, nor did the ulcerations require other treatment than exclusion from the air by means of a simple dressing, and cleanliness. The scars remain to the present day. I thus convinced myself and others—

"I. That the products of inflammatory action, if properly introduced into the human integument, may cause local ulcers, closely resembling chancroids and re-inoculable in generations.

"II. That this pus need not come from a syphilitic person or be inoculated upon a syphilitic person. If taken from, or inoculated upon, a person debilitated by any disease as syphilis, the effect would doubtless be the same though probably greater in intensity."

Many years subsequent to these experiments of Dr. Wigglesworth, which ought to have been made public at a much earlier date, Kaposi[1] published the following statement: "My own experiments have taught me that non-specific pus, such as that from acne and scabies-pustules, when inoculated upon the bearers as well as upon other non-syphilitic persons, will produce pustules whose pus proves to be continuously inoculable in generations; that from these pustules losses of substance occur, which heal with the formation of scar-tissue; and that as the number of pustules produced increases, the inoculability of the pus derived from them diminishes and finally ceases altogether."

It is not necessary for me to dwell upon the exact correspondence of the result of such inoculations and that obtained by the inoculation of so-called chancroidal matter.

The idea that the products of inflammation are the source from which the chancroid springs, and that the simultaneous inoculation of these products and of the germs of syphilis will account for the varying degrees of ulcerative and other phenomena met with in varieties of venereal sores, will strike many of you as novel, and it is easy to foresee the objections which will naturally arise. It will be asked: Can it be possible that the pus from acne, ecthyma, or scabies can give rise to a sore equal in duration and severity to that produced by matter from a typical chancroid? Comparative inoculations upon the same individual with these two agents may even be adduced to show that this is not the case. In replying to such objections, it must be frankly admitted that we do not as yet fully understand all the laws governing the inoculation

[1] Die Syphilis der Haut, etc., p. 47.

of septic matter. We cannot, for instance, fully explain why one individual should be more susceptible than another, why different parts of the integument, as that of the chest, the arms and the thighs, should develop ulcers so varying in their destructive tendency as is shown in the practice of syphilization; why the secretion from purulent urethritis and purulent conjunctivitis should be interchangeable, and yet have no effect upon the mucous membranes of the mouth, nose, or ear; why a chancroid of the prepuce should inoculate other points of that membrane, and yet commonly spare the glans penis; or why one upon the os uteri should allow the walls of the vagina in contact with it to escape; and so with other instances that might be brought forward.

That the effect produced is to a great extent proportionate to the ulcerative action of the source from which the matter is taken, is evident to any one who has performed auto-inoculation from indurated chancres. If the chancre consist of a simple erosion with a watery secretion, seated upon an indurated base, the first two or three, or even more, attempts at auto-inoculation will probably fail; but as the surface of the sore becomes irritated to suppuration by repeated pricks of the lancet, these attempts will succeed, first in producing minute pustules and ulcers, but subsequently, as the suppuration increases, others larger and better developed. Taking these facts into consideration, it need not be wondered at if comparative inoculations upon the same individual with matter from a simple skin affection and from a chancroid of the genitals, should show greater severity in the latter. But without entering further into this subject, I claim it to be sufficient to have shown that the inoculation of the products of inflammation will produce an effect identical *in kind*, even if not in degree, with that of matter from the most typical chancroid; and this is my reply to the objections I have named.

I have now, gentlemen, called your attention to the evidence drawn from artificial inoculation in favor of a duality in the poison of venereal sores, some of them being derived from the virus of syphilis, while others are due to the products of simple inflammation. This view, which I believe to be most consistent with our present knowledge of pathology, and to be supported both by experience and scientific investigation, has of late years found its advocates among medical men.

Thus Bäumler, in his recent able work on syphilis, after quoting experimental inoculations like those just given, says:—"The necessary conclusion is, that the *poison of the soft chancre may, under certain circumstances, be produced de novo without the intervention of the syphilitic virus,* while the syphilitic poison propagates itself only in one continuous series. Hence the chancroidal poison, or whatever in these experiments produced the pustules resembling chancroids, cannot even be compared with the syphilitic poison, to say nothing of regarding them as identical."

In the recent and well-known debate upon syphilis before the Pathological Society of London, that accomplished surgeon, Mr. Hutchinson, came within one short step of the truth when he admitted the origin of the local venereal sore to be "the products of syphilitic inflammation, but not usually containing the germs of syphilis." If he had omitted the adjective, "syphilitic," before the word "inflammation," his expression would have been consistent with the facts at present in our possession, and he would have found it inconsistent with such facts to proclaim

dualism as dead, since, as I have already shown, dualism is nothing more than a duality of poisons in the evolution of venereal sores.

If the view which I advocate be the correct one, it suggests an interesting analogy with the history of our belief as regards the nature of gonorrhœa, an affection which in the last century was regarded as due to the syphilitic virus. Ricord finally adduced convincing proof that it had nothing to do with syphilis. It was afterwards supposed to depend upon a virus of its own, the gonorrheal virus. We now know that it may be caused by any simple irritant, but more especially by the pus from the urethral and other inflamed mucous membranes, whether originating or not in contagion. Such as the history of gonorrhœa has been, so, I predict, the history of the chancroid will be.

In the preceding remarks, I have only casually alluded to the evidence in favor of a duality of poisons to be found in the symptoms presented by venereal sores themselves, and by the lymphatic ganglia in anatomical relation with them. The value of this evidence must always depend upon the observer's knowledge, skill, and experience in venereal diseases. How often do we witness the grossest errors in the diagnosis of venereal ulcers made by men who are deservedly eminent in general practice? Moreover, instances not unfrequently occur in which the symptoms are ill-defined, and in which the most experienced will wait for further developments before expressing an opinion. Hence, so long as the symptoms of the sores themselves were alone considered, the question of unity or duality remained undecided. And yet, to my own mind, the evidence founded on these symptoms is not to be despised, for in the great majority of cases they are sufficient to enable us to distinguish the syphilitic from the local sore, and the obscurity of some cases is readily explicable on the ground of the simultaneous inoculation of the products of inflammation and the germs of syphilis, and the well-known immediate action of the one and the incubation of the other.

One word as to the form of the question with which we started out. It may appear to some that as the products of inflammation are various, the question should read thus: "The virus of venereal sores, its unity or plurality?" But this is a matter of small moment. If any one prefers the latter reading, I have no doubt it will be accepted as an amendment. The actual point at issue is whether the syphilitic virus is, or is not, responsible for all the venereal ulcers met with in practice, and this is sufficiently implied in the question as originally worded.

Gentlemen, I will not longer detain you. I must apologize for the incompleteness and brevity of my remarks, necessitated, however, by the short time at our disposal.

The conclusion which I have to offer you, is based, as I have endeavored to show,

(1) Upon clinical experience, more especially in the confrontation of persons affected with venereal sores.

(2) Upon artificial inoculation.

(3) Upon the corroborative evidence presented by the symptoms of the sores themselves.

For convenience of discussion I shall divide my conclusion into the following propositions:—

I. The virus of venereal sores is dual.

II. Some venereal sores are due to the inoculation of the syphilitic virus.

III. Other venereal sores are due to the inoculation of the products of simple inflammation.

IV. These two poisons may be inoculated simultaneously.

Discussion on Dr. Bumstead's Paper.

After the reading of the preceding paper, Dr. R. W. Taylor, of New York, said:—The Section is to be congratulated upon having presented to it such an exceptionally perfect paper as that read by Dr. Bumstead; it is certainly the best exposition of the subject which I have ever listened to or read in any language. And I am glad to be able to say that my experience coincides with the observations of the distinguished writer. As bearing upon the distinction between the virulence of the pus of a chancroid and that of an initial syphilitic sore, I may refer to a case observed by myself of a man who in the first year of syphilis contracted gonorrhœa, and, having a long, tight prepuce, suffered from herpetic vesicles, which assumed the character of chancroids; buboes formed in the groins, and the pus from these ran down the thighs and produced pustules, which were healed with great difficulty. The man's wife was not then, nor is she at the present time, syphilitic, but she too suffered from chancroids. Another case was that of a newsboy whom I saw at a dispensary in New York; the boy was not in any degree syphilitic, and was believed to have been up to that time virtuous, but was suffering from a bad sore upon his leg; the pus ran down and produced ulcers similar to chancroids; this boy by sexual intercourse subsequently contracted syphilis.

Dr. E. L. Keyes, of New York, said:—Dr. Bumstead's third proposition presents the true question at issue. No doubt pus from simple inflammations may produce ulcers upon persons in a condition to be easily impressed, but such ulcers are not identical with chancroids. Pus is, of course, irritating, and different kinds of pus have different effects; but I do not think it proved that simple pus applied to a healthy person will produce anything identical with chancroid. The paper just read does not describe the ulcers said to have been produced by simple pus, and I do not think that it has been shown that they were typical chancroids. Urethritis is quite common among married persons, and so are excoriations among persons with tight prepuces; some persons even cannot have sexual intercourse without producing herpetic vesicles, but the resulting ulcers are not chancroidal. To say that ulcers from simple pus are identical with chancroids, is going too far. If that were the case, every one would be in imminent peril of becoming sooner or later a victim to this affection. In a majority of cases, the pus of acne will not produce upon a healthy person an ulcer identical with chancroid.

Dr. Bumstead said:—The time allowed for the reading of my paper excluded a description of the sores mentioned in it; and I thought it sufficient to state that Kaposi had been unable to discover any difference between chancroids and the sores thus produced. I have said in the paper that the more healthy the person, the less liable will he be to the evolution of the sores.

Dr. Keyes said:—I would ask Dr. Taylor when the chancroidal ulcers appeared in the case which he has described?

Dr. Taylor said:—I took very full notes of the case, but have them not with me; I can say, however, that the herpetic vesicles appeared about the tenth day after exposure, and that they assumed the chancroidal appearance about the twelfth or thirteenth day.

Dr. D. R. Porter, of Kansas City, said:—I am a believer in dualism, but I cannot believe that a chancroid can be produced from an inflammation not chancroidal; nor do I see how it is possible for herpetic vesicles to turn to chancroidal ulcers. I believe that chancroids are different from syphilis, and that chancroids are always developed before the tenth day.

Dr. Bumstead said:—Chancroids are always developed before the tenth day, and the only way in which the development can be deferred is for the virus to lie upon the surface some days without being taken into the tissues.

Dr. Taylor said:—There was no possibility of that in the case which I have mentioned, for the patient washed the penis in warm water frequently, and the continual wiping of the gonorrhœal discharge would have been sufficient to remove the virus from the surface.

Dr. Keyes said:—It is impossible for me to believe that a chancroid could appear as late as eleven days after exposure.

Dr. L. D. Bulkley, of New York, said:—I do not believe that the weight of evidence sustains the proposition, and I believe that the chancroid has no connection with syphilis.

Dr. Charles Heitzmann, of New York, said:—There are some persons very liable to pustules, and upon whom small lesions will produce them. During many years that was my own condition. I have witnessed many experiments in Vienna, but have seen no evidence of the analogy between the sore produced by simple pus and chancroid.

On motion, the propositions appended to Dr. Bumstead's paper were adopted as expressing the opinion of the Section, with the exception of the third, for which, on motion of Dr. Bulkley, the following was substituted:—"Science has demonstrated that suppurative inflammatory lesions, which resemble in certain features what we know as chancroids, may be produced on various parts of the body by inoculation with simple pus from various lesions."

LEPROSY AS OBSERVED IN THE SANDWICH ISLANDS.

BY

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GOVERNMENT PHYSICIAN.

[COMMUNICATED BY THE SECRETARY.]

THE history of leprosy as it exists on these Islands is yet very obscure. Its advent or origin cannot be traced with any degree of satisfaction. Among the present generation it is known as *mai-pake* (Chinese sickness), they contending that the first case ever known here was in a Chinaman. The older class of natives formerly knew it under the name of *mai-alii* (sickness of chiefs), and describe the disease as it existed then, precisely as we find it here at the present time. Some of the older foreigners, who have resided here for many years, and before the importation of the Chinese, recognize the present disease, or one closely resembling it, as having existed here as far back as the year 1830. If this be true, it is certain not to have been brought here by the Chinese.

The question of importance yet to be solved is, whether it is a disease *sui generis*, or a form of syphilis peculiar to the climate and dependent, to at least some extent, upon the habits and diet of the people.

It is no easy matter to arrive at a positive conclusion, for there is reasonably strong evidence advanced for the support of both theories. Since my residence upon the Islands, I have examined over four hundred cases of the so-called leprosy, and I am as yet unable to determine which of the two positions is the more tenable, or which view rests upon the stronger evidence. One great trouble or difficulty rests in the fact that almost every native either has, or has had, syphilis, acquired or inherited. Of the large number examined, I have found but two in whom there could be any doubt as to the pre-existence of syphilis, and in these two exceptions close relationship could be traced to persons having leprosy.

Two forms or varieties of the disease are observed, viz.:—the anæsthetic and the tubercular. At the present writing, there is sitting in my office a native woman about twenty years of age, married and having two children, one six and the other three years old. She is of medium height, weighs 146 pounds, and, to a casual observer, is a picture of health. Scrutinizing her more closely, we detect two brownish spots, one upon either cheek, which, to an experienced eye, tell only too plainly the terrible fate that awaits her. Her left arm hangs cold and lifeless at her side; this condition, she says, began about one year ago, with a numbness in her fingers, which lasted for a few weeks, since which time there has been an entire loss of sensation from the elbow downwards. In her case there is not only cutaneous anæsthesia, but a needle can be thrust through the skin, superficial fascia, sheath, and muscle itself, without giving rise to the least pain. The extensor and flexor muscles are relaxed, and can only be slightly contracted or extended. The ligaments are very elastic, permitting the fingers to be laid quite back upon the dorsum of the hand. The skin of the affected

arm is dry, harsh, and flabby, all of the fatty matter has disappeared, and the muscles are also atrophied. There is slight anæsthesia along the course of the gastrocnemius muscle of the left leg. This patient has had syphilis. There is no indication as yet of the existence of the disease in her children. She disavows having been associated in any way with a leper.

In the tubercular form of the disease, the earliest indications observed of its approach are tubercles upon the base of the tongue, falling off of the eyebrows, and a thick, corrugated state of the skin upon the forehead and cheeks. The ears become thickened, the helix being enlarged and the lobe at times as large as a hen's egg. Ectropion is often found in the incipient stages. Frequently the existence of these early indications after a few weeks or months is followed by ulceration. The fingers and toes drop off, this condition being accompanied by great distortion. In some cases the bones of the fingers and toes become absorbed, except small portions of the metacarpal and metatarsal bones, upon which the nails remain intact. There is more or less insensibility of the skin, but not so marked as in the other form of the disease. When ulceration of the nasal mucous membrane takes place, the sense of smell is lost, the vomer is absorbed, and considerable deformity of the nose results.

The present superintendent of the leper asylum, W. P. Ragsdale, a half white whose father was a Virginian, is a very sensible, educated man. He says that the first indication of leprosy apparent in his case was an ulcer on the bottom of his foot which could not be healed. He supposed this to be the result of syphilis, and so believed until the contraction of the flexor muscles, and the thickening of the skin on his forehead and cheeks, occurred, when he recognized himself to be a leper.

Another frequent accompaniment of this disease in its tubercular form is keloid tumor; in fact, by some physicians this is considered as pathognomonic of the disease. Cataract is often found in both forms of leprosy. There can be little doubt that the two forms are varieties of one common morbid condition. I have seen but two cases of elephantiasis Arabum (Barbadoes leg), one in a native, the other from the island of Tahiti.

Leprosy does not appear to be more frequent in one sex than in the other, but it is often the case in women that as long as the menstrual function remains perfect, the disease seems to be kept in abeyance. The least disturbance or irregularity of menstruation which occurs, causes a rapid progress in the disease. Conditions of society seem to have little effect, the disease attacking high and low of native race. The disease is confined almost exclusively to the native population. I have observed fifteen cases among Mongolians and four among Europeans, the latter being males of the lower order, living entirely with the natives, eating *poi* and salt fish, and sleeping in overcrowded filthy huts. Each of these white men presented undeniable evidence of the pre-existence of syphilis.

The disease is not confined to any locality, or elevation, but where prostitution is most rife, there the most cases will be found. I believe the disease to be hereditary, and in illustration of this fact I will give the following cases which are well substantiated:—

A native woman on this Island bore five children by a leprous husband; the disease was developed in each of these children, between the ages of eight and twelve years, and they were taken to the Leper Asylum where they have since died. The husband died from the same disease. The woman afterwards married a healthy man, and has now three children past the ages at which the

disease occurred in the other set. They are stout, healthy youngsters, with no indication of the disease in their systems.

A few weeks since the police brought to me a leprons woman, the mother of four children, the eldest eight years of age, the youngest two months. She was far advanced in the disease, her body covered with foul ulcers, with loss of phalanges of one hand, corrugated skin, and ectropion, and she was suckling the infant, which, together with the two next older children, was apparently in good health. The eldest, a boy, bore evident signs of the existence of the disease, having tubercles upon his tongue, ectropion, a bronzed appearance of the skin, with slight thickening. As a rule, leprous women are sterile, but in this case we find the contrary, although she had had the disease, according to her own statement, for thirteen years.

I am strongly inclined to the belief that leprosy is not a disease *sui generis* but an offspring of syphilis, although from New Brunswick, Bermuda, Jamaica, Dominica, Guiana, Cape of Good Hope, and some other countries, come reports to the contrary. I am compelled to believe as I do until stronger evidence is offered by these writers for the support of their theory. Dr. Stephenson, from Barbadoes, says:—I will not say that syphilis can produce true leprosy, but it is more common in the offspring of syphilitic parents. Dr. Hende, in his report from Magpore, in India, ascribes a portion of the cases to syphilis. Drs. Reed and Pollard, from Guiana, deny any connection between leprosy and syphilis, while Dr. Van Holsh, from the same place, says, "I believe leprosy to be connected with syphilis, yea, to be the offspring of it." Dr Trousseau, of Honolulu, says, "I believe that in syphilitic patients there is a predisposition to leprosy." Dr. McRobbin, a member of the Board of Health in Honolulu, says, "I believe leprosy to be an offspring of syphilis." The two last-mentioned gentlemen have had large experience in this disease, and so strong is their belief in its relationship to syphilis that they have stated that the eradication of syphilis from these Islands, would eventually cause the disappearance of leprosy, or, according to the number of cases of syphilis allowed to run its course, through its different stages, in such proportion will exist the number of cases of leprosy. Here again, on account of the widespread existence of syphilis, it is impossible to ascertain the extent of the contagiousness of leprosy, if it be contagious.

In the general acceptation of the word, by indirect contact, none of the medical men on these Islands believe it to be so; as to direct contact, there seems to be some doubt. The woman already referred to, who bore five children by the leprous husband, still lives, and bears no signs or symptoms of the disease. The wife of the present superintendent of the lepers has had three leprous husbands, and now lives, and has lived for many years, at the asylum, and yet bears no evidence of the disease. Père Damien, a Prussian priest, and a most excellent man, has devoted his life, during the last few years, to the welfare of these people, living the same secluded life, ministering to them in sickness and in health, working with and associating with them; yet the blighting curse has passed him by, and left him as yet unscathed.

Many cases might be noted to sustain the view of the non-contagiousness of the disease, yet I am strongly inclined to the belief that it is oftentimes conveyed by direct contact, through sexual intercourse or inoculation in other ways; several instances have been known, on these Islands, of women who have married second and third husbands, after the death of the first from leprosy, their later husbands sharing the same

fate as the first. Whole families, and those intimately connected or associated with them, have thus become diseased.

The natives seem to have no fear of the foul pestilence, and can often be found huddled together, men, women, and children, sick and well, under the same covering; smoking the same pipe which comes poisoned from the secretions of the leper's mouth; eating from the same *calabash*, when the hands and fingers are covered with ulcers. Dr. Manget, writing from Guiana, says: "I have met with only two cases in which, after minute inquiry, I believed the disease to have been communicated by direct contact." Dr. Regnaud, of the Island of Mauritius, says: "I have met with two instances in which the disease seemed to have been transmissible, in one instance from the husband to his wife, and in the other from a man to a child of his wife by a former husband." Drs. Pollard and Van Holsh, of Dutch Guiana, assert positively that it is contagious. From Ceylon, Bombay, Madras, Bengal, Calcutta, Magpore, and Benares, comes the report that it is not contagious.

A few years since, when smallpox was epidemic upon these Islands, vaccination became promiscuous. Shortly afterwards, leprosy was largely on the increase, and by many it was ascribed to this cause. The then Minister of the Interior (a graduate of medicine, too), appointed laymen to travel over the country and "vaccinate everybody." The subjects from whom the virus was taken were natives; it was transferred from arm to arm; and if leprosy is contagious, as this gentleman himself asserted in reports to the Legislature in 1872, the people of this country can, to a great extent, lay at his door the cause of the increase. In 1870, a law, similar to the "social evil law," in St. Louis, was enacted here by the Legislature, and put in full force; Dr. McRobbin, upon whom devolved the duties of Medical Officer, with his usual zeal and earnestness, went to work and accomplished much good; but this law was doomed to the same fate as that in St. Louis, and was repealed in 1872, the same idea of "licensing prostitution," as it was termed, prevailing here as there. That much good resulted, no one unbiassed by prejudice can fail to see, and had the law continued in full force to the present time, its effects would have been plainly visible, without the necessity for statistics to tell us of the good accomplished.

The Leper Segregation, or Asylum, is located upon the Island of Molokai, where all lepers are confined. The asylum comprises about eight square miles. It is a low plain surrounded on three sides by the ocean, which expends its mighty force upon this land, unbroken by reef or shallow water. It is with great difficulty that a landing can be accomplished, and then only by the aid of experienced natives. Upon the remaining side rises the lofty *pali* or precipice of Kalanpapa, 2000 feet high. In the side of this *pali* is cut a narrow path by which the asylum proper is reached. It is a serious undertaking, to go down and return, as it must be done on foot, and a false step might land one more quickly at the bottom than would be agreeable. Having accomplished the descent in a quiet way, we find ourselves upon the flat or landing place, where there are a number of native huts, occupied by lepers; here we mount our horses and ride to the superintendent's house, about one and a half miles away. From this place we have a splendid view of the ocean, the *pali*, and the asylum. It is a grandly beautiful sight; one well worth the fatigue and dangers of the trip. The asylum is composed of 200 houses, in which live the lepers, alone, or with their families and friends. Directly in front of us is the hospital, built of wood,

and in a quadrangular form; in this building are to be found the more advanced cases of those not able to care for themselves; nurses are furnished, and they are well cared and provided for. There are 696 inmates in the whole segregation; their houses are clean and neatly kept; food, both in quantity and quality, all that they can wish. The establishment is well regulated and governed; it is controlled by the Board of Health, and its present condition is greatly due to the energy and zeal of the Hon. S. G. Wilder, President of the Board. The number of cases of leprosy now at large upon the Island is very small, and the disease is evidently upon the decrease; and this decrease is, in my opinion, due to the improvement in the number of cases of syphilis, brought about by the Government and Board of Health, which furnish all natives with medical attendance and medicines free of charge, and in this way induce them to leave their old superstitions and harmful methods of treatment by *Kolumas*, or native doctors. Since 1866, no less than 1570 lepers have been received into the asylum, while 872 have died, leaving, at the present writing, 698 inmates.

In an affection so grave, we can but feel how limited is our knowledge, and how far beyond the resources of our art is this blighting curse. Still, we may be able to do something, not to relieve pain—for the sufferers usually pass through the several stages of the disease with but little pain and suffering—but to stay the progress of the malady, and to afford relief in intercurrent affections to which the debilitated condition of the patients renders them peculiarly liable; as, for instance, pneumonia, hepatitis, erysipelas phlegmonodes, conjunctivitis, etc.

The remedies which I have found most valuable in the incipient stages of leprosy, are more particularly the protiodide of mercury, given in doses of one grain daily, with half a grain of opium, and pushed to slight ptyalism; afterwards the liquor potassii arsenitis, twenty drops daily, continued for some days, when, if circumstances are favorable, the corrugations of the skin disappear, the eyes assume a better color, and a vast improvement in the general appearance of the patient is observed. This improvement may continue for weeks or months, when suddenly, without any apparent cause except perhaps inclemency of weather, the disease returns, and in all probability in an exaggerated form. In the more debilitated cases, the tinctura ferri chloridi is given with benefit. Drs. Donaldson and Boeck have found the internal use of mercury injurious in leprosy, but, given as before stated, I have found no evil results to follow; on the contrary, at least a partial control of the rapid progress of the disease has attended this treatment in my hands.

As an external remedy, I make use of the ointment of the iodide of sulphur, applied after washing thoroughly with warm water and carbolic soap. In some cases, the itching of the entire surface of the body is very severe, so severe, indeed, that the sufferers will, in their agony, tear out pieces of flesh with their nails; in this condition, I find the best application to be carbolic acid in the form of ointment, one part to four, which generally gives relief.

Discussion on Dr. Enders's Paper.

After the reading of the preceding paper, Dr. L. A. Duhring, of Philadelphia, said:—The paper is one of great interest, as it presents the question of a relation between leprosy and syphilis, which has never yet been satisfactorily

answered. My own opinion is that there is no relation between the two diseases. There is much division on the subject among those who reside in countries where leprosy prevails. From the remarks in the paper, and from my own observation, I would say that syphilis exists to a large extent in the Sandwich Islands, and that many cases there classed as leprosy are complications of the two.

The President, Dr. J. C. WHITE, of Boston, said:—The facts presented in the paper are interesting, and many of them are novel to me. I know that syphilis has for many years found a stronghold in the Sandwich Islands, and that leprosy, recently introduced there, has spread most rapidly. The suggestion of Dr. Enders concerning vaccination is valuable, as bearing upon the question of contagion in leprosy.

ON THE TREATMENT OF SEBORRHŒA.

BY

C. HEITZMANN, M.D.,

OF NEW YORK CITY.

SEBORRHŒA, in the definition of Hebra, "consists in a morbid secretion of epidermis, which is impregnated with sebum, and either forms a greasy coating or accumulates in scale-like masses upon a part of the skin which is, in other respects, healthy."[1] This disease, so very common on the scalp, where it bears the popular name of "dandruff," and on the face, especially on the nose, is, as dermatologists know, a very obstinate one, and difficult to cure. It is easy to remove the accumulated, fatty, epidermic masses, by rich applications of fat, by repeated washing with water and soap, and by repeated shower-baths; but a real and permanent cure can rarely be obtained by these means, even in those cases which are in intimate connection with diseases of the womb, or with a general trouble known as chlorosis, after the removal of such a primary or general disease. A short time after the treatment, the fatty scales return, the sebaceous masses gather again on the scalp and on the nose, and the disease runs its course, leading, after many years' standing, to atrophy of the sebaceous glands and the root-sheaths of the hairs, and at last to complete baldness. The latter result is very common with seborrhœa of the scalp, and was known to Hippocrates. Besides, the skin affected by seborrhœa is very liable to eczematous eruptions, especially when continuously scratched on account of an itching sensation commonly produced by an increased perspiration in summer time, or after excitement of the organism. If the disease be present on the face, repeated applications of irritating agencies, especially of the alcoholic solution of green soap, as recommended by some dermatologists, almost inevitably lead to a chronic dermatitis with induration and stiffness of the skin, a condition which gives a singular aspect to persons who have suffered a long time from seborrhœa of the face. If the disease be established on the nose, the chronic inflammation very often leads to enlargement of the bloodvessels, and the disease known as acne rosacea. Persons who have suffered in their youth from seborrhœa of this part, by and by acquire an enlarged, red nose, and in my experience are very liable to deforming eruptions of acne rosacea, the more, of course, if alcoholic drinks be taken, even in moderate quantities.

In the beginning of 1875, I was accidentally led to the local application of a remedy, which, as far as I can say after a relatively short experience, is capable of removing the disease radically. This remedy is tar. In a case of chronic squamous eczema of the scalp, apparently the result of seborrhœa, and complicated with a highly advanced baldness of the part, I applied the tar tincture, and not only cured the eczema but produced a new growth of hair, leading to a complete restoration of the

[1] On Diseases of the Skin, by Ferd. Hebra, M.D.; Edition of the New Sydenham Society of London, vol. i., 1866, page 104.

normal state. After a treatment of about two months' duration, the patient was cured, and remains cured and full-haired up to the present time.

The tar has been applied in seborrhœa of the scalp by Hebra, but only with the intention of removing the almost intolerable itching sensation by which the disease is sometimes accompanied. Furthermore, it has been applied in the form of tar ointment by Piffard, of New York, but only for a couple of weeks, alternately with different other remedies. Among the varieties of tar, I give the preference to the oleum rusci crudum, a product of dry distillation of the white birch. It is preferable to other kinds, as the oil of cade and the oleum fagi, on account of its relatively less offensive smell, this resembling the smell of Russia leather. Genuine ol. rusci is very slightly soluble in fatty oil, but readily soluble in alcohol. In private practice it is almost impossible to apply a fatty oil, as, for instance, the linseed or almond oil, impregnated with ol. rusci to the highest possible degree, because the smell of such a preparation is too strong, and to many persons intolerable. The alcoholic solution of ol. rusci, the so-called tar tincture, again, is rejected by most of the patients, because it blackens the scalp and renders the patient incapable of transacting business with other persons. The form which I have chosen, after different experiments, is that of a pomatum, in which the smell of the oleum rusci is killed by ol. rosæ, the only fragrant oil capable of covering the smell of tar to a satisfactory degree.

The formula which I have employed since March, 1875, is the following:—

℞. Olei rusci crudi, ℥ss;
Ung. aquæ rosæ, ℥iv;
Olei rosæ, gtt. x–xx.—M.

The quantity of rose oil to be added depends somewhat on the wealth of the patient, as it is a very expensive substance. In summer time we may unite melted wax or paraffin with equal parts of cold cream, in order to make the pomatum more consistent. The greatest care should be taken, on the part of the druggist, to mix the preparations named, exactly, and for a long time, inasmuch as the smell of tar is the less noticeable, the more carefully the union has been effected by mechanical means. Every evening, before going to bed, the patient rubs into his scalp, and not into his hair only, a quantity about the size of a small walnut; after that he has to cover his head with a tight, flannel nightcap, in order to press the ointment somewhat into the skin, and to avoid soiling the bed-linen. In summer time, as the wearing of a nightcap is unpleasant, it may suffice to protect the pillow with a suitable cover. Next morning the remains of the pomatum have to be taken off with a dry linen cloth, and then the patient may apply some fragrant pomatum. Twice a week he should take a cold shower-bath of moderate force, and about two feet in height, and as often should simply wash the scalp with water and Castile soap. Nothing further is necessary, and especially no internal treatment whatever is required.

The way in which the tar pomatum acts, is that after fourteen days the falling off of the hair is stopped; in fourteen days more the seborrhœa is gone; and again, fourteen days later, a distinct new growth of hair can be observed. After at least six weeks' treatment, the disease is in many instances cured. Slight relapses may occur, and are readily dealt with by repeated applications of the tar pomatum of much shorter

duration. In two cases of highly advanced baldness, evidently produced by former seborrhœa, and of several years' standing, I succeeded in producing a remarkable new growth of hair by the application of the above-named pomatum for two or three months. In cases of seborrhœa of the face, the continuous application of the tar pomatum, for six or eight weeks, will cure the disease and remove the stiffness of the skin. In cases of seborrhœa of the nose, besides greasing with the pomatum, repeated emptying of the enlarged sebaceous glands by mild squeezing with the finger-nails is enough to cure the disease even in chlorotic girls. As my experience embraces only twenty-eight cases of seborrhœa, though all were treated successfully in the manner described, I do not consider the value of the remedy as being thoroughly established. I bring the method before the profession in order to enable every physician to try such a simple treatment, and shall be glad to learn the results obtained by others.

THE TREATMENT OF SYPHILIS,

WITH

SPECIAL REFERENCE TO THE CONSTITUTIONAL REMEDIES APPROPRIATE TO ITS VARIOUS STAGES, THE DURATION OF THEIR USE, AND THE QUESTION OF THEIR CONTINUOUS OR INTERMITTENT EMPLOYMENT.

BY

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I. Does a mild beginning in syphilis necessarily indicate that the malady will run a mild course, so that the duration of treatment may be regulated thereby?
II. Is the internal use of mercury debilitating?
III. When is mercury useful in syphilis? Has it any control over the late symptoms?
IV. When is iodine useful in syphilis? Can it replace mercury in any stage of the disease?
V. Does iodine act by liberating mercury lying latent in the tissues?
VI. Should treatment be continuous or interrupted?
VII. General outline of a course of internal treatment.
VIII. Conclusions, negative and positive.

I. *Does a mild beginning in syphilis necessarily indicate that the malady will run a mild course so that the duration of treatment may be regulated thereby?*—My subject restricts me to the constitutional remedies appropriate to the different stages of syphilis, and I shall keep as closely within the prescribed limits as the nature of the question will allow. It is necessary, however, to answer this first proposition before taking up the general consideration of constitutional treatment.

I need not cite authorities to prove that syphilis, commencing mildly, is often looked upon as a trivial, self-limited disease. Such a belief prevails in the minds of many physicians to-day. It has received the sanction of high authority. Diday,[1] of Lyons, has taught it boldly for many years, and Lancereaux in the main supports his views. Diday divides syphilitic patients into two classes. Of these, one class, embracing about two-thirds of the whole number, says Diday,[2] contains cases beginning mildly. The prognosis in these cases is declared to be good; the patients are treated mainly by expectation; and mercury is considered unnecessary and even harmful. Cases commencing severely, the other class, get mercury; but, so the teaching goes, they only get it as an evil less bad than the severe syphilis which it is used to counteract, and the obvious conclusion is that the remedy must be employed during only a

[1] Histoire naturelle de la Syphilis; Paris, 1863.
[2] Diday et Doyon, Thérapeutique des Maladies vénériennes et des Maladies cutanées; Paris, 1876, p. 254.

short time to overcome the symptoms, and must not be continued to the injury of the patient. Lancereaux[1] is particularly strong on this point, and I think that a similar conviction is widespread among the profession in this country, the general notion being that mercury is bad, and only to be used because syphilis is worse. And even if this were not so, and if the profession should become a unit in regarding any given case of syphilis as liable to prove serious in the end, yet it would still be impossible to subject all cases commencing lightly to prolonged treatment. The ordinary patient can neither convince himself, nor be convinced—after his first alarm is over, and his trivial early eruption past—that he is likely to suffer severely from his disease at some indefinite, future period, and in the conviction that he will not so suffer, his physician is very apt to encourage him.

It is certainly true that most cases of syphilis commencing mildly continue to preserve their mild type whether treated or not, and are not so apt to lead to serious later troubles as where the earlier lesions are bad, the chancre, perhaps, phagedenic, and the first eruption pustular. It is natural that this should be so. Phagedæna is a personal peculiarity, and not a syphilitic symptom, and it is fair to assume that the quality of constitution which allows phagedæna to occur in a given chancre, will also cause the symptoms which follow it to be proportionally severe. It is the same with a tendency to form pus on light provocation, either in the elimination of an internal poison, or from external cause; this pyogenic tendency is a personal matter and by no means a symptom of syphilis, and where it shows itself in the early outbreaks of the disease by a pustular or even by a vesicular eruption, it is only natural that the course of the future symptoms should prove severe.

That these qualities of phagedæna, pustulation, etc., are personal, and not due to the acquisition of any especially bad type of disease, I think is easily demonstrable. I have seen more than once two patients whose syphilis had been derived from the same source, run a widely different course in their symptoms. The fact seems to me to be that an individual shows himself up physically when he is prostrated by the first onset of syphilis. A light beginning in the disease seems to indicate that the patient has power to rally and make a good fight. The physician, therefore, lulls himself into a belief that the patient can manage his disease without much aid from medicine, and, as the remedy, mercury, is regarded as an evil, he concludes that the patient is best off with as little of it as possible.

This reasoning would be unimpeachable were the premises accurate, but they are not so. First, mercury is not an evil. It is not debilitating, but tonic, in health as well as in disease, when continuously used in minute doses. This I think can be proved, and, if so, it becomes obvious that even if only a small percentage of the mild cases of syphilis become severe after a lapse of years, it is justifiable to submit all cases to such a continuous treatment as, itself tonic and beneficial to the patient, shall prove capable of keeping down symptoms during its use and for a considerable period afterward. Second, no one can tell how many cases commencing mildly may become severe in the end. That very many do

[1] Traité de la Syphilis; Paris, 1874, p. 548. "Un certain nombre de spécialistes, etc. . . . se croient obligés d'administrer le mercure pendant plusieurs mois, quelquefois même pendent des années; . . . . loin de s'opposer à la marche ultérieure de la maladie, ils conduisent le malade à l'anémie ou à l'obésité."

change in type is certain. The journals abound in such cases, and the experience of those who are accustomed to deal with the disease is always rich in incidents bearing on this point. I need not go beyond my own note-book to illustrate this.

CASE I.[1]—A gentleman had chancre and a faint eruption for which he took mercury in considerable doses during a few months. After ten years of health, he had syphilitic iritis, and a gummy destructive ulcer on the penis which resisted eight weeks of energetic treatment (caustics, etc.) at the hands of a surgeon, and cicatrized in three weeks under steadily increasing doses of the iodide of potassium.

CASE II.[2]—A medical student had chancre and indurated inguinal glands which were unmistakably syphilitic. He underwent treatment by small doses of mercury at my hands. His type of disease was so mild that he had no eruption whatever, nor any sign or symptom of disease except one indolent, indurated, post-cervical ganglion. Therefore he concluded that I was in error, and stopped his treatment entirely after a few months. In eighteen months I treated him for syphilitic hemiplegia. He had had no symptom meanwhile.

CASE III.—A physician got an insignificant sore, followed by a slight papular rash. He took no mercury, but was treated by cathartics. The rash went away, and he remained perfectly well for nine years. Then a lump appeared upon the top of his head, and remained stationary during eleven years. But now the patient had a fever, and, on getting up from it, the node spread, softened, and swept away an enormous piece of his skullcap. The wound healed kindly under the use of iodide of potassium.

CASE IV.[3]—A gentleman had urethral chancre. No eruption followed that he could remember. He certainly took no mercury, and remained well for twelve years, when he broke out with syphilitic ecthyma which disappeared after the use of arsenic for a time. Then his brain gave out, and was declared to be softening by one of the most noted experts in nervous disease in Europe. Finally this got better spontaneously, and a rapidly advancing, gummy, destructive ulceration invaded his soft palate. This got well immediately, with great improvement in his other symptoms, under the use of iodide of potassium alone.

CASE V.—A physician got chancre. A few mucous patches in the mouth followed, but, he stated, no eruption whatever. He took mercury for a year. Not a symptom appeared for thirty years. Then he died during a sharp attack of intelligential and convulsive troubles which he would not treat, not believing them to be syphilitic. The autopsy revealed a large gummy tumor in his brain.

CASE VI.—A gentleman got chancre followed by a light eruption. This was treated homœopathically and disappeared. Sixteen years afterwards he applied for treatment, being almost covered with nodes. He had ulcers in his throat and syphilitic lung disease. Steady improvement attended the mixed treatment.

CASE VII.—A gentleman got chancre and fell into the hands of an anti-mercurialist who dosed him freely and continuously for a year with iodide of potassium and sarsaparilla. His earlier symptoms were not severe. In one year ulcers appeared on both legs. The dose of the iodide was increased. The ulcers progressed in depth and area, three on the right leg measuring over one inch in diameter each. He now changed his physician. His new adviser discontinued the iodide of potassium and ordered protiodide of mercury. Improvement began at once. In two weeks the ulcers were cicatrized. He now ceased treatment, but, after two years, a gummy ulcer destroyed a portion of the septum of the nose; for this he consulted me.

[1] Van Buren and Keyes, Genito-Urinary Diseases, etc.; New York, 1874, p. 537.
[2] Ibid., p. 650.
[3] Van Buren and Keyes, Clinical Contribution, etc., Archives of Dermatology, vol. i. p. 108. Cases III. IV. V. and VI. were seen by me in connection with Dr. Van Buren.

These cases are not very exceptional. The list could be largely increased, but I restrict myself to these few cases because they sustain the point at issue from almost every side, and because some of them bear also upon other facts which will be used in subsequent portions of this paper. It will be seen that all these cases had a very light syphilitic attack in the beginning, and that they were all managed differently. Case I. had a light eruption, and took mercury in considerable doses for a few months. Case II. had no early eruption, and took a very little mercury for a very short time. Case III. took no mercury, but was treated by cathartics. Case IV. took no mercury, but was treated by balsams, because his chancre was urethral, and his malady mistaken for gonorrhœa. Case V. took mercury for one year, and remained well for thirty. Case VI. was treated homœopathically. Case VII. was treated for one year with iodide of potasium. I have had no patient who had been syphilized, but the seven cases cover nearly all the other methods of treatment: that by expectation; by cathartics; by diuretics (balsam, etc.); by mercury for a short time in large doses, for a short time in small doses, for a year in fair doses; by homœopathy; by iodide of potassium from the first—and all of these, starting mildly, turned out to be severe cases of syphilis.

It is indeed notorious how many bad cases of disease there are in which there has been very little early eruptive trouble, and in which the previous history of the patient shows treatment only during a few months. This treatment for a few months seems to be what nearly all cases get, and although very many of them, be they light or severe at first, escape serious subsequent trouble, yet there is no guarantee in the prevailing modes of treatment that they will so escape, *nor can a light beginning be counted upon to signify a type of disease necessarily mild.*

I have not thought it necessary to search among the array of corroborative cases which modern journals afford for material to further substantiate this point, preferring to adhere to personal cases, for the history of which I can vouch. I cannot, however, refrain from alluding to a recent article by Fournier,[1] a record of forty-seven cases of cerebral syphilis, and itself a strong argument against the too readily accepted maxim that mild syphilis needs but little treatment. Of the forty-seven cases only two were severe from the first, one was rather bad, thirty were ordinary cases, and fourteen "actually benign." One of these latter was the case of a medical student who got what is termed "un chancre de rien" (a mere trifle of a chancre), had a little papular rash and sore throat only, and took mercury for a few months. After seventeen years of calm, there came an explosion of syphilitic epilepsy, hemiplegia, hebetude, dementia, and death in spite of all the skill and care of Fournier and Ricord. My conclusions to this introductory section therefore are: (1) that all syphilis is severe and treacherous, and that further efforts are justifiable in attempting to perfect its treatment; and (2) that seemingly light cases need as thorough a course of medication as those cases which are apparently more severe.

II. *Is the internal use of mercury necessarily debilitating?*—Time would fail me should I attempt to go into this question exhaustively, yet upon its solution hangs the strongest possible argument for or against a con-

[1] La Syphilis cérébrale, est elle plus commune dans certaines formes de la diathése, etc., L'École de Méd., Aug. 30, 1875.

tinuous use of mercury. If mercury is an evil, if its influence is debilitating, then its use would only be justified by its ability to overcome some greater evil, some more debilitating agent; and it should be dispensed with in every case as quickly as possible. This indeed is the accepted view of the mercurial question in syphilis. I think, however, that it may be shown: (1) That mercury in minute doses is a tonic, and, (2) That mercury in minute doses has been in use in the profession as a tonic, in non-syphilitic cases, for very many years, and sanctioned by the highest authority.

(1) That mercury in minute doses increases the number of the red blood cells both in syphilitic and in healthy individuals, I think I have demonstrated with the hématimètre, publishing my results in the January (1876) number of the American Journal of the Medical Sciences. To that paper I must refer for details. Space will not allow me to reproduce the evidence here. I have, however, corroborated the conclusions therein reached by repeated, new experiments, during the present year, and these conclusions I beg leave to adduce in evidence now. They are based upon the counting of red blood-corpuscles in over one hundred different specimens of blood, which number has been greatly added to since the publication of the paper without occasioning any modification in the results at first arrived at. These conclusions are: (*a*) That 5,000,000 red corpuscles in the cubic millimetre of blood is a full, high average for the adult, healthy male, etc. (in New York); (*b*) That mercury decreases the number of the red cells when given in excess, especially in hospitals (this was also conclusively proved by Wilbouchewitch[1]); (*c*) That syphilis diminishes the number of red corpuscles below the healthy standard; (*d*) That mercury in small doses, continued for a short or for a long period, in syphilis, alone or with the iodide of potassium, increases the number of red corpuscles in the blood, and maintains a high standard of the same; (*e*) That mercury in small doses acts as a tonic upon healthy animals, increasing their weight. (Liegeois and Bennett cited.) In larger doses it is debilitating or fatal; (*f*) That mercury in small doses is a tonic[2] to individuals in fair health, not syphilitic. In such individuals it increases the number of the red blood-corpuscles.

I must again express regret that I cannot bring out the evidence here, which led directly up to these conclusions in such a way that I can see no possible escape from them, and, referring those interested to the original paper, I again ask to incorporate these conclusions as a part of my present argument. This proof of the tonic action of minute doses of mercury is scientific, but the demonstration from the *clinical side* is no less positive.

(2) We are all familiar with the well-known tonic consisting of a minute dose of corrosive sublimate in compound tincture of bark. I first learned it from the late Valentine Mott, who was loud in its praises as an "alterative" in the cases of strumous children. I cannot trace the authorship of this compound. It is very favorably mentioned as having "been long recommended both for scrofulous taint and the more declared forms of tuberculosis," in Copland's Dictionary of Practical Medicine, 1858;[3] and I find it spoken of as a preparation in common use, in a

[1] De l'influence des Préparations mercurielles sur la Richesse du Sang en Globules rouges et en Globules blancs. Archives de Physiologie, 4 et 5, 1876, p. 508.

[2] During as long a period as the experiments were kept up. Keyes, loc. cit.

[3] Vol. iii. p. 760.

lecture by Sir Astley Cooper, which appeared in the Lancet in 1823.[1] He praises it as an alterative "for the removal of chronic diseases in children." Modern ideas of "alterative action" are not very clear, and in connection with this mixture I never understood how mercury could be beneficial, until the truth flashed upon me, while counting blood corpuscles with the hématimètre, that mercury in minute doses was tonic in all cases in which it could be digested. I think that this tonic action of mercury in small doses must have had much to do with establishing the professional confidence in former days in so-called alterative[2] medication.

This conviction of the value of minute doses of mercury as an "alterative" (*i.e.*, tonic), not in syphilis but in all chronic disease, I find to be most deeply rooted in the minds of the older members of our profession throughout the country. I have received many letters, from far and near, from representative men, stating that they had inherited the faith from their preceptors, and had been for years in the habit of using minute doses of mercury, long-continued, as an alterative in chronic debilitating disease; that they had used it empirically, but with good effect. I have received similar answers from all the older men whom I have questioned. What can be stronger evidence than this that mercury in minute doses is tonic, increasing the vitality of the individual; and when to this is added the scientific proof, by actual counting, that mercury increases the number of the red blood-corpuscles, the chain of evidence seems complete. It may be well here to mention a remedy on which many of the profession in New York were accustomed to rely some years ago with confidence for the cure (or certainly the relief) of Bright's disease. It was two grains of corrosive sublimate in an ounce of tincture of the sesquichloride of iron. The dose was ten drops (not minims), containing about one-fiftieth of a grain of bichloride of mercury, a very fair tonic dose; and that improvement attended its use in many cases, is unquestionable. Finally may be cited the popular sanction of minute doses of mercury as a tonic which is found in the history of Swaim's Panacea,[3] a nostrum at one time in very general use. This remedy consisted of a minute quantity of corrosive sublimate in compound syrup of sarsaparilla. Swaim is believed to have gotten the idea from an old book, his first business having been that of a book binder.

While then all the evidence goes to show that minute doses of mercury are tonic, out of syphilis as well as in it, the proof is equally strong that mercury in large doses is debilitating, if at all continued, whether in syphilis or out of it. This is proved by history, by clinical experience, and by actual blood counting with the hématimètre, as has been shown by Wilbouchewitch and by myself, in the articles already referred to.

[1] Vol. i. p. 111.

[2] J. Kent Spender, of London, refers to the action of corrosive sublimate (Journal of Cutaneous Medicine, No. 15, 1870, p. 187), saying that it indirectly promotes the nutrition of several tissue elements, and speaks of Dr. Billing's mention of its action as tonic. In a letter to me, Dr. Spender says that Billing (First Principles of Medicine, 5th ed. 1849, p. 109) states that the "tonic" action of mercury illustrates the rationale of the operation empirically called "alterative." He does not enlarge or explain further.

[3] Dr. E. R. Squibb, of Brooklyn, informs me that this nostrum, appearing about the year 1825, was extensively advertised, and enjoyed unbounded popularity, bringing great wealth to its inventor. In 1828, Prof. Hare, of the University of Pennsylvania, discovered mercury in the mixture. He published the fact in the Amer. Journ. of Med. Sciences, vol. iv., 1829, p. 530, whereupon Swaim denied that the "Panacea" contained mercury. In fact mercury does not *now* seem to be one of the ingredients. Mr. Neergaerd analyzed a specimen of Panacea for me, but failed to find any mercury.

III.—*When is mercury useful in syphilis? Has it any control over the late symptoms?*—If it be once admitted that mercury is useful during any stage of syphilis, the proof that it is a tonic theoretically establishes its value during all stages of the disease; for the hématimètre on the one hand, and clinical observation on the other, prove conclusively that syphilis is essentially a debilitating malady, and that it lessens the number of the red blood-cells. Yet mercury is not alone tonic, it is antidotal to syphilis; not only generally to syphilitic symptoms, but specifically when locally applied, as Koebner[1] has shown, particularly for condylomata, referring also to Ch. Hunter and Hebra; and as Monti[2] has demonstrated by causing the local disappearance of patches of eruption by injecting the bichloride subcutaneously into them, while other patches at a distance remained comparatively unaltered. In a general syphilitic eruption, the advantage of combining local with general means is strikingly shown by the much more prompt disappearance of lesions which have been subjected to the local use of mercury, than of the same lesions elsewhere seated and not locally treated. This I have repeatedly noticed.

Every one admits the power of mercury in postponing and modifying the earlier symptoms of syphilis, and of shortening their course when they do appear. Even the anti-mercurialists allow this, their claim being either that mercury prolongs the attack on the whole by postponing the symptoms, since the disease must run its course—or that it suppresses the virus and causes the appearance of tertiary lesions by debilitating the constitution, or in some other way. No statistical or other evidence with which I am familiar has ever established either of these assumptions,[3] while the almost universal advocacy of mercury in syphilis, in one form or another, by a majority of the best minds in the profession in all countries, goes far to show that this is the only honest remedy for syphilis with which the profession is familiar. As for the two points of the anti-mercurialists—that mercury used early only postpones the symptoms, prolonging the total duration of the disease, and that tertiary symptoms are due to mercury—if time allowed, any number of cases might be cited in opposition to this view; such as Cases III. and IV. of this paper, in which, no mercury having been used, the disease was in full force after twenty-one and fifteen years respectively, in the most advanced tertiary type; or Case VII., in which mercury cured

[1] Archiv f. Derm. u. Syph., B. I., S. 628; quoted from Jahresber. d. schles. Gesellsch. f. vaterländ. Cultur.

[2] Jahrbuch. f. Kinderheilkunde, 1869, S. 381. Beobachtung über die Behandlung der Syphilis congenita et acquisita mittelst subcutaner sublimatinjectionen.

[3] Undoubtedly statistics exist on both sides of the question, but they fail to prove anything positive. Quite recently Jullien (Gaz. Hebd., Sept. 18 and Oct. 2, 1874), in a statistical paper attempting to establish the proportion of cases of tertiary disease occurring in those who had taken mercury for their earlier symptoms, as compared with those who had been otherwise treated, found in 218 cases of tertiary disease that 59, more than one quarter, had not taken mercury, while all the rest had taken it. The number of cases of nervous syphilis he noted to be much greater proportionally in those who had taken mercury. All this, however, proves nothing except that syphilis naturally runs on to tertiary symptoms when untreated. Statistics cleverly handled are capable of proving almost anything in relation to syphilis, because the disease itself is so capricious. In this case the statistics were made up from letters received from physicians in France, England, and Italy, and undoubtedly in these countries three-quarters of the well known physicians give mercury early in syphilis, and it would be but fair that three-quarters of the cases of tertiary syphilis found should turn out to have taken this remedy. This is especially probable when the statistics are compiled by one who is no lover of the drug, and his figures lose all value when it is remembered that there is no record of the cases which did not become tertiary under the mercurial and anti-mercurial courses.

tertiary ulcers, which failed to improve under fair doses of iodide of potassium, in a patient who had been treated by the latter remedy alone for a year after his chancre appeared.

That mercury retains its value late in syphilis is amply illustrated by the well-recognized power of the mercurial, general fumigation, in many advanced forms of syphilitic ulceration, late in the disease; by the great value of what is known as the mixed treatment, especially in controlling peculiarly chronic and grave symptoms of late syphilis; and by the numerous cases of nervous syphilis found in the journals of the day, in which the turn in the symptoms is noted as the point at which the gums became touched. Occasionally a case is found in which the customary forms of iodine cannot be borne late in syphilis, or in which they do not seem to meet the requirements of the disease. Under these circumstances an appeal to mercury alone will often yield a victory over symptoms until then rebellious. Case VII. of this paper is in point, mercury having succeeded where iodine had failed. While then it seems not doubtful that mercury retains its power in every stage of syphilis, yet it is no less true that it cannot be relied upon in emergencies in certain, especially the gummy, forms of tertiary disease. Here the different preparations of iodine take the lead.

As a final illustration of the power of mercury over tertiary lesions may be cited its undoubted value in inherited syphilis. Here the lesions are grouped in a disorderly manner—tertiary, perhaps gummatous, disease of the bones, lungs, kidney, thymus gland and liver, often accompanying secondary symptoms on the skin and mucous membranes—yet in these cases mercury is generally relied upon, and often proves capable alone of routing the enemy. I conclude, therefore, that mercury is of value during the whole treatment of syphilis, in minute doses throughout, for its tonic and anti-syphilitic action; in larger doses, as the occasion arises, to suppress outcropping symptoms, the action here being purely anti-syphilitic, and not tonic.

IV. *When is iodine useful in syphilis? Can it replace mercury in any stage of the disease?*—In studying this point, I have again had recourse to the hématimètre to watch the effect upon the red blood-corpuscles of the internal administration of the preparations of iodine. My conclusion, based upon a considerable number of blood-counts made by myself, and many by my friend Dr. L. A. Stimson, is that iodide of potassium is a tonic, and increases the number of the red blood-cells. It does this in all stages of syphilis. I have not yet tried its effect upon the healthy individual, but notably in some experiments by Dr. Stimson this point came out, that while iodide of potassium, given immediately after the appearance of a chancre, increased the red blood-cells with proportionally greater rapidity than did mercury given under similar conditions (for the effect of mercury during the first depression preceding the outcrop of the earliest eruption is often very slight), yet, in spite of this greater increase in the number of the red blood-cells, the administration of iodide of potassium did not retard the appearance of the secondary symptoms to the same extent as did the use of mercury. This fact corresponds to the well-known results of the experiments of Grassi, who determined that iodide of potassium increased, while mercury decreased, the proportion of the red blood-cells to the whole mass of blood when used in syphilis. In Grassi's day, mercury, when used at all, was

always given in excess, and his conclusions, although obtained by chemical methods mainly, are undoubtedly accurate.

These statements of Grassi have been made use of to construct a rule of practice; namely, to give iodides and tonics early in syphilis, avoiding the supposed depressing influence of mercury. But when it appears that the iodides do not seriously interfere with the outcrop of the early eruptions, while mercury certainly does; and when it turns out that mercury in small doses (but large enough to greatly modify the disease) is tonic, the rule of practice founded on Grassi's experiments fails to find any scientific support, and must fall to the ground. Case VII. in this paper illustrates this point. This patient was dosed with iodide of potassium for a year, but his early eruptions appeared none the less, although they were undoubtedly mild, and his tertiary symptoms came on in due time, perhaps as early as they would have come had no treatment been used. Iodine doubtless has some modifying influence over syphilis during its whole course, but not enough to make it wise to substitute this drug for its more powerful rival early in the disease.

I see no reason to believe that iodine has any eliminative power in syphilis, and I have failed to discover any evidence to show that it tends to prevent relapse. That it holds a place, however, in the treatment of syphilis, is undoubted; and a very high place it is. When tissue-infiltrations take place; when the lesion approaches the gummatous type; when there is considerable connective-tissue proliferation, especially if it takes place rapidly, whether the affection is of an external or of an internal organ; when nodes form upon the bones; when the cartilages begin to soften; when ulceration is spreading rapidly, then the preparations of iodine used unsparingly for the symptom are the right hand of treatment. Here nothing can replace iodine. To its benign influence, when well assimilated and rapidly pushed, the most formidable lesions yield a quick response.

In certain of the chronic lesions occurring late in syphilis, the power of mercury is greatly enhanced by combining some preparation of iodine with it; but where the lesion is a gumma, mercury may be dispensed with at once, and entirely, and iodine pushed with a lavish hand until the symptom yields. Mercury, however, must be resumed to seal the cure and prevent relapse, and it must be given with long patience and scrupulous care that it is thoroughly assimilated. In syphilis, therefore, when the lesion is gummatous, no matter where it is situated, iodine may replace mercury with profit to the patient; but under no other circumstances can it take the place of this drug, although it may often be advantageously combined with it. Iodine is indeed what many have considered mercury to be, a remedy not at all curative of syphilis, but causing the disappearance of certain of its symptoms. A curative agent it is not, because it has little if any power of shortening the duration of the disease, of keeping off or modifying many of its symptoms, or of preventing relapse.

V. *Does iodine act by liberating mercury lying latent in the tissues?*—I think that this question may be briefly disposed of in the negative. That iodine taken at the same time renders a given quantity of mercury more active, is probable, this being perhaps the secret of the success of the mixed treatment for chronic obstinate lesions, not gummatous. But whatever power as an anti-syphilitic remedy iodine possesses, it probably exercises by its merit as a tonic, increasing the number of red cells in

the blood; for this it does even early in the disease, in spite of the depressing influence of the virus.

When a patient has been carrying a certain amount of mercury without showing it in the mouth, the sudden administration of iodide of potassium may call forth symptoms of salivation. Indeed, iodides given alone, in some exceptional cases, have produced quite marked symptoms of mild salivation. But a little positive evidence outweighs any amount of negative testimony, and whatever power iodine may have in rendering mercury active, still, in its own sphere, in controlling gummy exudation in any of its forms, iodine does not yield a hair's breadth to any remedy, or demand the least assistance from mercury. An excellent case bearing on this point was printed editorially in the Gazette des Hôpitaux for January 28, 1860.[1] Cases III. and IV. of this paper are directly in point, iodine alone having cured patients who had never taken mercury. Case VII. might be referred to as evidence on the other side; but, as I have already stated, some forms of tertiary disease, notably some of the ulcers, seem to demand mercury for their treatment, and yield to it much more promptly than to iodine. In this case, also, I could not ascertain to what extent the iodide had been pushed, and at best the evidence is negative, while the other is positive.

The late Prof. Boeck, of Christiania, informed me, while in this country, that, when tertiary symptoms occurred in his patients who had been treated solely by syphilization, he was in the habit of using iodide of potassium with good effect, combined with a continuance of syphilization. In these cases surely there was no antecedent mercury, the liberation of which could do the work. And finally, in proof that the iodine possesses the power, it is only necessary to recall that, in those frightful cases of devastating disease which we sometimes meet with, and which yield to the iodides, the dose must not be restricted, but pushed with rapidity up to the point of tolerance, the amount of the dose assimilated being in direct relation to the rapidity of cure.

VI. *Should treatment be continuous or interrupted?*—The answer to this question has already been brought out in what has been written. The continuous use of iodine need hardly be considered, for I feel unable to include this drug among the curative agents of syphilis. It has enormous power in overcoming certain symptoms, but as these symptoms are intermittent, the use of iodine should also be interrupted. While the symptom lasts, and for a certain time afterwards, iodine is called for; but as it does not prevent relapse, or shorten the whole course of the disease, there is no occasion for its prolonged use. Mercury, on the other hand, has received the palm as the anti-syphilitic, at least the symptomatic, anti-syphilitic, remedy, *par excellence*, from a majority of the best medical scholars and authorities up to the present time. It is an antidote to the poison, whatever the nature of that poison may be. I think that the results of blood-counting by the hématimètre, used upon the blood of syphilitic patients who had taken small doses of mercury continuously for long periods (over three years),[2] proves that mercury is not debilitating, but, indeed, a tonic, when used in minute doses.

[1] Although the writer states that he thinks iodine often does not act well unless mercury has been previously used.

[2] Keyes, loc. cit.

Clinical experience also has convinced me of the value of the continued treatment. I have employed it in all cases, light or severe, in which I could control the patient, since the early spring of 1869. Generally I have not commenced treatment until the post-cervical and epitrochlear, glandular engorgement, and an eruption, have come to confirm the diagnosis. My observation has included nearly all the cases of my partner, Professor Van Buren, and I have to thank him for his kindly suggestions which have led to many of the steps in this investigation, an investigation which has convinced us both more than ever of the value of the uninterrupted use of mercury in small doses in the treatment of syphilis. It has become a rule with us, to which we find but few exceptions, that patients, after acquiring syphilis, remain in as good and often in better health while under treatment than previously. Severe cases are the rarest of exceptions, and tertiary symptoms (in those treated from the start) nearly unknown. Slight localized lesions, such as a scaly spot on the palm, a cluster of a few papules here or there, appearing from time to time, are, customarily, all that mark a continuance of the disease, except the lesions of the throat and mouth. Slight ulcers about the throat, mucous and scaly patches in the mouth and on the tongue, are apt to persist and recur at intervals, for long periods. These are the main, very often the only, symptoms requiring attention after the first eruption has passed, and they respond to local measures, or to a slight increase in the strength of the general treatment.

Usually the continuous treatment is kept up for from two and a half to three and a half years, or even longer, and in all cases, if possible, until six months, and sometimes a year or more, after the appearance of the last syphilitic symptom. Many of the patients so treated are now married and raising healthy families—themselves to all appearances perfectly well.

Of course it is impossible to state how many of these patients are absolutely exempt from any further relapse. Seven years and a half are not sufficient to pass final judgment on any method, but against other methods may be urged: (1) That the numerous bad cases constantly met with have all been treated by them; (2) That salivation is certainly harmful; (3) That mercury, not pushed to salivation, but used in large doses, has been proved to be debilitating by the hématimètre (Wilbouchewitch, Keyes), by clinical experience, and by experiments upon animals (Liegeois, Bennett, Wilbouchewitch, and others); (4) That the intermitting treatment is simply symptomatic, and virtually makes one confess that he has no remedy for the disease, but can only moderate the symptoms until such time as they may see fit to cease to reappear.

For the continued treatment with minute doses of mercury it may be urged, on the other hand: (1) That it has given good results thus far (seven and a half years); (2) That it is scientifically and theoretically correct to use continuously for a given evil an antidote which can itself be proved to be beneficial to the individual, be he sick or well; (3) That it seems proper to use an antidote continuously against a poison which is continuous, although having long periods of latency. For the poison must be continuous and still present in the tertiary stage, although it has then lost much of its virulence and power of transmissibility. How else can we account for the undoubted fact that parents with tertiary disease may produce children with inherited syphilis (I have notes of such cases), and inherited syphilis is well known to be capable of transmitting itself in full force to the nurse and other healthy persons by contact; (4)

That this continued treatment is only a modification of the "coup sur coup" plan, so widely practised, for the quantity of mercury is augmented upon any outcrop of symptoms, the minute, continuous dose being maintained in the intervals. It is, indeed, not very widely different from the common, symptomatic, interrupted treatment, yet I cannot but believe that it is a great improvement upon both of the ordinary plans.

VII. *General outline of a course of treatment in a typical case.*—When the first eruption appears, or sooner if by confrontation or other sure means the existence of syphilis is placed beyond any possible doubt, my habit has been to put the patient upon a mercurial, and to increase the dose, slowly if there is time, more rapidly if the eruption is pressing, paying all possible attention to hygiene, food, surroundings, care of teeth, etc., until slight diarrhœa with colicky pain is complained of, or until the gums just begin to be touched. The amount required to produce this effect is now known to be the patient's dose, beyond which he cannot safely go without aid from opiates, or changing the form of administering the drug, and at which, if it is maintained, the effect on the general health will be injurious. Having found this dose, it may be maintained, if the eruption demands it, by giving opium, bismuth, rice, milk, etc., until the symptom yields; then it should be reduced to one half, or a little below this point (this dose will prove tonic to the patient), and there maintained day after day, continuously, year in and year out, waiting for other symptoms.

Should such symptoms occur, for there may be none except mucous patches, which local means will relieve, I think it is well to give the patient mercurial baths,[1] when practicable, two or three times a week or oftener if they do not prove depressing, maintaining the same internal dose. Instead of this, the half dose held in reserve may be immediately or gradually added to the tonic dose, or inunction may be practised, until the symptoms are subdued, when the tonic dose is to be again resumed and unremittingly continued.

Later in the disease, should any of those symptoms supervene which are known to be favorably influenced by iodine, the iodide of potassium or sodium may be combined with the mercurial (preferably with the biniodide of mercury), and the two continued (mixed treatment).

Should cerebral symptoms come on; or bones, or nerves, or muscles, or viscera, or any of the tissues become involved in any of the forms of gummatous growth (this, however, I have not yet seen in any case where the directions have been strictly followed from the first); then the iodides are to be used unsparingly for the symptom, it being of little importance whether the mild mercurial is continued for the time being or not.

Many deviations from this simple course may be necessitated by varying cases, especially when the continuous treatment is only commenced late in the disease. I think that a case treated from the first symptom should receive mercury continuously, in small doses, for a period not less than two and a half years, or, in any event, until at least six months have passed after the entire disappearance of the last clearly syphilitic symptoms. It is hardly necessary to add that all treatment may be sus-

[1] Two drachms of black oxide after a few minutes of steam (not hot air), the whole bath to last not over twenty minutes, is a fair average.

pended without ultimate disadvantage should any acute disease accidentally occur within the limits of this time.

My time has expired, and it would be hopeless for me to attempt to take up a consideration of exceptional cases, either of patients such as those who cannot take mercury or iodine, or of peculiar forms of disease, or surrounding circumstances, where these remedies seem powerless, and where an appeal to change of air, or to derivation by the bowels (Zittman's decoction), or derivation by the skin (syphilization or tartarization), seems to effect more than can be done by following the rules established by success in the majority of cases.

I cannot consider the value of mercury locally, as a means of treatment of the lesions, or of other local remedies; nor the relative value of iodine used as the tincture (internally), or as the iodide of potassium, of sodium, of ammonium, of calcium, or of amylum, or as metallic iodine with albumen, or as iodoform; nor of the varied use of these agents by the stomach or rectum. My remarks have been general, and intended to apply to the typical case, and I have endeavored to group them so as to lead up to the following negative and positive conclusions, which I now beg respectfully to lay before you, and on which I ask the expression of your opinion.

Negative Conclusions.—Views for which there would seem to be no positive foundation in fact:—

I. Syphilis commencing mildly needs but little treatment, and does not require mercury.
II. Mercury given internally is necessarily debilitating.
III. Mercury is only useful in secondary syphilis.
IV. Iodide of potassium is of considerable value in secondary syphilis.
V. Iodide of potassium is of no value unless preceded by the use of mercury.
VI. Iodide of potassium acts by liberating mercury which has been lying latent.

Positive Conclusions.—Propositions which, in the present state of our knowledge, may be affirmed:—

I. Mercury is an antidote to the syphilitic poison, and of service in controlling all its symptoms in all (even the latest) stages of the disease, its power over gummata being least marked, and not to be relied upon.
II. Mercury in minute doses is a tonic.
III. Iodine cures certain symptoms of syphilis, but does not prevent relapse.
IV. The use of mercury, long-continued, uninterruptedly as far as practicable, in small doses, from the time of the earliest eruption, constitutes the best treatment of syphilis.

Discussion on Dr. Keyes's Paper.

After the reading of the preceding paper, Dr. L. D. Bulkley, of New York, said:—Dr. Keyes's first positive proposition requires qualification, for mercury is very injurious in gummatous cases. An exception should also be made for certain rare cases. I had a patient who had intercourse on the fourth of July; a chancre appeared in due course of time, and the man came to me for treat-

ment in the following September. I observed this case constantly for a year and a half, and it developed all the symptoms possible during that period. Yet that man could not from the first take mercury without positive injury.

Dr. Keyes said:—I myself know of a worse case, in which the patient lost both eyes from the disease, but could not take mercury in any form. The proposition, however, does not profess to cover every idiosyncrasy.

Dr. R. W. Taylor, of New York, said:—The paper gives distinctly and clearly what many books leave in doubt, namely, the time of beginning mercurial treatment. I think that there are many good reasons for using mercury before the secondary lesions begin; in cases in which the induration of the chancre is marked, it will melt away under the influence of mercury. I have, however, seen cases in late stages in which too much reliance had been placed upon mercury. For the late lesions of the skin, the iodide of potassium will be found very useful.

Dr. Keyes said:—I recall two cases in which the chancre appeared upon the lip; in these, having no doubt that the chancres were syphilitic, I gave mercury at once and with good effect.

Dr. Bulkley said:—If the diagnosis is made at first, there is no reason why mercury should not be at once administered. If I had the slightest suspicion that a sore was syphilitic, I should feel it my duty to give it immediately.

Dr. Taylor said:—In any case of doubt, however, the future of the patient is not imperilled by waiting for the further development of the disease. I am fully convinced of the tonic qualities of mercury, and have known it to be used with better effect than iron or quinia in cases in which the latter are the common remedies. Pains at night, which refused to yield to morphia, I have known to be allayed by an eighth of a grain of mercury daily; mercury is also very efficient in controlling syphilitic fever.

Dr. William Oldright, of Toronto, said:—I would ask Dr. Keyes to specify the particular symptoms for which he would give iodide of potassium, and to say whether iodide of potassium is better than other forms of iodine?

Dr. Keyes said:—In lesions of the joints, nerves and viscera, tubercular skin affections, etc., it is well to give the iodide. Its action is so ready and so plain when given in large doses in such cases, that in a week or ten days the physician can tell whether he is upon the right track, and, if he finds that he is not, can fall back upon mercury. Other iodides may be used, but I give the preference to the iodide of potassium because it is the most easily assimilated.

Dr. Bulkley said:—Mauriac has presented a series of cases of bone lesions in which the iodide failed and mercury had to be resumed; I myself, too, have seen such a case.

Dr. Keyes said:—I have not seen many cases of bone lesion early in the disease, but in those which I have seen I have not interrupted the mercurial course, though I have given the iodide in connection with it. I know that there are some tertiary lesions which do not yield to the iodide, but the rule is that it is of benefit to them.

Dr. Taylor said:—Where troubles of the fibrous tissues and bone lesions come on early, I have always used the iodide; they will not yield as well to mercury alone as to it and the iodide combined.

Dr. Charles Heitzmann, of New York, said:—I would ask Dr. Keyes whether, having said that the iodide of potassium will not prevent a relapse, he means to assert that mercury will prevent a relapse in syphilis?

Dr. Keyes said:—I would not make such an assertion absolutely. I can only speak from my own experience, and I am well satisfied with the course of my patients under the mercurial treatment. Mercury has caused them to have better health than they enjoyed before they contracted syphilis. I cannot say that they will not have other symptoms, but I am convinced that the continuous treatment with mercury is the most scientific, the most philosophical, the most harmonious, and in fact the best.

Dr. HEITZMANN said:—I have employed Dr. Keyes's method of treatment, and have found it unable to prevent a relapse.

Dr. TAYLOR said:—Generally, after two or three years of mercurial treatment, the patient is practically cured as a syphilitic, and it seems to me useless to debate about the unimportant disorders that may subsequently occur.

Dr. HEITZMANN said:—I have seen, in Vienna, cases of mild beginning, which have got well without any decided treatment, and others that have required the most careful management. I believe that syphilis can be cured, but whether the cure should be attributed to the skill of the physician or to other causes, no one can say.

Dr. BULKLEY said:—Instead of giving mercury every day of every month, while the treatment lasts, I generally keep my patients under constant treatment for from three to six months, then let them drop one week a month, for three months, then two weeks a month, and sometimes let them alone for a month at a time; thus they do not take so much mercury, but keep the system always under its influence.

Dr. TAYLOR said:—I would ask Dr. Keyes whether, after uninterrupted treatment for two years and a half, he has not found a decrease of blood-corpuscles, and been forced to cease treatment, and after a while resume with increased doses?

Dr. KEYES said:—The reverse of this is the result of experiments. One-twelfth or one-eighth of a grain of the bichloride, three times a day, is a large dose; one-thirty-fifth to one-fiftieth of a grain is a tonic dose. Wilbonchewitch gave large doses, and kept his patients housed in close hospitals, and of course the number of their red corpuscles decreased. I had a patient who after continuous treatment for two and a half or three years had six millions of red corpuscles to the cubic millimetre, the best blood-count I have ever known. This patient has never had any trouble since, and has become the father of a perfectly healthy child.

Dr. TAYLOR said:—By constantly taking mercury, the patient becomes tolerant of it, and therefore, as Fournier has said, the treatment should be stopped for a time in order to allow the system to recover and become more susceptible to its action when again administered. Moreover, as syphilis does not show itself by long and continuous evolutions, it is consistent to treat it intermittingly; both before and since reading Fournier's paper, I have used the intermittent treatment with excellent results.

Dr. KEYES said:—Tolerance is not established when mercury is given in tonic doses. The intermittent treatment is nothing more than symptomatic treatment, leaving the patient in the intervals to take care of himself; my plan is to keep the patient in good condition by giving him the tonic dose until the symptoms appear, and then to give him the anti-syphilitic dose.

Dr. L. A. DUHRING, of Philadelphia, said:—The only way of arriving at a satisfactory conclusion as to the best manner of administering the drug, is by observing series of cases treated by both methods. In my practice I use both methods, making my selection with reference to the patient himself and the peculiar circumstances of his case. I see advantages in both methods. I think that the plan suggested by Dr. Taylor is practically the best, and that that suggested by Dr. Keyes is often impracticable.

On motion, the propositions appended to Dr. Keyes's paper were adopted as expressing the opinion of the Section.

THE TREATMENT OF SYPHILIS.

BY

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[COMMUNICATED BY THE SECRETARY.]

IT is, in my opinion, a most fortunate circumstance that the members of the International Medical Congress at Philadelphia have determined to discuss the treatment of Syphilis, because it cannot be said that any clear body of doctrine has as yet been laid down as to how this should be carried out, whilst there never has been at any time in the history of medicine so good an opportunity for coming to a correct conclusion on the matter.

Syphilis is now well known to have been quite a new disease in Europe, somewhere about the end of the fifteenth century. As to whether it had existed formerly in America and Japan, it seems to me that our historical works do not enable us to come to any clear conclusion. At its first outbreak, syphilis was so perfectly an unknown disease that the regular physicians confessed that they did not know how it should be treated, and it was handed over to a host of illiterate persons who, as usual, treated the disease so badly that at the end of the fifteenth century it was, it seems, a terrible and revolting spectacle.

Mercury was employed, probably in the form of inunction, in the treatment of syphilis, as early as 1497; but Paracelsus was the first to recommend its administration internally, about the year 1570. At first, mercurial inunction was confined by physicians of repute to cases of true syphilis, before the confusion-period when gonorrhœa and soft sores were put down as belonging to syphilitic inflammation; but, gradually, this method of cure was applied also in cases where no true syphilis was present; and, as salivation was caused, much misery and disease were occasioned by the use of the drug. It was chiefly from this reason that the period of the Peninsular War was such an important one in the history of the treatment of the disease. In 1812, whilst that war was going on, Dr. William Fergusson wrote a letter on the treatment of syphilis, which was read at a meeting of the Royal Medical and Chirurgical Society of London. In this letter, he showed that the natives of Spain, although entirely untreated by mercury, generally made good recoveries from their venereal ulcers, whilst the British troops had their sores in very many instances terribly aggravated by the careless use of mercurial inunction. This letter of Dr. William Fergusson was followed by a number of important experiments made by Rose, Guthrie, Hennen, John Thompson, of Edinburgh (the teacher of James Syme), Desruelles, and especially Fricke, of Hamburg, all of these observers having treated both soft and hard sores without mercury. On the whole, so great was the advantage which accrued in their experience from the disuse of salivation in gonorrhœa and soft sore, that most of these

authors were strong anti-mercurialists, for a long period of their practice, at least.

About the year 1838, M. Ricord entered on his office as *chef de clinique* of the Hôpital du Midi, of Paris; and from that time we may date most of the accurate ideas we possess about venereal diseases. He made it abundantly clear that gonorrhœa was not a syphilitic symptom, and, hence, it was no longer treated with mercury. He next withdrew soft sores from mercurial treatment, and thus confined his attention to the mercurial treatment of true syphilis. For many years, M. Ricord treated the hard sore and its sequelæ by a six months' course of iodide of mercury, in small doses; of late he has prolonged the time of treatment to about a year. The great surgeon of Edinburgh, Mr. James Syme, and his distinguished pupil, Professor Hughes Bennett, most vigorously protested against the long mercurial courses advised by Ricord and his followers, and the Edinburgh School has of late years been, on this account, on the whole, anti-mercurialist in its tendencies.

The author of this essay, in a pamphlet published in 1863, adopted anti-mercurialistic views, and treated from 1860 to 1870, his syphilitic patients entirely without mercury, whether for the hard sore, or for secondary or tertiary lesions. The result of this treatment went to show that many cases of true syphilis were very mild indeed, the whole disease consisting in such cases in a few discrete eruptions, with occasionally slight alopecia, and sore-throat, coming after indurated sore with enlarged and indurated inguinal glands. Tertiary symptoms, however, pretty frequently supervened after non-mercurial treatment of the early symptoms of the disease, in the form of gummy tumor of the testicle, sloughing sore-throat, or ozæna, with destruction of the small bones of the nose. Hence, the lesson taught by abandoning mercury for a time was that syphilis was not unfrequently a mild disease; but that it also was not uncommonly a grave affair.

At the present juncture, it would seem to me, from the evidence given in 1867 by the members of the Société de Chirurgie, of Paris—Diday, Verneuil, Lefort, etc.—as well as by that most learned of all writers on syphilis, M. Alfred Fournier, of Paris, taken in conjunction with the evidence of Henry Lee, Hutchinson, De Méric, and Berkeley Hill, in London, that mercury, when administered very carefully, and in small enough doses, so as never to excite salivation in the slightest degree, and for a sufficient length of time, has (what has been styled perhaps rather sanguinely) some *antidotal* power in extinguishing the virus of syphilis. Mr. Hutchinson, who is fond of hypothesis in this matter, speaks of mercury destroying the *germs* of syphilis, and affirms that if mercury be given in the period of the initial lesion, it will often nip the disease so completely in the bud that not a trace of secondary syphilis may appear. Hutchinson gives very small doses of mercury and chalk, not more, as I understand him, than half a grain, twice daily; but he has never clearly indicated for how long a time he keeps up this treatment in the primary and secondary periods of the disease. M. Alfred Fournier is the most strenuous of all modern defenders of mercury in the treatment of syphilis. In his lectures, delivered at the Lourcine Hospital, of Paris, he boldly alleges that, without doubt, of all causes for the appearance of tertiary lesions, not one is so potent as the negative one—omission of mercurial courses from the early treatment of this disease. He is very careful not to advise short courses of mercury, or to lay down hesitating rules as to the length of time it should be given. He is no believer in the

possibility of diagnosing mild from severe syphilis, as Diday thought he could do. All hard sores and secondary eruptions are, accordingly, treated by Fournier for the first two years of the existence of syphilis by means of interrupted courses of the iodide of mercury, so as to administer that medicine for some ten months in the course of the two years. His dose is from five to ten centigrammes of the green iodide of mercury daily, *i. e.*, from about three-quarters of a grain to a grain and a half of the iodide in the twenty-four hours.

Although I am not entirely persuaded of the complete truth either of Hutchinson's germ-theory and the antidotal powers of mercury, or of Alfred Fournier's theory of prophylaxis against tertiary symptoms, I have been so far influenced by the views of these able syphilographers, that I have, of late years, given rather lengthened courses of the green iodide of mercury, or of mercury and chalk, to all my syphilitic patients in the early periods of their disease. Commencing with two-thirds of a grain in twenty-four hours, accompanied by two grains of extract of hyoscyamus, in the form of a pill, I found myself constrained to lessen the dose to one-third of a grain of the green iodide, as a usual formula. Even with this small dose, salivation not unfrequently occurs, if the patient be not carefully watched. This result of this treatment has not hitherto been in any way discouraging. No accident of gravity has ever occurred with these doses, and the patients seem to pass through the roseolar and eruptive period (as well as through the iritis, which occasionally ensues even in the midst of a mercurial course), in a very favorable manner. I cannot, as yet, pronounce distinctly whether these prolonged courses have warded off tertiary symptoms, but trust that, by this apparently innocuous treatment, I may have in some cases prevented the appearance of that dreaded stage of the disease.

It must not be supposed that Fournier has carried all his Parisian medical brethren with him when he proposes this long-continued mercurial treatment of syphilis. I found, indeed, M. Lancereaux, in a conversation which I had with him in the month of May last, 1876, much opposed to Fournier's idea that long mercurial courses were capable of preventing the occurrence of tertiaries. It will be remembered, by those who are familiar with the literature of syphilis, that Diday, in his work on the "Natural History of Syphilis," published in Paris in 1863, was of opinion that no particular cause for the appearance of tertiaries had been shown to exist. M. Lancereaux shares, it seems, this conviction with Dolbeau and Perrin, as declared in their speeches at the Société de Chirurgie in 1867, and is of opinion that no mercury need be given either for the hard sore or the secondary rash, because these may disappear of themselves. He would reserve mercurial treatment for the tertiary period of the disease. M. Després never administers mercury at any period of syphilis, but trusts to iodide of potassium as a remedy for tertiary symptoms. M. Hardy, in common with other physicians, gives mercury conjoined with iodide of potassium in the tertiary period; and the school of Aix-la-Chapelle, in Prussia, seems to prescribe the inunction cure in all periods of the disease.

Before leaving the question of the treatment of the early periods ot syphilis, I cannot refrain from saying one word as to the confusion which has recently been occasioned by the writings of the late Dr. Morgan, of Dublin, and of Mr. Jonathan Hutchinson, as to the nature of the soft sore, and the tendency of these doctrines to cause a revival of the treatment of that lesion by mercury. I beg to protest with all my

powers against any but local treatment of soft chancre. It is no more requisite to administer mercury in that disease, in order to ward off syphilis, than it is to give it in gonorrhœa. Mr. Hutchinson asserts that *mixed* sores are very common, and seems to hint that it might often be safer to administer the antidote to syphilis, on the chance that the soft sore might be followed by the hard one; but such practice is, in my opinion, a step backwards to the doctrines which were dominant when Ricord and Bassereau made their great discoveries.

Supposing that we have been unable to prevent our patients from arriving at the tertiary stage, or that they have come to us already affected with some gummy deposit, whether in the skin, the mucous membranes of the throat, or the viscera, what should then be our treatment? For my part I have no difficulty in saying that the great remedy for the gummy period of syphilis is the iodide of potassium; and that it is often most dangerous to delay in administering this drug. Mercury is often, I am persuaded, entirely useless in the case of tertiary sore-throat, sarcocele, tertiary epilepsy, or specific lung disease; whereas large doses of the drug so marvellously brought into use by Wallace, of Dublin, in 1834, will often effect a rapid and surprising cure.

The rule in practice ought to be that sloughing sore-throat, or ozæna, ought *at once* to be treated by ten-grain doses of iodide of potassium, administered three or four times daily. So, too, with ulcers of the lower extremity and the face, and with phthisis syphilitica.

It seems to me, then, that the indications for mercury in the period of gummy products are few; and that it should only be used when the iodide of potassium has been tried and found wanting. Above all, I think we should never give mercury alone in the tertiary period, as seems to be the fashion at Aix-la-Chapelle. By administering mercury without iodide of potassium, in the tertiary period, we run the risk of seriously compromising the health of the patient, for, whilst inert mercurial treatment is being pursued, the soft or hard palate may be perforated or destroyed. If, then, the practitioner be inclined to give mercury in the period of gummy products, he should at all events combine it with the iodide of potassium. It must be borne in mind that iodide of potassium occasionally, though very rarely, may produce symptoms of acute poisoning. I have sometimes seen eruptions of a formidable nature, vesicles filled with sero-sanguinolent fluid, caused by small doses of that great remedy for syphilis; and once saw a case which apparently proved fatal. In such cases, the disease may be mistaken for syphilis, and more iodide of potassium administered.

Summing up the views contained in the previous pages, I would say at this moment that:—

I. Mercury, in very small doses, should be given in prolonged courses during the first eighteen months of syphilis, leaving off occasionally, and most carefully watching the patient's teeth, etc. This should be done with the view of preventing the occurrence of the tertiary period. Mercury should be given in the period of the hard sore, with the hope of arresting the progress of the disease towards the eruptive stage.

II. When tertiary symptoms appear, they should at once be treated by large doses of the iodide of potassium, combined, in very rare cases, with small doses of the bichloride of mercury. In general, the iodide of potassium is all that is needed to cure tertiary lesions, at any rate temporarily.

MEASURES ADOPTED IN DENMARK TO PREVENT THE SPREAD OF VENEREAL DISEASE.

BY

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There existed in early times, in Denmark, a sort of control with respect to loose women, but it seems to have been with a view of rendering them more easily distinguishable from other women, partly by ordering them to wear a certain dress, partly by assigning for them the places they were permitted to live in. Thus, for instance, in the year 1496, King Hans published over all the kingdom, edicts, ordering that common women and harlots should wear, as head-dress, a cap, half red and half black, no stuff better than "Deventyrsk," and no better linen than what might be had at eighteen Skilling an ell.[1]

In a law given by Christian the Second,[2] we find among other things:—

> We will that common loose women living in towns shall not be permitted to live in streets and lanes with other good and honest people, but there shall be assigned to them particular quarters of the towns which they are to inhabit; and We will that they must and shall wear good clothes, etc., such as they can afford and procure; they shall not, however, be permitted to wear mantuas, that they may be distinguished from other, modest women and honest people; and We will that they shall not be molested by blows and wounds. Whoever does such a thing shall have his neck forfeited. If anybody should lose his money, clothes, or other things, by rioting in the houses of such loose women, this shall not be judged by any court. But by this is not understood such women as are on terms only with one fellow. They may live in places convenient for them, such as they please to choose in the town, not, however, in places where there is most and greatest commercial business.

These legal provisions indicate that, in those times, a sort of control existed with respect to public women, and are besides a proof (particularly the latter of them) of a high degree of humanity in promising these women protection against violence and molestation.

In process of time, another way was chosen, and, influenced by the fear of venereal diseases, an attempt was made to counteract the propagation of the same by persecuting women suffering from those diseases. This may be seen, for instance, from a writ issued by Christian the Third in the year 1546, from Aarhus, to all superintendents of the whole kingdom of Denmark,[3] in the following terms:—

> Our love to all. May it be known to all that We have learned from the directors of the common hospitals of the kingdom, that in these hospitals are often admitted women who formerly, when young and healthy, have lived in

[1] J. C. Wendt, History of the Beginning and Progress of Venereal Disease in Denmark, 1820.

[2] Laws given by Christian II., published by Resenius.

[3] J. C. Wendt, op. cit.

notorious profligacy, and led an indecent life, and have thereby been spoiled by *pocks*, *French disease*, and other complaints. We therefore beg and request that you instruct and give orders to all parish priests, in the towns as well as in the country, throughout the whole diocese, that they ever and again, from the pulpit, do proclaim to the common and lower classes of the people, that it has been resolved, that wherever there may be found such loose and spoiled women who, in consequence of their wicked and lewd life, have been spoiled by *pocks* and *French disease*, or other such foul and leprous disease, they shall not, after this day, be admitted to or received at any hospital, there to be supported and nourished, so that everybody may know this, and avoid and abstain from such bad and loose life.

Since that time, several legal provisions and edicts have been published, by which the propagation of venereal diseases has been partly prevented, as far as could be done, by attempts to restrict profligacy. Thus:—

Any male who is found to come to a whore-house to commit fornication, is to be punished; the first time with imprisonment during a week, the second time double the length of time. Females being in such houses, in order to give themselves up to lechery, are to be whipped at the whipping-post, or confined in a house of correction. (Danish Law, 6–13–20.)

Pimps and bawds seducing any man's wife or daughter to live in profligacy and lead a wicked life, are to be whipped at the whipping-post, and driven out of the town by the hangman. (Danish Law, 6–22–1.)

They who keep whore-houses are to be punished by being whipped at the whipping-post, and exiled from the province where they have been living, or confined in the house of correction. (Danish Law, 6–22–5.)

The police-master shall diligently investigate that nothing is publicly done against modesty and decency, and particularly that no whore-houses are kept; and shall do his best that those who keep them, as well as those who frequent them, may be seized and summoned before the police court; and that the brothel-keeper, as well as those who have been found in such houses in order to give themselves up to lewdness, may, after being convicted, be sentenced and punished according to law. (Police Regulations, October 22, 1701. Post II., Chapter 3, § 1.)

They who keep public houses in Copenhagen are forbidden to keep more female servants than are really wanted, or to permit loose women to lodge in their houses, if they will not be sentenced and punished for pimping. (Town-hall placard, July 28, 1828.)

If females are found to stay in houses of bad repute, to stroll about the streets by night, or to have subsisted by idleness, without being able to account for their having a substance sufficient to permit them to live in idleness without hurting anybody; or if such persons, though they have been hale and hearty, have not for a year had any regular service, then they ought to be, the first time, summoned before the police office, there to receive a warning, not only to keep away from such ill-famed houses, but even to take regular service, lawful labour, or have a legal profession; and if, after this, they continue their former life and conduct, he (the police-master) shall sentence and punish them, after the laws, as vagrant persons of no use, but rather hurtful to the public, and send them to work in the house of correction. (Instructions for the Police-master of Copenhagen, March 24, 1741, § 9.)

Besides this, the edict of November 27, 1775, contains several special provisions to prevent lewdness from being practised by women kept by inn-keepers and others for that purpose, and also many directions with regard to the behavior to be observed by such persons with respect to their female servants, and also for the behavior of women out of work with regard to the lower dancing houses, all to the same purpose; and finally, § 9 contains the following:—

Loose and idle women who have their lodgings in separate apartments and rooms, ought, at the request of the police-master, to account for their manner of living; and they are to be punished if complaints are brought forth with respect to the seducing young men, or the like. As to women who trespass against this, the police-master ought, according to circumstances and the peculiarity of the case, and without appeal, to sentence them to pay a fine from two to ten Rbd., or to be punished by detainment, with bread and water, and exposure at the pillory, or to be punished by imprisonment in the rasping-house or "Börnehus," or other house of correction, from one to three months.

Towards the end of the last century a most important provision was issued for preventing the propagation of venereal disease, viz.:—

A Royal Rescript of March 14, 1788, *to the Civil Governor and Bishop of the Diocese of Aarhus, of the following contents:—*

It has been reported to the King that the venereal disease has for a number of years been prevailing in the diocese, and that the chief cause of its continuing is supposed to be that the infected, partly from shame and partly in order to avoid the costs of the cure, conceal the complaint, or apply to quacks, until they suffer most severely from the disease, and spread the contagion. In order to remedy this, it is decreed by these presents that all belonging to the lower classes infected with venereal disease, whether wealthy or poor, shall be favoured with completely free cure, and have the attendance of the surgeon of the district to which they belong; and the surgeon shall have for his pains, from each such patient, a payment of two Rdl.; and all sick persons who, according to trustworthy information to be relied upon, are not rich enough to pay for their being attended while sick, must be treated gratis; and all the expenses of medicine, the doctor's fee and his conveyance, and the support of the poor, are to be paid proportionally after the standard of the land-tax; and the King also wills that the patients from different parishes shall, if possible, be assembled in one town, in order to be cured in the most convenient and shortest way; and that persons of the lower classes, who do not in due time request to be cured, shall, after having been cured, be punished with imprisonment with bread and water, or other arbitrary punishment dictated by the chief magistrate according to circumstances. In the mean time the bishops should duly admonish the clergymen to tell the names of the sick or suspected who are known or may become known to them.

By royal rescript of July 2, 1790, the above rescript was extended to be a rule for the whole kingdom of Denmark. By letter from the ministry of home affairs of December 21, 1863, these rescripts have been interpreted in the most liberal manner possible. This letter was issued in consequence of an inquiry from the Direction of the Infirmary at Aarhus, whether they were obliged to receive venereal patients arriving from other districts, and whether the city of Aarhus was obliged to pay the costs of their stay at the hospital, or whether the costs should be paid by the district where those persons had their home. The jurisdiction of the district admitted that the hospital was in duty bound to receive any syphilitic patient who should ask for admittance, but was of opinion that the costs in such cases, where the persons concerned had their homes in other districts, and had only come or been sent to Aarhus to be received in an hospital to be treated for and cured of syphilis, should be paid by the districts or towns where the persons in question had their homesteads; whereas the costs ought to be paid by the city of Aarhus, if the patient had resided there even for a short time. The ministry agreed with what the district had said, but decided that the costs for cure and support at the hospital should be paid by the district or town where the patient had

his homestead, only when the patient had come to Aarhus in consequence of a summons of the competent authority, in order to be cured there; but not if it were in consequence of his own resolution, or the summons of private people, in which case the general rule was to be applied, that the patient's homestead must pay the cure.

Although prostitution has been acknowledged as a matter of fact, it has not been thought proper, as it has been termed, to legalize it by legal provisions; but it has been tolerated. This tolerance has been looked upon as legal authority in consequence of the police-placard of March 9, 1809, according to which, women belonging to the jurisdiction of Copenhagen were summoned, if suffering from venereal disease, to apply to the police within a fortnight. Persons conforming to this summons were promised free cure and support, and their names kept secret with regard to persons not concerned, and they were free from being accused and punished, even if their conduct had been such that, conformably to the police regulations, they might have been accused of lewdness and idleness; whereas they who did not apply were threatened to be acted against with the whole severity of law. It was in order to avoid, as much as possible, everything that might give to prostitution a stamp of legality, that no formal concessions were given either to public women or to their keepers, and that neither of the parties was to pay for the tolerance shown. The position of public women was not regulated by laws and regulations, but they were under the discretionary authority of the police.

As early as the commencement of this century, *visitation* of public women was introduced at Copenhagen. Between the years 1800 and 1807, the police-master Haagen engaged a physician in order to inspect the prisoners, and to examine public women. By royal rescript of June 6, 1815, which formerly could not be published, the control in Copenhagen was very much improved. This rescript runs as follows:—

To the Police-Director of Copenhagen: We have received a memorial in which has been most humbly proposed to us such means as might be employed against the spreading of syphilitic contagion in our royal residence of Copenhagen, and after having duly considered the matter, we by these presents notify you that, concerning this, we most graciously have established the following stipulations:—

(1) You are authorized to enjoin all who may be looked upon as belonging to the class of trading people, going under the general name of inn-keepers, under pain of punishment according to the law of November 27, 1775, to give up to you their names in full, and the houses they are living in, and when they are about removing, and then at the same time what and how many female servants or lodging females they have at their houses, when and whence they have come, and when they leave their service, and where they are then going; all this for the purpose of procuring for the police an exact knowledge whether any such inn-keeper, under cover of the trade for which he has a license, may intend keeping a whore-house.

(2) You are likewise authorized to demand from loose and idle women who, according to the placard quoted, § 9, ought to account for their mode of living, to let you know their lodgings, and any change of the same, on pain of the above punishment.

(3) You must strictly take care that no women, against decency, try to allure males by indecent gestures or words, in doors or windows, or in the street; and you must sentence women who thus trangress, to punishment according to the placard of November 27, 1775.

(4) To women suspected by the police, and living alone, and to girls living

in public houses that have attracted the suspicion of the police as favoring lewd life, you are to have distributed instructions how to know the venereal disease, which have been printed by the care of our Chancery, and you must induce them to have always these instructions in their houses.

(5) You must order all women named under the foregoing (4) to have themselves visited at their own houses by their own physicians (which physicians ought to be named to you and acknowledged to be skilful), or by the police physician, in places which you must choose; you also must charge them to procure a report from the physician, and this report ought to be delivered at the police office the same day that it has been procured.

(6) You must superintend that the reports of the physicians are punctually delivered, and after that you must have them arranged in such tabular form that they may be easily inspected in order to discover immediately if anybody has neglected to have herself visited, or to deliver her physician's report.

(7) You must give such directions that every one neglecting to deliver such report, shall immediately be visited by the police-physician.

(8) Besides this visitation, which is to take place EVERY MONTH, you must, after deliberation with the town-physician, and at not fixed terms, have such women visited by the police-physician, for the purpose of having a continual control that can be relied on.

(9) As by rescript of this day to the direction of the poor-house of Copenhagen, the same has been charged to have women who are found to be infected, taken to an hospital to be cured, and also to grant to those who ask for it free cure and support there, you are not permitted to allow any woman who is found to be infected, to have herself cured at her house, unless you be assured that this may be done without exposing the public to any danger.

(10) Whenever a woman who is living alone or in a suspected public house, and who, after having received from you the above-mentioned instruction how to know the venereal disease, is by investigation of the police found to be infected to such a degree that she cannot excuse herself by pretending ignorance of the existence of that disease, and has not then applied to be cured, and has in the mean time neglected the visitation ordered by law, she shall not be allowed free cure and support as otherwise given to such patients, but she herself, if she is living alone, or otherwise the inn-keeper at whose house she has been living, shall be ordered to pay the costs of her cure, and in want of payment the costs are to be worked off by public labor.

(11) If any woman or man has been found to have infected a person of the other sex, and the person thus accused is convicted of having known her or himself to be suffering from the disease, and to have nevertheless ventured on incautious intercourse with the other sex, the same is to be punished by the police, with fines or corporal punishment according to circumstances.

By a writ from the Government-office of August 20, 1822, there is repeated what the police-physician has formerly been charged with, and among other things the visitation of public women; but at the commencement of 1841, there had not yet been introduced any definite mode of visitation. When the same was to take place, about once a month (formerly, most likely, four times a year), the public women were summoned, at their lodgings, by the police-officers. In March, 1841, visitation was ordered to take place twice a month; in June, 1844, it was provided to take place once a week; at length, in July, 1853, *twice a week for each woman.* This is now maintained, and the women are no longer summoned at their lodgings to each visitation, but, on a card given to each of them, since 1852, the days are indicated on which they are to appear in order to be examined. Each woman is separately examined with the speculum, and besides the inspection of the exterior genitals and the skin of the thighs and belly, the anus and the cavity of the mouth

are also examined. At every opportunity endeavors are made to detect the persons with whom the women infected with venereal disease have been in contact. Such persons are then, through the police, invited to have themselves visited and duly treated.

With respect to the men of the army and the navy, there are particular provisions with regard to venereal disease. The staff-physician of the navy is, according to instruction of July 10, 1789, § 11, to take care that, among the persons levied for the navy, nobody is suffering from itch, *syphilitic exanthemata*, ulcers on the legs, ruptures, etc. Whenever a vessel is to put to sea, a visitation is ordered to take place.

With respect to the army, a writ from the ministry of war (College of General Commissariat) dated May 5, 1804, orders that the men shall be visited before they are sent home, in order to prevent their bringing venereal disease to their respective communities. If they are found to be suffering from such disease, they are detained in their garrison until the competent chief-physician can give them a certificate of health. A circular of August 5, 1805, from the same board, provides that, at the beginning of their time of service, an instruction shall be given to the soldiers respecting the symptoms of the disease, that they may know the dangers connected with their concealing or neglecting the same. All sergeants, musicians, and soldiers are visited when beginning their time of service, when being dismissed, and as a rule regularly twice a month. In order to prevent, as far as possible, the propagation of syphilis in the cities, military persons suffering from that disease are required to indicate the probable source of their disease, so that the police may know it, and prevent such woman from further propagating the disease.

The measures taken to prevent the propagation of venereal contagion have been, as may be seen from the preceding notices, arbitrary, and consequently not sufficiently to be relied on. This has been remedied by the following:—

Law of April 10, 1874, *respecting Measures to prevent the Propagation of Venereal Disease* (*approved by the Parliament and by the King*):—

§ 1. Persons suffering from venereal disease, whether able or not to defray their cure, are entitled to claim medical attendance at public expense, and in the mean time they are bound to submit to such cure if they cannot prove their having private medical attendance; if the circumstances of the persons affected are such that the contagion from them to other persons can be satisfactorily prevented only by their removal, or if they do not observe the instructions given in order to prevent contagion, they must be taken to an hospital to be cured. Respective questions are, if necessary, to be decided by the bailiff (in Copenhagen by the director of police) under recourse to the minister of justice; and the observance of the obligation can be enforced by coercive fines, dictated by the authorities. Persons who receive support through regular public alms and are found to be suffering from venereal disease, must be taken to an hospital to be cured. If, after the disease being cured, there is a particular reason to fear relapse of the same in a contagious form, the physician who has attended the patient may give her the injunction to appear before him at a fixed time, or to produce a certificate from an authorized physician that no such relapse has taken place. This injunction may also be enforced by coercive fines, dictated by the above-mentioned authorities.

§ 2. A child suffering from venereal disease must not be given to be suckled by any other woman than its own mother, nor is any nurse knowing or supposing herself to be suffering from the disease, permitted to take the child of any other mother to suckle it. Respective transgressions incur the penalty

mentioned for the crime in question, in § 181 of the General Civil Penal Law, and in the mean time the transgressor, if the disease has been propagated, shall be bound not only to indemnify the infected persons for the expenses connected with their cure, but also to give indemnity for the sufferings and losses occasioned by the disease. The same duty of indemnification is incumbent on any person who places a child at nurse, knowing that it is suffering from venereal disease, or suspecting it to be so, or who gives a child, suspected of the disease, to be suckled, without previously informing the foster-parents or the nurse that the child is suffering or suspected. These legal provisions are also applicable to public authorities who place children to be fostered or suckled. A child is to be looked upon as suspected of suffering from the above-mentioned disease, even if no symptoms of it have been observed, if the mother is suffering or has formerly suffered from the disease in any of its constitutional forms, and three months have not elapsed since the birth.

§ 3. If a woman, not belonging to those who, by regulations or rules, are submitted to the inspection of the police as belonging to prostitution, is definitively suspected of subsisting by prostitution, she is to receive warnings from the police, and to be informed of the consequences connected with the neglect of the warnings. In the mean time, if she consents to it, or admits herself to be leading a licentious life, she is to be examined by a physician, and, if she is found to suffer from venereal disease, to be submitted to cure according to § 1. A woman duly taught and authorized can also be trusted with this examination, and in that case the consent alluded to will not be necessary.

§ 4. § 180 of the Penal Law has been changed in such a manner that the penalty of imprisonment mentioned in the same may, where no compulsory workhouses exist as appointed by law for expiating punishment for vagrancies, be changed to compulsory work for a period not exceeding ninety days. If a woman is accused so as to be punished according to § 180 of the Penal Law, she is to be examined by a physician, and, if she is found to be suffering from venereal disease, is to be submitted to cure according to § 1. After having suffered the punishment, she is again to be warned according to § 3, and may then be submitted to the inspection of the police, conformably to the rules given in the following paragraphs.

§ 5. If a woman, in consequence of the preceding paragraph, is submitted to the inspection of the police, or voluntarily submits to it, she is to be ordered by the police to submit to medical examination at fixed periods, and to give notice of every change of lodgings. If, when examined by a physician, she is found to be suffering from venereal disease, she is to be submitted to cure according to § 1.

§ 6. Transgressions of the injunctions given in consequence of the preceding paragraph, are punished by imprisonment with bread and water for a period not exceeding four days, imprisonment with the usual prison-fare not exceeding sixteen days, or compulsory labor not exceeding twenty-four days (compare § 4.) If such woman does not present herself to be examined, and has no admissible reason for her absence, and is afterwards found to be suffering from venereal disease, she may, if residing at Copenhagen, or any other place having, in consequence of the law of February 4, 1871, § 2, Libra C., legal provisions for the inspection of licentious women, by the decision of the respective police masters, be submitted to the rules and regulations of that place respecting the inspection of licentious women. But she cannot be ordered to live in public houses unless her conduct after that time necessitates such measures.

§ 7. A woman who has already been sentenced according to § 180 of the Penal Law (compare § 4 of this law), and who is again found to have been guilty of subsisting by prostitution, is, besides being punished in consequence of the said rules, with the restriction mentioned in the preceding § 6, to be submitted to the regulations or rules for the inspection of licentious women, as long as, after having suffered the punishment, she continues to sojourn at

Copenhagen, or any other place for which, in consequence of the law of February 4, 1871, § 2, Libra C., such rules are given.

§ 8. The inspection mentioned in § 5 ceases, as far as the woman in question has not already formerly, in consequence of §§ 6 and 7, been submitted to, or has herself submitted to, the inspection which according to regulations and rules is instituted in certain places for licentious women, at all events at the expiration of six months. If, with respect to a woman's situation or conduct, such change has taken place as may be considered sufficient security with respect to her future conduct, the police master ought to exempt her from inspection before the expiration of the period mentioned.

§ 9. The medical examinations mentioned in this law are, if at the place in question no local police-physician has been appointed, to be executed by the public physician of the place, and in the locality designated by the police. For this the physician receives an annual remuneration, fixed by the municipality and approved by the minister of justice, or, if such remuneration has not been fixed and approved, a payment, for each examination, of 4 Kroner (2 Rd.) for the first person examined at the same time and at the same place, and 1 Kroner (48 Skilling) for each following person. The remuneration is paid from the town fund, in the country from the fund of repartition of the district, and in Bornholm from the united fund of town and district. The physician is not separately paid for a certificate stating whether the person examined has been found or not found to be suffering from the disease. The medical examination mentioned in §§ 3 and 5 may also, with the permission of the police-master, and on condition that the particular regulations given are observed, be executed by an authorized physician approved by the police, with whom the woman in question must then herself settle the matter.

§ 10. They who at public expense are taken to an hospital to be treated for venereal disease, are not permitted to leave the hospital before they have been dismissed by the physician of the hospital. Transgressions of this rule are punished by imprisonment with bread and water not exceeding five days, or simple imprisonment not exceeding one month.

§ 11. Lawsuits in consequence of §§ 4, 6, and 10 of this law are prosecuted in the same manner as public cases, but at Copenhagen with closed doors. The police is authorized to forbid persons, keeping public houses, to lodge women who have received warnings according to § 3 of this law, and also to forbid them to employ such women for the entertainment of their guests, or as servants.

If women submitted to the inspection of the police or subsisting by prostitution, or persons assisting and lodging them, are guilty of transgressions or neglect of the above prohibition with respect to regulations or rules which, in consequence of law of February 11, 1863, § 8, or law of February 4, 1871, § 2, C., are given for the inspection of licentious women, the police-master is authorized to sentence such persons to a fine not exceeding 100 Kroner (50 rix-dollars), imprisonment with bread and water not exceeding four days, or imprisonment with the usual prison-fare not exceeding sixteen days, or compulsory labor not exceeding twenty-four days.

Full effect was afterwards secured to this law by the police, through a practical provision. It being well known that a great number of the women who, in the capital, were comprehended in the above law came from other jurisdictions, and that, on the other hand, women who in the capital had received warnings according to § 180 of the Penal Law, or who had been entered in the registers of public women, left the capital and went to the provinces in order to continue a licentious life there, it was attempted to bring about a control based on a regular co-operation between the police of Copenhagen and that of the rest of the kingdom, with respect to such women during their removal to different parts of the country. It was stipulated by the police of Copenhagen that, when-

ever a woman under inspection as mentioned in § 5, or at length entered in the registers for public women, gave notice that she would leave the town, or was known by the police to go to another part of the kingdom, the respective police authorities should be informed of it; from such authorities the police of Copenhagen then requested similar communications, and to be informed of every sentence passed against any woman for transgressions after having received warnings according to § 180 of the Penal Law. In an appendix to the Police-Intelligence, published on the first and fifteenth of every month, and sent to all the police authorities, informations are given *partly* with regard to Copenhagen, respecting all women under inspection, or having left inspection, or entered in the registers for public women and again crossed out, *partly* with regard to the whole kingdom, respecting women who have been sentenced for transgressions after warnings according to § 180 of the Penal Law.

The warning mentioned in the law is by the police of Copenhagen given to the respective women by means of a printed blank, as follows:—

Transcript from the Registers of Warnings, kept by the Police of Copenhagen, for licentious women.

Case No. . . .

Year 18 . . the . . . has been given to . . . . . . the following warning: born at . . . . . . years old, and now lodging at . . . . . having admitted herself to have incurred positive suspicion of subsisting by prostitution, warning is given to her according to § 3 of law of April 10, 1874, and she has been given to understand that a transgression of the warning, according to § 180 of the Penal Law, and § 4 of the law above mentioned, will entail upon her punishment of imprisonment or compulsory labor.

For the correctness of the transcript:

. . . . , Police Inspector.

Women who, according to law, are submitted to the inspection of the police, receive a printed blank of the following contents:—

Transcript from the Registers of Warnings, kept by the Police of Copenhagen, for licentious women.

Year 18 . . the . . . . . . , who, according to sentence passed by the criminal and police court, division for public police cases, has suffered punishment of . . . . . . for transgression against § 180 of the Penal Law, appeared, and received renewed warning conformably to the warning given to her the . . . and which was read to her, and she was given to understand that a transgression would entail upon her punishment of imprisonment or compulsory labor.

It was at the same time declared to her that, according to § 4 of the law of April 10, 1874, she was submitted to the inspection of the police, and she was, therefore, given the injunction to appear, for the purpose of medical examination, in that division every . . . day, between . . . . and . . . . o'clock in the afternoon, and to give notice of any change of lodgings within twenty-four hours, under penalty of punishment according to the said paragraph, if she should in any way transgress against this injunction.

For the correctness of the transcript:

. . . . , Police Inspector.

On this transcript are also printed the contents of the penal law, and an extract from the law of April 10, 1874. Public women entered in the registers according to the law of April 10, 1874, have been, since that time, according to § 8 of the law of February 11, 1863, subjected to treatment according to the following regulations:—

Regulations for the Inspection of the Police respecting public women in Copenhagen.

By whom the inspection is to be made.—§ 1. The inspection incumbent on the police with respect to public women, is to be made conformably to § 31 of police instructions of June 7, 1869, by the third police inspector, and under the superintendence of the police director. The increase of medical assistance required in consequence of the execution of the sanitary measures connected with the said inspection, and also the distribution of the medical functions, is, according to § 7 of the law concerning the re-arrangement of the police of Copenhagen, to be regulated by supplements to the police regulations of June 12 of last year [1873], and to the police instructions of June 7, 1869.

Public Women.—§ 2. Every woman registered as a public woman is subjected to these regulations, and to the right of punishing which the police director has been given by the law respecting the mode of treatment of public police cases, of February 11, 1863, § 3. (Compare law of April 10, 1874, § 12.)

§ 3. No woman can, at her own request, be registered among public women before she has completed her eighteenth year. Exceptions, however, may be made with regard to women who are well known to have already become addicted to a licentious life, and when all endeavors to reform them have proved fruitless, and if they have, at the same time, completed their sixteenth year, and are of sufficient bodily development.

§ 4. No married woman can, at her own request, be registered as a public woman, unless she has been abandoned by her husband; and the latter, if his declaration in this respect is to be had, refuses to renew cohabitation.

§ 5. With regard to every woman, registered as a public woman, exact information must be produced of her name in full, place of birth, age, homestead and last place of residence, her own personal situation and preceding life, together with her motives for this step. It is therefore the duty of the police inspector, whenever a woman requests to be registered as a public woman, and especially if she has not already been submitted to the inspection mentioned in §§ 3–7 of the law for measures to prevent the propagation of venereal diseases, to satisfy himself that such woman fully knows the import and consequences of her intention, and by admonitions, by the assistance of her relations, and by other means at his disposal, to try to induce her to better purposes, and, if possible, facilitate her return to decent life.

§ 6. If, after the above injunctions having been observed, it is found that her request ought to be granted, she is to be submitted to medical examination, in order to ascertain whether her bodily constitution should in any way prevent her from being registered, and whether she is at the time free from venereal contagion. She is afterwards, by the police inspector, and before the protocol, to be acquainted with all the contents of these regulations, the needful of which is to be entered in the registers, and after this read to her, in the presence of valid witnesses, and agreed upon and signed by her. She is then registered in the list of public women which is to be kept in schematical form, containing, for each woman, an extract of notices concerning her person.

§ 7. When inscribed, such a woman receives, against a certain payment, a control-book containing the paragraphs of the present regulations with regard to her position as a public woman, as well as other rules that are to be observed by her, together with rubrics for medical visitations during a whole year. On the title-page is written her name in full, and the day when inscribed; on the next page the days on which she is to present herself for medical examination together with subsequent changes in that respect, and on the last page of the book her lodgings, and, if she is living in a public house, also the number of her room, together with later changes in this respect.

§ 8. Public women, with the exceptions afterwards mentioned in §§ 15 and 49, ought always to be in the possession of their books of control, in order to be able to produce them whenever so requested by the police. It is strongly

forbidden to pawn them, or to lend them to others. If a pubiic woman has lost her book of control, she ought, immediately, to inform the third inspector of it, when a new copy, marked "renewed," is to be given to her.

§ 9. The public women are divided in two classes: (1) Women who are living by themselves; (2) women living in public houses. To the first class belong: (*a*) those inscribed in consequence of §§ 6 and 7 of the law of April 10, 1874, as long as their conduct does not necessitate their being ordered to take lodgings in a public house; (*b*) those who have special permission to live by themselves. Such permission can be given by the police inspector, but, as a rule, only to those women whose conduct has always been unexceptionable, and only with the consent of the landlord of the house. This permission may be recalled at any time, and this ought to be done with such women as are often guilty of transgressions of these present regulations, particularly of §§ 11 and 12. All other women must live in public houses.

§ 10. Every public woman, whether living in a public house or by herself, must have the permission of the police inspector to change her lodgings, and, if she is herself the cause of her removing, she is only permitted to remove twice a month.

§ 11. No public woman is permitted to have her child at her house after its fourth year, and by no means the children of other people. She is not permitted to have her sweetheart at her house. She is not permitted to receive visits from males who are not yet adult. She is not allowed to receive any pawn from males visiting her. She is not allowed to lodge other women. The police has free access to inspect her rooms by day and night.

§ 12. Public women must carefully abstain from any disorder and from any indecent behavior in streets and public places, and must strictly observe the rules given them by the police in that respect; especially are they forbidden: (1) To appear in any indecent or sensational dress; (2) to show any indecent behavior; (3) to flock, to follow anybody, to accost or call anybody by words or signs; (4) to place themselves at their windows, or to be standing in the house-door, in the street, or on the foot-pavement before their houses; (5) to go in company with several others, or to marshal up, or walk up and down streets, public places, and walks; (6) to have other places in the theatres or public places of amusement than those permitted by the police; and (7) to be sitting guests in coffee houses, restaurants, and similar places of entertainment. During the time that the street lamps are lit, the regulations given under (4) need not be observed by public women living in the following streets: Holmensgade between Lille Kongensgade and Vingaardstræde, Bróndstroderne, and Diderik Badskjorsgang. Public women are never allowed to go through Amaliegaderne and Amalienberg Plads, nor, after particular orders from the police, in the principal streets of the city later than noon. Public women living in public houses are not permitted to appear in the street from the time when the street-lamps are lit, until they have been put out; the police-director is authorized to allow exceptions from this injunction.

§ 13. Public women are strongly forbidden to apply to males who have visited them, or to the relations of the same, in order to ask money from them, or for other reasons. But they may request, and according to circumstances expect, the intervention of the police.

§ 14. No public woman is permitted to go beyond the limits of the jurisdictions of the northern and southern parts of the district of Copenhagen and the jurisdiction of the island of Amager, nor to be absent more than twenty-four hours. Before being permitted to undertake a journey, she is to be visited by a physician.

§ 15. As soon as any public woman thinks herself infected with venereal disease, she ought to apply immediately to the examining physician, or to present herself at the hospital kept for that disease, and there she must deliver her control-book, that it may be sent to the police-inspector together with information respecting her being admitted at the hospital. Every public

woman admitted at an hospital for any other complaint than venereal disease, must also deliver her control-book at the hospital.

§ 16. Whenever a public woman is by illness prevented from coming to the medical inspection, she must every day of the usual examination send to the police-inspector an attestation from the physician attending her, concerning the nature of her illness, and that there is no symptom of venereal disease, or else apply for the inspector's permission to be examined at her home.

§ 17. Whenever a public woman, by certificate of marriage, has proved that she has been legally married, she is to be immediately obliterated from the registers of inscription.

§ 18. A public woman otherwise wishing to be obliterated from the registers of inscription, ought to apply to the police-inspector, whom she is to make fully acquainted with her future profession, occupation, and lodging, and who is then to inform the police-director of the case, and he is then to decide whether her request can be complied with; she must, however, if she does not adopt a solid position of life, or may by other means come to a situation offering sufficient security as to her future conduct, continue to be under the inspection of the police, and be subjected to the right of punishing with which the police-director is invested according to § 12 of the law of April 10, 1874, until, after having shown an unexceptionable conduct during three months, she may be definitively obliterated.

§ 19. Every woman obliterated from the registers of inscription, ought to receive a warning conformably to § 180 of the Penal Law, and it ought to be said to her that, if she again tries to subsist by prostitution, she will be prosecuted and punished, and submitted to the measures ordered by §§ 4–7 of the law of April 10, 1874, and, among other things, be immediately entered in the registers as a public woman if she has formerly been punished for licentious life.

§ 20. *Public Houses.*—The police is authorized to allow the existence of houses to afford lodgings for public women, under the inspection and responsibility of a hostess living in the house. Such concession may exceptionally be given to male persons, who are then subjected to the same regulations given, in the following sections, for the hostess.

§ 21. Concession to keep a public house is given by the police-director. It is always granted for an undetermined period, and preferably to old public women whose conduct has been without grievances. When such concession is sought for, the house must be exactly described, and it must be proved that the house-owner's consent has been given.

§ 22. Such public houses can, without the consent of the magistracy, only be permitted in the following streets: Lille Bróndstrode, Diderik Badskjorsgang, Dybensgade, Faruergade, Gaasegade, Helsingorsgade, Holmensgade (between Lille Kongensgade and Vingaardsstrode), Hummergade, Knabrostrode, Magstrode, St. Gertrudstode, Tornebuskegade, and Vogumagergade. A street once having become free from such public houses, new ones cannot be established there without the consent of the magistracy.

§ 23. Every hostess keeping a public house, shall be submitted to the present regulations, and the right of punishing given to the police-director by § 3 of the law of February 11, 1863. She must also conform to the injunctions which the police may find it necessary to give her.

§ 24. No married woman, dependent on her husband, can be permitted to establish a public house, without the written consent of her husband.

§ 25. In a public house no other inmates are permitted, without the permission of the police-director, but only the hostess, her husband, and children under the age of 7 years, the public women, and the necessary female servants. All rooms for public women ought to have consecutive numbers on the outside and the inside of the door, written in plain figures.

§ 26. They who keep public houses are bound to observe the greatest possible cleanliness, and to procure all such things as the police-director, after

having discussed the matter with the examining physician, may prescribe with respect to health. The windows facing the streets must always have Venetian blinds that are locked up.

§ 27. The hostesses of public houses must be accountable for the lodging women's coming to medical examination at the fixed hours, and in clean and decent dress.

§ 28. They who keep public houses cannot by any means refuse to the police officers to inspect the house, by day or by night.

§ 29. They are not permitted to give lodgings to any public woman, without notifying it to the police. The police-inspector must fix the number of public women allowed to each house.

§ 30. The hostesses of public houses are bound to inform the police-inspector of the name, age, and native place of each of their servants, and the latter can only be engaged with the consent of the police-inspector. Hostesses who are unmarried, or separated or divorced from their husbands, can be submitted to medical examination as ordered by §§ 40–49 and 52–54.

§ 31. The police is authorized to fix the payment which the woman living in public houses must pay both for room and fare in the house, and the hostess is to conform to this injunction.

§ 32. The hostess is answerable for the observance of the injunctions given in § 11, and she also can be rendered answerable that public women living at her house observe the injunctions of § 12, as far as, according to the nature of the same, she is able to have any influence in that respect.

§ 33. When a public woman has been given notice from her hostess to remove, both of them must, at the same time, appear before the third police-inspector, in order to have the necessary remarks added to the registers of inscription, and in the woman's book. Removal may be notified from both sides, with only twenty-four hours' notice.

§ 34. The hostesses are subject to punishment if women are found in their houses who have not their books of control, and have not given notice that they have been lost, or who have not been duly announced to the police conformably to § 29.

§ 35. The hostesses of public houses must always have in their possession an extract from these present regulations, containing all injunctions with regard to them; it must be bound like a book, and must bear the name of the hostess and the street number of the house, and state the number of public women permitted to live there.

§ 36. Any dissension taking place between the hostess and the public women living at her house, concerning their situation in that quality, is to be decided by the third police-inspector, and may only by his permission, or that of the police-director, be brought before a tribunal.

§ 37. All sorts of games with the guests are forbidden in the public houses.

§ 38. Objects having been forgotten in such houses ought to be delivered to the police within twenty-four hours.

§ 39. The hostesses are not permitted to leave the town for more than one day.

Sanitary measures.—§ 40. Every public woman ought to be, as a rule, submitted to ordinary medical examination twice a week.

§ 41. Extraordinary medical examinations are to take place as often as the police-officer thinks it necessary, and they do not exempt the public women from the ordinary medical examinations.

§ 42. The ordinary medical examinations are made in the public localities kept for that purpose; and there must be at least two such rooms in convenient parts of the town, besides the rooms for examinations at the police office. Medical examinations may also be allowed to take place at home, where the localities are fit for such use. This permission is granted by the police-director with respect to public houses, but may be given by the police-inspector to women that have private lodgings. The police-inspector is also

authorized to permit a woman who has private lodgings, to have her own physician, approved by him, whose certificate that she is not suffering from any venereal disease then replaces the medical examination. Such permissions may be recalled at any time.

§ 43. Public women who do not appear to be medically examined, or who are not at home in their houses at the time fixed for their examination, are to be arrested and taken to the police office to be examined, and are also subject to punishment.

§ 44. If any public woman tries to prevent the examination from being made as exactly as the physician thinks necessary, she is looked upon as having not appeared; and, according to circumstances, can also be taken to an hospital for observation.

§ 45. Whenever an ordinary or extraordinary examination has taken place, the physician is to inscribe in the control-books of the public women, and in registers kept for that purpose, the day when the medical examination took place, the state of health of the woman, the nature of the complaints observed, and other remarks he thinks useful. All these remarks ought to be made by the examining physician in person.

§ 46. Every public woman who by ordinary or extraordinary examination is found to be suffering from venereal disease, is to be taken immediately to an hospital, and have along with her a certificate declaring the nature of the complaint and where it is. Persons who show dubious symptoms of this disease are likewise taken to an hospital until it has been decided whether they are infected or not. In consequence thereof, women who are living alone, and are permitted to be examined at home, must appear at the hospital if they have not been found to be sound; and the hostesses of public houses who are allowed to have the examinations made in their houses, are co-responsible that persons living in their houses, and who at the medical examination are found to be infected or are suspected of being so, are immediately sent to the hospital.

§ 47. Every conveyance of loose women between the police office and the hospitals must be by carriage, and, as far as possible, at the cost of the respective women.

§ 48. Any public woman who, after having been declared to be suffering from venereal disease, removes from her lodgings, is immediately to be apprehended and carried to the hospital, and, after being cured, is to be punished. Likewise every public woman who is found to be suffering from the said disease without having declared it, will be punished if, according to the declaration of the physician, it must be supposed that, in consequence of the nature of the disease, she cannot possibly have been ignorant of it. Every public woman who is conscious of, or supposes herself to be afflicted with, venereal disease, and has nevertheless sexual intercourse, will be prosecuted and punished according to § 181 of the Penal Law.

§ 49. Whilst public women are at the hospital, their control-books are deposited at the police office, and are only returned to them when they have been dismissed from the hospital after having been cured. They are also, during the rest of their stay at the hospital, subjected to the injunctions of the police with respect to their conduct, and submitted to the right of punishing.

§ 50. Public women are bound to observe strictly the sanitary rules given to them by the examining physicians, or the police-inspector, or the physicians of the respective hospitals.

§ 51. The examining physicians are not permitted to treat public women, their hostesses, or the servants of the hostesses, for venereal disease or any other illness, nor to receive any fee from them or from others on their account.

Payment for medical examinations, etc. — § 52. Extraordinary medical examinations shall not be paid for. Likewise the ordinary medical examination, in at least one of the public localities for examination, is to be made gratis. For other ordinary examinations, when executed in the public rooms for examination, must be paid 25 Ore, or 50 Ore for each separate examination

after the time when they ought to have taken place; and, when executed at home, 1 Krone if the examination is executed in a public house, and 1 Kr. 50 Öre when in private lodgings.

§ 53. The fee for examinations at home, as well as for examinations to be paid for in public rooms for examinations, is to be paid in advance for a fortnight to the third police-inspector, and, as to examinations in public houses, paid by the respective hostesses or hosts, or else by the respective women. The hostess or the woman is given a mark for each examination paid for in advance, when the examination takes place, given to the physician and by him to be delivered to the police-inspector.

§ 54. Every public woman who has not paid in advance, and, according to the above rules, in due time, is in consequence thereof bound to appear at the next medical examination (§ 7) in the rooms for examinations gratis, and at the time fixed for such examinations.

§ 55. The control-books for the public women, as well as the books, etc., which, according to these regulations, must be found in public houses, are furnished by the police, but are paid for by the respective women or hostesses with a price like that which the police has paid for them.

Divers provisions.—§ 56. Public women subjected to these regulations who, without the precincts of the town, are guilty of transgressions against the obligations under which they are, according to these regulations or in consequence of the same, are to be punished as if the transgressions had taken place within the precincts of the town.

§ 57. The police-director is, by § 12 of the law of April 10, 1874, authorized to dictate punishment by fines not exceeding 100 Kr., imprisonment with bread and water not exceeding four days, and imprisonment with the usual prison-fare not exceeding sixteen days, or compulsory labor in the institution for compulsory labor at Copenhagen, to public women, hostesses (and hosts) who have a concession to keep public houses, and to the servants whom the police-inspector has permitted the hostesses or public women to have.

§ 58. All expenses occasioned by the inspection of public women are to be paid by the municipal fund which receives the payment fixed by § 52 for medical examinations.

Similar regulations may, according to § 2, C., of the law of February 4, 1871, be made for country towns.

No positive results of the law of April 10, 1874, can, of course, yet be pointed out with respect to the diminution of the venereal diseases; but such effects can be stated as will give an idea of the results that may be hoped for.

No greater number of public women has been registered than formerly. The respective yearly numbers were

1872	289
1873	294
1874	353
1875	345

but before that time the numbers were as large; thus in

1862	354
1863	352
1864	354

and for the rest it has been between 200 and 300. But the number of women under control, by being submitted to medical inspection without being registered as public women, has risen to a very high degree. The number of women suspected of a licentious life, and who have produced attestations from private physicians, are

1872	337
1873	567
1874	1027
1875	1340

These numbers show that, since 1874, control has been effected with a very great number of the sort of women who are the most dangerous with respect to the propagation of venereal contagion.[1] The great importance of this control may be seen from the following table, in which the first columns refer to the women under inspection, and the others to women not under frequent inspection, but occasionally apprehended and examined by the police.

Result of examinations of women under inspection.		Result of examinations of women not under inspection.		
Number of examinations.	Number found diseased.	Number of examinations.	Number found diseased.	Years.
	PER CENT.		PER CENT.	
27674	780 or 2.8	602	173 or 28.7	1860
27825	772 " 2.7	551	167 " 30.3	1861
31229	935 " 2.9	653	192 " 24.4	1862
30483	1056 " 3.4	651	216 " 33.1	1863
32020	1046 " 3.2	642	169 " 26.3	1864
29106	1050 " 3.4	476	122 " 25.6	1865
27863	990 " 3.6	642	152 " 23.6	1866
23538	686 " 2.9	888	171 " 19.2	1867
22622	814 " 3.6	722	230 " 31.8	1868
23407	800 " 3.7	806	261 " 32.2	1869
25740	879 " 3.4	602	181 " 30.0	1870
25748	679 " 2.6	655	182 " 27.7	1871
26145	823 " 3.1	637	253 " 39.7	1872
25547	751 " 2.9	786	229 " 29.1	1873
26783	885 " 3.3	953	300 " 32.5	1874
29165	907 " 3.1	670	178 " 22.2	1875

[1] Among special affections I will only mention mucous patches, which have been found nine times as often in women not under control as in those registered and regularly inspected.

SECTION ON OBSTETRICS.

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OF PHILADELPHIA.

On taking the chair, Dr. BARNES said:—

I do not know how I can express my thanks for the very unexpected honor of being called upon to preside over this Section—one of the most important connected with this Congress. In England we recognize the value of this branch of study, but there is no country that can claim superiority over America in the development of the science of Obstetrics. Your writers are acknowledged as authorities, and, as you are in the lead, it is incumbent upon you to cultivate and improve your discoveries. It is incumbent upon us all to cultivate the practice of obstetrics, for it lies at the very root of the human race. I have been asked why I did not attend a congress in St. Petersburg, which may be called a great medical centre. I preferred to come here, and yet it seems singular to leave the centre and come to what we might call the outside. It is no slight task to spend three weeks of one's holiday on the stormy ocean, but our assembly to-day is an event—it might be called an epoch—in the History of Medicine. I cannot sufficiently express my sense of the honor conferred upon me. I hope to hear some interesting discussions upon the topics brought before us, and have no doubt that I shall go back to my home wiser than when I came.

THE CAUSES AND TREATMENT OF NON-PUERPERAL HEMORRHAGES OF THE WOMB.

BY
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The short time at my disposal makes it impossible for me to give anything more than a brief outline of the etiology and therapeutics of uterine hemorrhage, and I shall, therefore, confine myself to a succinct expression of the views which may be fairly sustained by our present knowledge of the subject.

Without stopping to discuss at length the meaning of the terms applied to uterine hemorrhages, I may say that I shall use the words "uterine hemorrhage" and "metrorrhagia" in the same sense, and shall apply them to such losses of blood as occur between the menstrual periods, or in unusual amounts at the time of the periods themselves. An excessive, or too long-continued, flow of blood at a menstrual period is practically neither more nor less than metrorrhagia, originating in like causes, and amenable to the same treatment.

Causes of Metrorrhagia.

Non-puerperal hemorrhage is properly regarded as a symptom, and the consideration of its causes brings to our attention a great variety of local and general pathological conditions. Exceptional cases of metrorrhagia sometimes result from direct violence to the uterus, or from sudden and severe shocks to the system, which apparently act upon the uterus in common with other organs, and cause it to bleed because of its inherent tendency to do so. The pathological causes which induce uterine hemorrhage do not always—perhaps do not generally—act singly, but two or more of them are found associated in most cases, some being essential, and others merely auxiliary. The uterus, by virtue of the conditions upon which menstruation depends, is naturally a hemorrhagic organ; and it is in consequence of its anatomical and physiological peculiarities that the ordinary and frequently acting causes of uterine hemorrhage are rendered so potent and effective.

The more obvious phenomena of menstruation are doubtless the result of a definite reflex nervous influence exerted by the ovaries upon the uterus. Although this influence is more distinctly manifested in the great hyperæmia which precedes the occurrence of the catamenial discharge, and the changes in the utricular glands and mucous membrane of the womb, yet it is unquestionably constant in its action and parallel in this respect to that which presides over the motions of the heart, the arteries, and the alimentary canal. Generated in the nervous apparatus of the ovaries, and contemporaneous with the changes called ovulation in those organs, this influence is probably conveyed by afferent nerves to the genito-spinal centre (the existence of which was first established by

Budge, of Greifswalde[1]), or to some other reflecting ganglion, whence it is sent back to the uterus, giving rise to a wonderful series of tissue-changes during the month. Some of these changes have been lucidly described by Dr. John Williams, in the Obstetrical Journal of Great Britain and Ireland, and by our own talented young countryman, Dr. Engelman, in his recent essay upon the subject, published in the American Journal of Obstetrics. These changes are aptly termed by Aveling, nidation and denidation.

A few days before the menstrual flow makes its appearance, the whole uterus, and especially its mucous membrane, becomes greatly hypertrophied and very vascular; when the discharge begins, the membrane is invaded by fatty degeneration. This process is so rapid that, in four or five days, the entire mucous membrane disappears, leaving the muscular structure of the inside of the uterus exposed, while some remnants of the utricular glands are left, and found entangled among the denuded fibres. As soon as the monthly flow ceases, a reproduction of the membrane is commenced, and it continues to grow until at the end of twenty-eight days its menstrual maturity is attained. Accompanying these changes in the cavity of the uterus are others equally remarkable, affecting all the other tissues of the organ. The bloodvessels become enlarged, and circulate an increased amount of blood; the fibrous tissue is developed beyond its inter-menstrual condition; while hyperæsthesia indicates extraordinary nervous endowment. In fact a true hypertrophy of the uterus occurs. During the discharge, the process of involution reduces the organ to its smallest dimensions, and the hemorrhage ceases. The culmination of this hypertrophy in the discharge of blood from the uterus is doubtless not merely an accompaniment, but a consequence of the breach of capillaries in the mucous membrane. These of course are physiological phenomena, but I desire to invite attention to the fact that they strongly resemble pathological conditions, and would be so considered in any other organ in the human economy. Moreover the dividing line between health and disease in uterine hemorrhage is as difficult to trace as that between sanity and lunacy.

It is not difficult to understand that an exaggeration of the hyperæmia, or an unusually rapid disintegration of the uterine mucous membrane, would cause more than a normal amount of flow; nor that a want of accordance in time might be followed by the same result. Indeed most cases of uterine hemorrhage are traceable to conditions which disturb the equilibrium of these phenomena. The causes which thus act are varied and numerous.

Morbid nervous influences which increase the discharge of blood from the uterus, sometimes emanate from the nervous centres, and hence may be properly termed *centric;* much more frequently, however, they are reflected through the nervous centres from other and sometimes distant organs, and these last are entitled to the denomination of *reflex* or *eccentric* nervous influences.

Mental and emotional excitement emanating directly from the brain, and cerebral and spinal excitement originating in inflammation or functional exhaustion of the brain or spinal cord, are examples of *centric* etiological influences. Many years ago I witnessed the ravages of an

[1] Ueber das Centrum genito-spinales des N. sympatheticus. Virchow's Archiv f. Path. Anat. und Klin. Med., Band XV., S. 115–126, and Chicago Journal of Nervous and Mental Diseases.

epidemic of cerebro-spinal inflammation, in which uterine hemorrhage was of almost universal occurrence among those adult females who fell under its influence.

Morbid *reflex* nervous influences afford a more numerous class of causes. First among them, both in frequency and importance, are those arising from abnormal conditions of the ovaries, such as congestion, inflammation, displacement, and erotic excitement. Next to the influence of these bodies is that exerted by the mammary glands. Menstruation is generally more profuse when it occurs during lactation. The effect of mammary irritation in causing congestion of the uterus, and thus promoting hemorrhage from it, is well illustrated by the familiar fact that sinapisms or blisters applied to the breasts will often cause metrorrhagia. Vesical irritation or inflammation, which gives rise to tenesmus; rectal irritation, as from the presence of hemorrhoids, or ascarides; and dysenteric inflammation, through the reflex influence which they exert upon the uterus, are generally recognized causes of uterine hemorrhage. Among other reflex causes may be mentioned certain forms of indigestion, hepatic congestion and inflammation, and some of the disturbances of the small intestines, as may also strong impressions upon the cutaneous surface, as from cold, or from the long-continued application of heat in warm climates and seasons.

All of these last-mentioned causes I think act through the reflex system of spinal nerves, and perhaps also through the agency of the sympathetic ganglia which perform a reflex function between the viscera. The morbid effects of the various reflex nervous impressions are rendered more effective and intense by the presence of such uterine diseases as predispose to hemorrhage by increasing the vascularity of the uterus.

Many *pathological conditions* which conduce to the production of uterine hemorrhage, independently of direct nervous influence, act by increasing the hyperæmia of the uterus. When the mucous membrane is granulated, or is the seat of inflammation, of fibrous polypus, or of malignant fungus, the circulation of the uterus is increased, and harmony in the process of nidation disturbed; and these conditions will be accompanied by an unusual and long-continued flow of blood. Subinvolution, congestion and inflammation, hyperplasia, tuberculosis, cancerous and fibrous deposits in the muscular structure, and chronic and acute endometritis, in addition to preventing the normal deciduous changes in the mucous membrane of the uterus, maintain a permanent hyperæmia, and thus render the womb prone to large losses at each return of the menstrual period. We have in fact abundant reasons for assuming that chronic hyperæmia, no matter how produced, will, by virtue of the malnutrition connected with it, prevent menstrual changes from being effected in an orderly manner, and thus render the mucous membrane more frail in organization, and consequently incapable of resisting the force of vascular pressure to which it is periodically subjected.

Besides the causes of uterine hyperæmia last alluded to, and existing within the tissues of the womb, there are many other *outside pathological conditions* acting in a different way. Some of these cause venous hyperæmia by mechanical retardation of the circulation, while others give rise to both arterial and venous hyperæmia by nutritional attraction, and others again cause arterial hyperæmia alone, by forcing unusual amounts of blood into the organ. Among the most frequent and important causes of *venous* retardation are displacements and flexions of the uterus—procidentia, retroversion and retroflexion—the former by stretching the

veins and rendering their course more tortuous, the latter by twisting them and thus lessening their calibre; exudations into the cellular tissue and peritoneal pouch, from cellulitis and local peritonitis, and effusions of blood in the cul-de-sac of Douglas, in retro-uterine hematocele, by pressing upon the veins, prevent a free return of blood from the uterus, and thus cause venous hyperæmia. Retardation of movement in the uterine veins may also be caused by obstruction to the venous circulation quite remote from the womb, as by the pressure of a tumor upon the ascending vena cava, by a loaded condition of the large intestines, by dislocation or enlargement of the liver, by obstruction to the free passage of blood through the heart from valvular disease, and even by certain pulmonary affections.

In the class of causes giving rise to both *arterial and venous* hyperæmia, may be mentioned fibrous, fibrocystic, polypoid and fungous growths of the fibrous structure of the uterus. These all increase the flow of blood to and through the vessels of the uterus, both arteries and veins are increased in capacity, and to these changes is added general hypertrophy. In these cases, the hyperæmia of all the tissues is sometimes enormously great, and the losses of blood are proportionally large and dangerous; the hemorrhage, unlike that from venous obstruction, is not checked by the emptying of the vessels, but continues until the arterial and cardiac *vis-a-tergo* is weakened by approaching syncope.

Causes producing *arterial* hyperæmia alone, are hypertrophy of the heart, general plethora, febrile excitement, and violent exercise. The uterine hyperæmia in these cases is caused by unusual arterial and cardiac pressure alone. When not attended by local pathological conditions, the hemorrhage in these cases is not apt to be serious.

Other not uncommon causes of hemorrhage from the womb are various *diseases of the blood.* Among these may be mentioned scurvy, leucocythæmia, chlorosis, albuminuria and syphilis. It is not likely that the vice in the composition of the blood is the sole causative influence operating in the above-named conditions. In scurvy, for instance, we know that the solid tissues—whether as a primary condition or as an effect of the blood-changes—are diseased, the capillaries more fragile than natural, and consequently less capable of resisting the cardiac impulse. As evidence that the vicious condition of both blood and solid tissues is the cause of uterine hemorrhage in scurvy, the well-known fact may be added that bleeding is very easily provoked in other mucous membranes. It is the more likely to take place from the mucous membrane of the uterus, because of the great normal fluctuations in the circulation of that organ, and also because the vitiated state of the blood would naturally cause disturbance in other conditions attendant upon menstruation, especially the decidual changes. It will be seen therefore that the peculiarity in the operation of this variety of cause, is not due to the presence of local or general hyperæmia from retardation of the venous circulation, or from arterial and cardiac pressure, but is due to the tendency of the blood to escape through the walls of the vessels, and to the inability of the capillary tubes to resist the circulatory force ordinarily applied to them.

As another cause of hemorrhage from the womb, must be mentioned the well-known law of the human system to continue a long-established *habit*, after the original cause is removed. This is probably the only rational explanation of those rare uterine losses which are sometimes observed in pregnancy, and in cases where both ovaries have been re-

moved. The habit of bleeding continues after the ovarian, reflex, nervous influence has been withdrawn from the uterus.

Still another rare, yet very dangerous, cause of uterine hemorrhage, is that known to surgeons as the *hemorrhagic diathesis*. The writer has seen one case in which he believes that the bleeding was clearly attributable to this mysterious condition, and which proved fatal. It was that of a young girl who died with her second menstrual flow.

The wide range of causative conditions found connected with uterine hemorrhage is but an inverse exhibition of the sympathetic relations of the uterus. When diseased, it exercises an almost universal pathological influence upon other organs, and as a consequence it is susceptible of being impressed to the same degree by certain morbid conditions of all important viscera. In this assembly of intelligent practitioners it will not be regarded as making an undue claim to say, that the practice of gynæcology requires a more thorough theoretical and practical familiarity with the details of all the branches of medicine than any other of the so-called specialties. We are not prepared to treat the most common of female diseases without being able to scan with scientific scrutiny every organ and function of the body. Nor until we can compete successfully with the general practitioner, the surgeon, the alienist, and the neurologist, in the therapeutic processes of their respective departments, may we hope to exercise in the highest sense the office of the gynæcologist. These remarks apply with force to the comprehension of the causes and treatment of hemorrhages of the unimpregnated and non-puerperal uterus.

Treatment of Metrorrhagia.

I find it quite impossible to satisfy myself as to the best order in which to bring forward the various measures proposed for treating uterine hemorrhage. Those which have for their object the removal of the causing conditions, properly fall under the head of *curative* means; while those which we employ to stop the bleeding temporarily, until the remedies of the first order have accomplished their purpose, seem as naturally to belong to the category of *palliative* measures. We find in each of these divisions, however, remedies which act in both ways, and the palliative means are often radical and energetic. Notwithstanding the many obvious deficiencies in this arrangement, it seems to me to be the best that I can adopt.

Palliative Treatment.—Before entering into a detailed description of the more essential, remedial methods of curing the various forms of hemorrhage, it will be profitable to consider some of the important, minor measures which are applicable in almost all instances. As the great majority of hemorrhages occur at the menstrual periods, we often have opportunities of adopting measures in anticipation of them. These measures are sometimes calculated to entirely prevent an exaggerated flow, and at others to very much modify it; and in all to greatly promote the action of more direct remedies. The patient should abstain from all causes of local or general, vascular or nervous excitement. Among these causes are mental and bodily fatigue, emotional excitement arising from certain social relations, sensational books, and the contemplation of erotic objects. The patient should also abstain from stimulating drinks and highly seasoned food; her clothing should be loose and cool, so that no

part of the body may be constricted, and the genital organs should not be too warmly covered. Her bowels ought to be kept regular, or rather free. The secretions from the skin, liver, and kidneys should be maintained as nearly as possible in a normal condition, and tonics, such as arsenic, strychnia, and quinia, with digestible, nourishing and unstimulating diet, should be given in quantities sufficient to keep the health up to the normal standard. Other things which will contribute very greatly to good results, are plenty of pure air, night and day, and moderate, muscular exercise. Many other general directions will suggest themselves, which I cannot stop now to mention.

When the time for the paroxysm has arrived, and the hemorrhage has commenced, isolation, quietude, and recumbency are very important precautions to be enjoined. Position indeed may be made to do much good of itself. If the hemorrhage is not severe, mere recumbency will be sufficient; but if it is protracted, the hips should be elevated, and sometimes it will be beneficial to raise them so high as to cause the blood to gravitate to the fundus uteri, and to fill the whole genital canal before any of it passes out. To a considerable extent this may be made to act as a tampon. The position chosen to effect this object may be on the back, or upon the knees and chest. If the latter position can be commanded, it is much the best, as the reversal of gravitation is more complete. Cold and acid drinks, cold applications to the hypogastric and sacral regions, hips and vulva, and in the vagina, are also among the remedies applicable to almost all cases.

Many practitioners value astringents, administered internally, in uterine hemorrhage, but I have found so little benefit from them when not given with opium or belladonna, that I seldom resort to them. Where there is much pain in the pelvis, and a dry state of the skin, opium and ipecacuanha are often very servicable. Lobelia, gelsemium, digitalis, aconite, and veratrum viride, may also be mentioned as very frequently applicable where there is vascular and nervous excitement.

Perhaps the medicine most generally applicable in paroxysms of uterine hemorrhage, is ergot. In all cases of local arterial hyperæmia, as in tumors, hyperinvolution, etc., we may expect good from its employment. But it will generally fail to be useful when the uterine hyperæmia is venous, as in retroversion, pelvic infarction from periuterine effusion, abdominal tumors, etc. It will not act efficiently in cases of carcinomatous deposit, granulations of the mucous membrane, or tuberculous degeneration of the fibrous texture of the uterus.

In the more dangerous instances of hemorrhage, these moderate palliative measures are not sufficient. In some, the amount of loss is so great, and occurs so suddenly, as to threaten the life of the patient. Or, if life is not in danger, the discharge may be sufficient to lead to other very serious, remote consequences. These emergencies are to be met by such means as will promptly arrest the flow, and keep it in check until curative processes can be instituted. Fortunately this may be done with great certainty by mechanical and chemical appliances generally at our command. The genital canal, practically closed at its upper extremity, and conveniently open at its lower termination, admits of being impacted to an impermeable degree, and allows of topical applications to its whole extent. In using either form of these topical measures, the effort should be made to apply the remedy as near to the bleeding point as possible.

When practicable, we may secure the best effects by employing the mechanical and chemical means conjointly. The *mechanical* means em-

brace the different forms of the tampon. Plugging arrests the hemorrhage by forcibly opposing the evacuation of the blood, and by thus imprisoning it in the smallest cavity. The blood so confined, coagulates, and fills the space between the tampon and the bleeding surface with a fibrinous clot which also closes the mouths of the vessels. When plugging is skilfully performed, the relief is temporarily perfect, and gives us valuable time for other treatment, or allows the cyclical period to pass, when the hyperæmia subsides. The *chemical* means consist in the use of powerful hæmostatics. By their chemical action upon the solid constituents of the blood, they produce a much firmer coagulum than results from mere stasis, and, if applied to the ruptured vessels, seal them up with coagulated plastic material, while if further away the coagulum forms a chemical tampon which opposes the flow toward the vulva. Used with the mechanical tampon they may be made to fill the interstices of the material of which it is formed, and thus solidify the whole mass.

In the greater number of dangerous cases of the kind of uterine hemorrhage treated of in this paper, the mouth of the womb is sufficiently patent to permit the introduction of the plugging material saturated with a hæmostatic preparation into the cavity of the uterus. Dr. Sims's method of preparing the material and performing the operation of plugging the womb is admirable in its simplicity and efficiency. The substance used is the finest article of cotton-wool, saturated with a liquid composed of one part of the strong solution of the subsulphate of iron and two of water. After the cotton has been perfectly saturated, it is deprived of the major part of its fluid by pressure, and is then allowed to dry until ready for use. The application is made by wrapping a sufficient quantity of the dried iron-cotton around a long, small piece of whalebone, and introducing it into the cavity of the uterus, when the cotton is detached and left there. If the hemorrhage is comparatively moderate, one of these pieces may be sufficient; if severe, it will be necessary to stuff the uterine cavity full. This can be best accomplished by having the patient placed on her side, and the uterus exposed by Sims's speculum. To facilitate the removal of this ferruginous tampon, the suggestion of Dr. J. R. Chadwick, of Boston, is, I think, a valuable one, viz., to wrap strong thread loosely around the cotton as it surrounds the whalebone. I prefer this method of using the hæmostatic to its injection, because the shock from the application is much less.

If the mouth and cervical cavity of the womb are not sufficiently open to permit of the introduction of this hæmostatic preparation, we may plug the cervix with prepared sponge. The first sponge should be pushed through the cervix into the cavity, and up to the fundus uteri, so that when it expands its upper end may possibly reach and press upon the bleeding point. If large enough, the cervical cavity will be completely filled and the bleeding effectually checked. The sponge should be carbolized, and well secured, before it is introduced, by passing a strong piece of twine through it, from one end to the other. Neither the cotton nor sponge should be allowed to remain longer than twenty-four hours, and half of that time is usually long enough. After removal, the vagina may be cleansed, and the application repeated if necessary. I have sometimes been obliged to renew the sponge tampon several times in the same case, though this is not usually required.

If these means are not at hand, or if the case is not sufficiently urgent to require plugging of the uterus, we may resort to the vaginal tampon.

This may be made of cotton, of which pieces as large as pullets' eggs may be used, rolled somewhat solidly, and each secured with thread and lubricated with oil or lard. A sufficient number to perfectly fill the vagina should be prepared. The patient should be placed on her left side, with the limbs flexed, and the upper one thrown forward over the other. The operator, standing at the back of the patient, inserts into the vagina two fingers of the left hand, with which he draws the perineum well backward. This will open the canal so that the clots may be easily removed with the fingers, when, with the right hand, the cotton may be placed with great facility in the vagina. At first several on the os, and around it, and then the whole vagina may be packed solidly under the eye of the operator. If Sims's speculum be at hand, it should be used instead of the two fingers to hold back the perineum. Or we may vary this according to the process described by Dr. Thomas, in the American Journal of the Medical Sciences for July, 1876, page 147. After dilating the vagina, "pieces of cotton, soaked in water, pressed and flattened out by the fingers, each about the size of a very small biscuit, are pressed into the vaginal cul-de-sac by means of forceps till this is filled. Then other pieces are packed firmly around the cervix until only the os is visible—a smaller pad is then pressed firmly against or introduced within the cervical canal, and the whole vagina is then filled to its lowest portion." An ordinary surgeon's roller answers admirably for a plug, and may be introduced by first inserting one end, and then passing it up in short folds until enough has been placed in the vaginal cavity to fill it up compactly. In most cases where we desire to leave the patient, the tampon should be retained by a compress and "T" bandage.

When we have reason to anticipate a sudden occurrence of severe hemorrhage in our absence, we may instruct the patient or nurse how to make and apply a very safe vaginal plug. A sponge, large enough to fill the vagina closely, may be prepared by wetting it in a strong solution of alum, or in a weak solution of subsulphate of iron, passing a piece of strong twine or tape through the centre, and then wrapping it with tape in an elongated shape to its smallest dimensions. It may then be laid aside to dry. When the necessity for its use arises the tape is removed, and the sponge thus compressed may be passed without any resistance entirely into the vagina. It is soon expanded by the blood, and the vaginal cavity thoroughly filled. A few of these sponges prepared ready for instant use will enable the patient to prevent any material loss until the practitioner arrives. The plug may be removed by the tape or twine whenever desired. The plug may be allowed to remain from eighteen to twenty-four hours, when it should be withdrawn, and, the vagina having been thoroughly cleansed with carbolized water, replaced if necessary.

Curative Treatment.—The central nervous disorders which cause uterine hemorrhage will, when recognized, require the treatment set forth in the various works upon these subjects. I need not, therefore, dwell here upon the management of the spinal and cerebral inflammations and irritations, nor upon the numerous forms of emotional excitement which lead to metrorrhagia. The treatment of the reflex, morbid, nervous influences, belongs more particularly to gynæcology, and will call for all the ingenuity and varied knowledge taught in that branch of practical medicine. The ovarian derangements, being the more obvious and common of these, may be noticed first. Our means for replacing and retaining in position displaced ovaries are very meagre. The patient must be

confined to the horizontal position, with the pelvis elevated as much as practicable. The knee-chest position is the best, and may often be maintained for a considerable part of the twenty-four hours. Generally the displacement is accompanied by congestion or inflammation of the ovary, which increases its size and weight. When this is the case, the treatment, in addition to position and quietude, recommended during the intermenstrual period, will consist in the use of counter-irritants, hip-baths, hot water vaginal injections, and alteratives, administered internally, or applied externally in the form of ointments, or per vaginam as suppositories, injections, etc. Among the alteratives, the muriate of ammonia will be found very valuable. When there is much debility, the bichloride of mercury, dissolved in the compound tincture of cinchona, is among the very best. Iodine, iodide of potassium, and iodide of iron should also be named as efficient alteratives in these conditions of the ovaries. One derivative measure which I desire to mention as especially beneficial in these cases is dry cupping over the sacrum, often repeated. To be effectual the cups should be large, and allowed to remain for a long time, say an hour or more. When there is much pain in the ovarian regions, suppositories of the extract of belladonna and ergot, once or twice daily, will not only relieve the pain, but will do much towards allaying the inflammation.

When hemorrhage occurs in a nursing woman, if it is of sufficient gravity, the child should be weaned. At the time of the paroxysm, if the breasts are tumid and tender, cold may be applied to them to relieve both the uterine hemorrhage and the mammary congestion. These patients require invigorating measures in connection with the local treatment of the breasts.

The vesical or rectal tenesmus which gives rise to hemorrhage must be treated by the remedies found necessary after investigating the cause. So, also, with diseases of the stomach, bowels, and liver, as well as with the effect of cold or of long-continued heat.

Subinvolution and chronic congestion of the whole uterus require to be treated very much alike, by the application of such remedies as condense the uterine tissues—ergot, belladonna, quinia, electricity, cold injections, compresses and sitz-baths. When there is no tenderness, ergot will be found a very efficient remedy, if administered for a sufficient length of time—several months for instance. If there is considerable tenderness and pain, belladonna and quinia will be best adapted to the case. Ergot in some instances induces sensitiveness and heat in the pelvic organs, and then it should be used very cautiously. This effect of ergot is especially noticeable when there is chronic local peritonitis or cellulitis. If there is a high degree of sensitiveness, a mercurial alterative may very properly be given in connection with the belladonna and quinia, and a good form for administering it is the bichloride of mercury dissolved in the compound tincture of cinchona; or we may use mercurial inunction, or mercury in suppositories. I have not been able to do much good in these cases with iodine in any form. If given with iron, as the iodide of iron, it has occasionally a good tonic and alterative influence. These conditions of the uterus are very obstinate, and require a continuous treatment, oftentimes for many months.

The treatment of endo-metritis is too well described by systematic writers on gynæcology to require more than a very brief notice. It consists mainly in a persevering continuance of stimulating applications to the diseased mucous membrane. I do not like the term caustic, for

even the strongest remedies used for this purpose are applied so sparingly that their effects are little more than strongly stimulative. In the light of our present knowledge of the processes of menstruation, these remedies, as suggested by Dr. Atthill, should be resorted to immediately after the monthly flow has ceased. By common consent of the profession, in this country, the treatment of granulations of the uterine mucous membrane consists in scraping them off, and the teaching of Sims and others has made the process sufficiently familiar not to require description here. Although not curative, the same treatment may be mentioned as most efficacious in arresting the hemorrhages resulting from cancerous granulations. In a discussion of Dr. Hanks's recent paper, Dr. Peaslee gives the very judicious advice not to cut into the sound tissue in the process. In cases of malignant fungus, we may often arrest the tendency to hemorrhage by injecting alcohol, by means of a hypodermic syringe, deeply into the substance of the part. This process frequently repeated sometimes retards the growth very materially. The tincture of the chloride of iron, similarly used, will often have the same effect.

The various conditions which give rise to retardation of the venous circulation, require to be treated according to the improved methods now so well understood by the profession. The displacements of the uterus which are arranged among these conditions, must be corrected by the various ingenious appliances designed for this purpose. And this may be done during the time of the preternatural flow with the expectation of moderating it at once.

Dr. T. D. Fitch, of Chicago, has recently proven this last assertion in the management of a case occurring in a patient who had just passed the menopause. The uterus was retroverted, and all the means resorted to did not even moderate the metrorrhagia until the organ was elevated and retained in position by an appropriate pessary, when in a short time the bleeding ceased. After the subsidence of the flow, the patient removed the instrument, on account of some slight inconvenience which it gave her, but the flooding began again in a very few hours, and continued, notwithstanding repeated efforts to arrest it, until the pessary was once more introduced, when the hemorrhage again subsided, and has not returned. The patient was still wearing the pessary when I heard of her case.

The subject of the treatment, and especially the surgical treatment, of tumors of the uterus which cause arterial hyperæmia and uterine hemorrhage, is too extensive to be entered into in this paper; and I refrain from considering it the more willingly because it will no doubt be exhaustively and ably discussed by Dr. W. L. Atlee in the paper which he is to read before the Section. I cannot, however, forego the duty of making some suggestions as to the method of treating these growths with internal remedies.

The extreme danger from hemorrhage, connected with fibrous tumors of the uterus, is not so often encountered since the profession has become acquainted with the great influence exerted upon certain conditions of the unimpregnated uterus by ergot. It is now understood that fully seventy-five per cent. of hemorrhagic cases of fibrous tumour of the uterus may be rendered free from danger, as far as the hemorrhage is concerned, by an intelligent and persevering use of ergot, and that in twenty per cent., the tumors may be removed. In using ergot, in these cases, the mode of administering it cannot be uniform. Some patients cannot take it in any sufficient doses to answer the purpose; some cannot take

it in the form of fluid extract, or wine, but can take the solid extract in the form of pills; others can take it in any form. When the stomach will not tolerate the ergot, it may be given hypodermically, or per rectum in suppositories, and I believe that it can be made to act efficiently when given in any of these ways.

Whatever method or form we may adopt in the administration of ergot, we should give it in sufficient quantities to produce a sensible effect by causing contractions and pain, and there is no better rule to guide us, as far as I can judge, than to give it in increasing doses until that result follows. Twenty minims of the fluid extract, three times a day, will sometimes be sufficient, while some patients, on the other hand, will require twice or three times as much to produce the effect.

The length of time required to obtain the ultimate effects of the ergot in these doses, varies as much as the quantity required. The tumor will sometimes diminish very rapidly, but generally the decrease in size is quite slow. From one month to over a year may be required to accomplish a cure when it can be accomplished at all. Ergot is sometimes very violent in its action, but by withdrawing it temporarily, lessening the dose, or combining and alternating it with anodynes, it may be safely managed. Although I have given it extensively, and for a long time together, I have not seen anything worse than inconvenience arising from its use.

It will not, of course, be expected that I should enter into a consideration of the treatment of scurvy, leucocythæmia, albuminuria, or syphilis, as general conditions causing uterine hemorrhage, both for want of time and because they are subjects well treated of in works on the general practice of medicine. Our ignorance of the last of the causes mentioned in the first part of this paper, viz., the hemorrhagic diathesis, will also excuse me from dwelling upon the treatment of that mysterious condition which afflicts some unfortunate individuals belonging to our race.

Discussion on Dr. Byford's Paper.

After the reading of the preceding paper, Dr. William Goodell, of Philadelphia, said:—I did not wish to begin the discussion, but as others are backward I will try to start the ball. I was not able to hear all that Dr. Byford said, but some of the points made by him are of extreme interest. I wish to refer to the almost absolute inertness of astringents when given by the mouth in cases of uterine hemorrhage. The text-books laud them, but I have found most of them useless. In my opinion, the canonical pill of acetate of lead and opium would be inert, save for the opium in it, the latter being a valuable hæmostatic. A pure astringent which I have found of much value is gallic acid—not administered in the ordinary dose of five grains, but in doses of from twenty to thirty grains every two hours, or more according to the urgency of the case. I have discovered no bad effects resulting from its use, although I have given half an ounce in the course of a day. It is a bulky and disagreeable remedy, but I mix it with some table-syrup or with molasses, and have found it the only astringent of any efficacy in stopping these hemorrhages. Ergot acts well, especially when used hypodermically, but I know of cases where it has increased the hemorrhage; I do not now refer to cases of fibroid tumor in which it will sometimes so act, but to other cases of metrorrhagia in which I have found it to increase the discharge from the uterus in a way that is inexplicable to me. Free dilatation of the cervix by tents will alone often cure hemorrhage. The causes of uterine hemorrhage are sometimes obscure, and I

have sometimes been humiliated by not discovering them. I have dilated the cervical canal, passed my finger into the uterine cavity, made careful examination, and yet have found no tangible cause. There are other causes beside granular conditions of the lining membrane, which may be so minute as to elude an examination by the finger. When I could find no explainable cause for such hemorrhages, I have treated them empirically, by introducing into the cavity pure nitric acid. I refer particularly to the case of an unmarried lady, in which the hemorrhage lasted for weeks at a time. I fully expected to find a polypus, but did not. The flow was arrested by nitric acid, but it shortly began again; I then used intra-uterine injections of iodine, but the benefit was only temporary. At my request Dr. A. H. Smith also examined the patient, but failed to discover any cause. The hemorrhage finally yielded to ergot, which in the first instance had failed. To this day I am ignorant of the cause of this hemorrhage. I have found the tincture of iodine of use, though it is not infallible. In using it, I first stretch open the os with a dilator, and then inject about a fluidrachm. It has the great advantage of not forming hard clots. There is a plain, homely remedy, which I have successfully used in menorrhagia when others have failed—cinnamon tea, or cinnamon boiled in milk. Heat applied to the spine has also proved of benefit, though not invariably. I am pleased to find that the bichloride of mercury has been referred to by Dr. Byford in high terms. This is a favorite remedy of mine, not only as an alterative, but for its property of increasing the number of red blood-corpuscles. When given for two or three weeks, but not longer, in cases where much flooding has taken place, it renews the blood rapidly, fattens up the patient, and is in my opinion a fine tonic, especially when combined with iron and arsenic.

Dr. J. W. Rosebrugh, of Hamilton, Canada, said:—I think that Dr. Goodell has failed with the acetate of lead because his doses have been much too small. Dr. Workman, of Canada, has given half a drachm at a dose, and claims that with large doses he can stop any hemorrhage at once. I do not, myself, like the remedy, but in cases of extremity it may be effectual.

Dr. Edward M. Hodder, of Toronto, said:—The subject is limited to cases of non-puerperal uterine hemorrhage only. But such doses of acetate of lead as are referred to by Dr. Rosebrugh are only to be used in cases of post-partum hemorrhage. I have employed the tincture of cinnamon largely, and have also used the tincture of cimicifuga, as well as sulphuric acid, but always in conjunction with local treatment. I have injected the perchloride of iron into the uterus until the acid has passed through the Fallopian tubes into the peritoneal cavity, but with only temporary inconvenience. I have also used the persulphate of iron in relaxed and flabby conditions of the uterus. I think that if astringents were used, and the uterus were kept in place, cases of hemorrhage would be more amenable to treatment than is generally supposed.

Dr. Alexander R. Simpson, of Edinburgh, said:—The subject is an important and practical one. It appears, from what I have heard, that uterine cases are much the same on both sides of the Atlantic, and that no one treatment will suit all cases. I have a general impression as to the inutility of astringents, and yet there are cases in which I would use any of them. While agreeing with Dr. Goodell as to the efficiency of gallic acid (which I prefer above all others), and in some cases of sulphuric acid, I wish to mention another agent which often acts admirably as an astringent, and that is the oxide of zinc. I know of cases which have been carried over by one or two-grain doses thrice daily. It is a truth, however, that physicians are often left in the lurch, and have to resort to direct local treatment. The first direct application should be in the shape of vaginal plugging, although I have but little faith in the value of the tampon *per se*. For instance, in some cases the hemorrhagic habit is kept up by the terror and dread of the patient, but the use of the plug by stopping the flow gives her confidence, and there is a corresponding lessening of the menorrhagia. It is difficult at times to apply the tampon so as to make it effective, and I therefore think that plugging the cervical canal i

the better plan, especially as it opens the way for subsequent exploration and treatment. As to direct applications to the uterus, nitric acid is good in some cases, but where a hemorrhage is in progress, I do not find it so effective as the perchloride of iron. In cases of enlargement and of thickening of the mucous lining, it would be important to scrape off the redundant portions by the curette, as recommended by Récamier and Sir J. Y. Simpson. In one case, I simply scraped off the thickened membrane, and, contraction of the womb being kept up by ergot, the patient recovered. The blood at times also gets into such a watery condition as to tend to the continuance of hemorrhage, and then special constitutional treatment is required.

Dr. Thomas W. Gordon, of Georgetown, Ohio, said:—I think very highly of the value of quinia in cases of non-puerperal hemorrhage occurring in malarious districts, or in broken-down constitutions, and think that in such cases it should never be omitted.

Dr. J. A. Campbell, of Grafton, West Virginia, said:—I believe with Dr. Gordon in such a thing as an intermittent uterine hemorrhage, and know that in malarious sections of the country we could not treat hemorrhage without quinia. In most cases, however, the hemorrhage is dependent upon congestion of a displaced womb. The circulation of the blood being prevented by prolapsus, congestion arises, engorgement follows, and then there is a hemorrhage at every recurrence of the menstrual period. I have in my practice insisted upon nightly replacement of the organ by the patient herself, for no physician could attend to this, unless he had but the one case; I consider replacement the chief prophylactic for engorgement and the consequent hemorrhage. This replacement can be effected by the woman herself, by first assuming the knee-breast position, and then by either separating the labia with two fingers, or by introducing a short glass tube into the vagina. I have used iced water also with advantage, but of course not during the menstrual period. Or a gallon of water may be injected every four or five hours, not letting the temperature be much below 60°. In the water I sometimes dissolve sixty grains of the sulphate of iron. The fluid extract of ergot I often give hypodermically, or internally in doses of thirty or forty drops. Dilute sulphuric acid is a favorite remedy with me; I have known women, who from continued hemorrhage have become debilitated, and whose menstrual flow has been so lengthened in duration that the interval between each period has been reduced to a very few days. In these patients, by examination with the speculum, I have found polypoid granulations; but a few applications of nitrate of silver have changed the entire aspect of the case.

Dr. Thomas F. Rochester, of Buffalo, said:—Quinia should be used in all cases of chronic hemorrhage, whether under malarious influences or not, but I am not in favor of full doses; quinia acts upon the capillaries, and thus exerts a good effect. I do not therefore think that its use should be confined to cases of malarious hemorrhage alone.

Dr. Edward H. Trenholme, of Montreal, said:—There are two modes of treatment, the local and the constitutional. The former is adapted to check the actual flow when the patient is in imminent danger. I employ "Sims's" iron cotton, prepared in proper-sized plugs. As a constitutional remedy, where it was difficult to find the cause, I have found oxide of silver, in combination with opium, very efficient.

Dr. B. F. Sherman, of Ogdensburgh, N. Y., said:—I can also recommend the styptic cotton for use as a tampon, and, in granulations, a saturated solution of permanganate of potassium. In obscure cases I give internally one grain of the oxide of silver thrice daily, combined with one-quarter of a grain of opium.

Dr. Charles Shepard, of Grand Rapids, Michigan, said:—While I am a warm advocate of the use of quinia, I desire to laud a mode of local treatment which I employ. I use an elastic rubber bag into which I inject iced water.

This is carried up through the cervix, and it applies itself so closely to the bleeding surface as to be very efficient in stopping the hemorrhage.

Dr. H. D. VOSBURGH, of Lyons, N. Y., said:—Gallic acid is, I think, the only astringent from which much benefit can be derived in uterine hemorrhage. All physicians have pet remedies, and pet plans of applying them. I have found the ordinary solution of persulphate of iron effective; I inject it into the uterine cavity by means of a glass tube with a rubber bag at one end, and cannot recall a case of non-puerperal hemorrhage which has not yielded to this remedy.

Dr. ROBERT BURNS, of Philadelphia, said:—In an experience of forty years, I have seen many cases of this character. In my early practice, when the speculum was hardly known, my dependence was upon large doses of astringents, which often disappointed me. But since the discovery of the speculum I have much less trouble. My system is a simple one, and varies but little. I first attend to the state of the constitution—if the patient is weak, giving her tincture of chloride of iron, quinia, and aromatic sulphuric acid. I then use a combination of a drachm each of tannic acid and sulphate of zinc, in an ounce of glycerine, which I apply to the interior of the uterus, through the previously dilated cervical canal, using cotton saturated with the mixture. I repeat this several times, and brush the cervix with the same, putting the patient on gentle purgative treatment. There are cases of extreme hemorrhage where ordinary methods fail, but most of them are amenable to this mode of treatment.

Dr. THEOPHILUS PARVIN, of Indianapolis, said:—We must all agree, from the discussion and from the number of remedies that have been suggested, that the uterus is an organ which can be abused with impunity. It has been said that we must find the cause of the hemorrhage: true, but in the mean time one's patient may bleed to death. Stop the hemorrhage first, and find out the cause afterwards. There is one method which has not been referred to, and that is vaginal injections of hot water. This is a first-class remedy. Hot-water rubber bags, applied to the back, have also been found efficacious. The difficulty of local treatment is often due to the fact that the physician shrinks from the examination of an unmarried woman; but desperate cases require modesty and shame to be put aside. The remedies spoken of may be empirical, but they are the best in such cases. As to local treatment, there is often some disorder with the parts. Some practitioners use the curette, while others say that it is murder to do so. Mucous polypi in the cervical canal are irritants, but are not the source of the hemorrhage. They cause an increased flow of blood to the parts that are irritated by their presence, and there is a solution of continuity as the result. Remove these polypi, and then apply tincture of iron. I thought that I had made a discovery when I used a small forceps instead of the curette for the purpose of pinching off small granulations, but I have found that others had used them before me. The hemorrhage is not an entity in itself, but merely the expression of something else. We cannot cure the patient without finding out the cause, which, in doubtful cases, I believe is often an inflammation of the lining membrane of the uterus.

Dr. S. S. TODD, of Kansas City, Mo., said:—I wish to remove the subsulphate of iron from the list of injurious remedies. I used it shortly after its introduction by Dr. Barnes, and have reported eight cases, in all of which it was successful, though the hemorrhages were all dangerous. I believe that many lives have been saved by its use. I desire to reiterate that the subsulphate of iron is absolutely innocuous.

The President, Dr. ROBERT BARNES, of London, said:—I have been much impressed with the great advantage of these meetings; the members of the Section have heard a variety of expressions from all parts of this great continent, and every expression of individual opinion has embodied much that is valuable. I have received several new ideas while the discussion on this interesting question has been going on. In London, we know but little of

malarial hemorrhages; but we have found quinia a valuable means of controlling bleeding, as it contracts the uterus, and is otherwise of service to the system. I always use it in cases of hemorrhage from subinvolution, and do not think a merely local treatment all that is needful in such a condition. I have noticed the law of habit mentioned by Dr. Simpson, for hemorrhages will sometimes take place when the uterus is apparently healthy, not only independently of ovulation, but often after the climacteric period. These hemorrhages are largely attributable to regional attraction of blood to the uterus, for that organ is liable to regional hemorrhage, even when there is no disease of a local character. The treatment must be local or general, and sometimes it is necessary to resort to both. Topically, the best plan is to dilate the cervix and inject the perchloride of iron. Astringents by the mouth are of use at times, and, of these, I think with Dr. Goodell that gallic acid is the best. The heart-action often has an effect on the hemorrhage, and by using aconite and other remedies to reduce cardiac action, there will be a corresponding reduction in the flow of blood. In some cases in which the cause of hemorrhage could not be ascertained, I have used, with excellent results, the witch-hazel, which I believe to be a native of this country. Although the curette has undoubtedly been of service in many cases, yet I condemn its use, for it is often followed by furious bleeding, and the growth of small malignant excrescences is more rapid after using it. Besides, it is, at best, a dangerous instrument, and requires to be handled with the greatest care. In one year I lost two patients from its use.

Dr. Byford, in reply, said:—Quinia is only second to ergot in causing uterine contractions, and is an admirable remedy. I have known of miasmatic conditions producing hemorrhage in non-puerperal cases. Quinia is of use in these cases, especially in causing condensation of the uterus. In the vast majority of cases, hemorrhage from a non-puerperal womb is owing to one of two conditions, either hyperæmia or a vitiated state of the uterine mucous lining, from inflammation or from growths. For the former condition I use ergot and local remedies, such as the tincture of iodine, but not as intra-uterine injections. In my experience, uterine hyperæmia and hypertrophy need the application of a strong astringent to the neck of the womb alone, for the body of the uterus can be made to contract by applications to the external and internal surfaces of the cervix. Local remedies can be applied in many different ways. The other common cause of hemorrhage is a disorganization of the lining membrane of the uterus. Hemorrhage becomes more easy when this mucous coat is not firm, because it gives way more readily. Topical treatment is here indispensable. When bleeding has occurred after the menopause from a determination of blood to the womb, I have been able to do more with large doses of quinia and iron than with ergot or with anything else. I do not consider ergot particularly valuable in such cases. The vaginal plug is of great use when used properly and thoroughly. It should not simply be pressed against the os, but firmly packed in front and behind the cervix, in order to compress it. I have, in my own practice, been rather timid in regard to injecting fluids into the womb, probably too much so, but I avoid such treatment wherever it is possible to do so.

THE MECHANISM OF NATURAL AND OF ARTIFICIAL LABOR IN NARROW PELVES.

BY

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"Opinionum commenta delet dies, naturæ judicia confirmat."

CICERO, *De Naturâ Deorum.*

In studying the mechanism of natural and of artificial labor in narrow pelves, let us first inquire what is meant by a narrow pelvis, and what are its kinds.

A pelvis with a true conjugate (*conjugata vera*) measuring less than four inches constitutes a narrow pelvis. For, although the biparietal diameter of the average fœtal head at term measures less than four inches, the working space between the promontory of the sacrum and the symphysis of the pubis is lessened by double the thickness of the uterine wall, and by double that of the vesical wall. While, therefore, the actual conjugate is ample for an average head to pass, the virtual conjugate may be too small. Thus, according to our best authorities, a standard head in a pelvis measuring in its conjugate 3.75 inches—viz., half an inch less than that of the average-sized pelvis—constitutes a difficult labor.

A spontaneous delivery is not impossible under such circumstances, but the forceps will usually be needed; sometimes, indeed, craniotomy. Every line below this measurement greatly increases the difficulties of parturition. For instance, out of thirteen vertex presentations in pelves measuring at the least 3.75 inches, Dubois had three cases of craniotomy or of cephalotripsy. In conjugate diameters between 3.75 and 3.1 inches, he had ten presentations at term of the vertex, and one of the face. Of these, two ended spontaneously, two were delivered by the forceps with the death of one child, and seven needed the cephalotribe.[1] Baudelocque contends that out of five hundred children at term and of average size, hardly one will be delivered alive through a pelvis of three inches. To this, Dezeimeris[2] adds that one of 3.25 inches does not give much better results, and that one of 3.5 inches also offers to the child a very dangerous passage. Joseph Clarke gives,[3] as the result of an experience of 14,077 cases of labor, that "three and a half inches is the least diameter" through

[1] Management of Labor in Contracted Pelves. By Wm. H. Jones. This author, it is true, makes these cases occur respectively in pelves whose conjugates measured "over 3.5," and "between 3.5 and 3 inches." But since he has adopted the classification of Dubois, these figures seem to me incorrect. For Dubois's first and second categories of deformed pelves are those in which the conjugates measure respectively, at the least, 3 75 inches (au moins 9.5 centimètres), and between 3.75 and 3.1 inches (au plus 9.5 centimètres et 8 centimètres au moins). This information I get second-hand from Cazeaux, and not from Dubois, whose thesis is beyond my reach.

[2] Dictionnaire en 30; Art. Accouchements Prématurées, p. 427.

[3] Contributions to Midwifery; by T. E. Beatty, p. 23.

which he has known "a full-grown fœtus to pass entire." Chailly-Honoré says,[1] "The forceps will frequently extract a living child through a conjugate of over nine centimetres (3.54 inches), sometimes, indeed—although this is very rare—under this size." From these facts, the broad rule may be laid down that, when the short diameter of the brim does not exceed the biparietal diameter of the child's head, the unaided efforts of the mother will in general be inadequate to effect a safe delivery, and that even with instrumental aid the life of the child will be greatly imperilled.

The most common kinds of narrow pelvis are three in number:—

(1) The simple, flat pelvis, or conjugate narrowing with correlative transverse widening.

(2) The generally and uniformly narrowed pelvis, in which all the pelvic diameters are symmetrically shortened.

(3) The generally narrowed, flat pelvis, which combines the bone-lesions of the other two, but in varying proportions.

Of these three varieties, the one most commonly met with is the simple, flat pelvis, in which the lesion is limited mainly to the brim and to its posterior half. Practically the obstruction is a marginal one, and is confined to the sacral pole of the conjugate diameter. In the second variety—or the generally and uniformly narrowed pelvis—the obstruction is not a marginal one, nor of limited area, but diffused over every plane of the pelvic canal. In the third variety, the obstruction depends upon the preponderance of conjugate or of transverse narrowing, and is, therefore, either mainly at the brim and marginal, or mainly in the pelvic canal and diffused.

Since "Art is the imitation of Nature, and those will best succeed in any science who closest watch her operations,"[2] let us next note the mechanism of an unaided head-first labor in these different kinds of narrow pelvis.

In the generally and uniformly narrowed pelvis, the mechanism is very analogous to that which takes place in the standard pelvis, but is of course attended with more delay. The head enters the brim synclitically and with the normal Solayrés obliquity—that is to say it engages in one of the oblique diameters of the brim, but without lateral (Nægele's) obliquity. It will also be strongly flexed (Rœderer's obliquity), because the resistance at the brim is equal on all sides, and the shorter hind-arm of the head-lever must therefore descend. The head also begins to flex at a higher plane of the pelvic cavity than in the standard pelvis. For flexion in the latter is chiefly brought about by the resistance of the os uteri; in the former by the more highly situated brim. During the descent of the head, the synclitic movement is preserved except in so far as vertical or lateral shearing interferes with it. Since the absolute conjugate narrowing is rarely great, and the general narrowing is symmetrical in all directions, the furrows or the fractures of the cranium are by no means so common as in the other varieties of deformed pelvis. But, when present, they usually start from the boss of the parietal bone and run more or less parallel to the sagittal suture, as in cases of fracture reported by Lizé[3] and D'Outrepont,[4] and one of depression of the skull by Danyau.[5] It must, however, be borne in mind that, while the bone-furrow always

[1] Accouchements; Paris, 1867, p. 649.
[2] The Present State of Midwifery in Paris; by A. Tolver, 1770, p. 22.
[3] Union Médicale, Fév. 1860, p. 295.
[4] Journal de Chirurgie, par Malgaigne, 1843, p. 41.
[5] Ibid., p. 49.

indicates the transit-line, the fractures do not, as they may radiate from it in every direction.

When left to the unaided efforts of nature, the typical mechanism of a head-first labor in the flat pelvis, or in one whose brim is narrowed mainly in its conjugate diameter, is as follows:—The hind-head offers its biparietal diameter at the conjugate, but cannot enter. The resistance being nearer to the occipital end of the head causes extension, and the fore-head dips. As the fore-head descends, the sagittal suture, losing its Solayrés and Rœderer obliquities, becomes transverse in two senses—viz., it lies in the transverse diameter of the brim, and with the two fontanelles on the same plane. The shorter and more compressible bitemporal diameter—that is to say, the upper or vault portion of each temporal region near the coronal suture—reaches the conjugate, and is driven into it, by the changed direction of the propelling force, which, acting through the vertebral column, is now thrown out of its perpendicularity to the base of the skull, and so bears at an angle directed to the forehead. Thus the head is nipped, bent in on its sacral side, and moulded to the promontory. Sometimes it is the bicoronal (the bifronto-temporal) diameter which is nipped. Hence the frequency of face-presentations in narrow pelves—a frequency, according to Michaelis, eight times greater than in normal pelves.[1] Hence, also, the ease with which the anterior fontanelle is reached, and the corresponding difficulty with which the finger touches the posterior fontanelle.

The resistance is now at a point nearer to the fore-head than to the hind-head, and the latter, therefore, begins to descend, but with the biparietal diameter *to one side* of the conjugate diameter. But the resistance of the iliac margin of the brim to the descent of the hind-head causes that portion of the cranium to expand and to mould itself to the corresponding sacro-iliac space. It also tends to shove the whole head over to the forehead side of the pelvis. In a generally narrowed, flat pelvis with absolute transverse shortening, this displacement in the direction of the occipito-frontal diameter may go on until stopped by the impact of the forehead on the opposite ilium. The head, therefore, passes the conjugate, not vertically in the bicoronal or the bitemporal diameter which first engaged, but along a transit-furrow running somewhat obliquely from that portion of the vault near the coronal suture to the base posteriorly, that is to say, in the resultant of three forces—flexion, uterine propulsion, and occipito-frontal displacement in the transverse (bisiliac) diameter of the brim. In proportion as the head descends, it becomes more and more flexed, and the biparietal diameter, which passed the brim to one side of the conjugate, will, at a later stage of labor, be found almost directly under the promontory. It must, however, be remembered that, since all the transverse diameters of the pelvic cavity are also lengthened out, the head usually descends to the floor of the pelvis with less flexion than in the normal pelvis, and anterior rotation of the vertex is proportionally delayed.

Thus, from the mechanism of natural labor in this kind of pelvis, it will be seen that the head enters the brim in a state of partial extension, or at least in a state midway between flexion and extension, and passes the short pelvic diameter by a short and most compressible cranial diameter. Further, the head usually passes the brim with also a lateral obliquity. Before the engagement of the head, the sagittal suture in a

[1] Clinical Lectures by German Authors, New Sydenham Society, 1876, p. 302.

typical case will be found very close to the promontory. To engage, the head must cant over the edge of the promontory; and during the first stage of descent this suture approaches the pubis. Later on, it begins to return towards the sacrum. This last movement is owing partly to the moulding of the posterior side of the head, and partly to the greater resistance to descent at the sacro-vertebral angle, while the pubic margin of the brim offers a smooth and glib surface. The head, therefore, pivots on the promontory as a centre of motion, and, as it were, rolls over into the pelvic cavity. It "doubles the promontory," as our honored President, Dr. Barnes, so happily expresses it.

After such an unaided delivery, or after an artificial delivery in which the application of the forceps is delayed until the head has been driven past the brim, the cranium exhibits two very characteristic shears. In the one, there is a bending in of the sacral side of the head, and a bulging out of the pubic side—it is kidney-shaped. In the other, an equitation of one parietal bone over its fellow—usually the anterior over the posterior. The site of the pressure-furrow, or of the bone-lesion, varies with the size and the position of the head, and with the shape of the pelvis. It is not found at one of the poles of the biparietal diameter, and more or less parallel to the sagittal suture, as in the uniformly narrowed pelvis in which the vertex dips *ab initio*, but at the forepart of one parietal bone, close to and more or less parallel to the coronal suture. Often it lies directly over this suture, overlapping both frontal and parietal bones, and sometimes on the frontal bone alone.

When the disproportion between the size of the head and that of the brim is relatively great, or when the conjugate is short and the head unduly ossified or bullet-shaped, engagement is often delayed. Under such conditions the head does not at once become fixed by the preliminary dip of the anterior fontanelle, but it coys at the brink of the brim without entering. Perched on the sacral shelf, it swings backward and forward, and see-saws with every fœtal movement. The finger will find the sagittal suture usually close to the sacrum, and the posterior fontanelle now in front and now behind the transverse diameter of the brim; now dipping into the inlet, anon tilting up out of it. It is momentarily fixed by each uterine contraction, and then is the time to take the position. This cranial play usually ends before long, and that by the adjustment and moulding of the bicoronal region to the conjugate, and by its consequent engagement and descent. And this may be hastened, as the late Edward Martin has shown,[1] by turning the woman over on to the side towards which the forehead looks. The breech of the child then sags over to the same side, the hind-head is lifted out of the brim by the leverage of the spinal column, and the fore-head sinks in more deeply. Whenever this dipping of the fore-head does not happen, delivery without artificial aid will be indefinitely delayed.

In the foregoing description I do not pretend to have labelled all the natural processes involved, nor to have codified every law which governs the cranial movements in narrow pelves. Nor do I contend that the anterior fontanelle always dips, for the head may be a small one, or the cranium a yielding one, or other disturbing elements may intervene. But from a study of the writings of Schrœder, Spiegelberg, Martin, and Otto de Haselberg, and also from patient bedside observation, I believe that the above mechanism is in the main correct—it certainly best ex-

[1] Prager Vierteljahrsschrift, 1868, Band ii., Analect, S. 76.

plains the behavior of the head. Since, however, the truth is not always truthlike, and since the flexion of the head with the dip of the hind-head has hitherto been taught as the mode of engagement, a few more words on this point are needful.

If, in a pelvis narrowed mainly in its conjugate diameter, the hind-head always dips, and the vertex first enters the brim, as contended by Hodge and by many others, what explanation can be offered for the indisputable fact that the anterior fontanelle, at the outset of labor, is so commonly on the same plane with the posterior fontanelle, or even on a lower plane? How interpret the well-known frequency of face presentations in narrow pelves? Again, if it can be shown, that, after such unaided labors the pressure-marks or other coarse cranial lesions are found, not at one of the poles of the biparietal diameter, which is alleged to be the implicated diameter, but, as I have asserted, at the fore part of the head somewhere near the coronal suture, then the burden of reconciling this fact to theory must rest on those who hold to the initial dip of the posterior fontanelle.

Let me summon to the stand impartial witnesses, such as had no pet theories to which their facts were trimmed. And, in these forceps days, I own to some difficulty in finding cases of labor in narrow pelves which were either neglected, as we should now call it, or left entirely to the unaided efforts of nature. Nor are the following examples noting the pressure-mark, selected ones; but the *only* ones which I have been able to collect within a short and busy time.

In describing a natural labor in a pelvis whose conjugate measured about three inches and a quarter, Mlle. Anna Puéjac, midwife in chief of the Maternité at Montpellier, says[1] that "At the *anterior* angle of the left parietal bone, there was a very considerable depression, sufficient to admit the half of an ordinary egg. It was the exact cast of the promontory." Smellie writes[2] that in a case of narrow pelvis he waited until the head was "squeezed down to the lower part of the pelvis"—that is, had passed the brim. The pains failing, he then applied the forceps along the sides of the pelvis, and caught the head, which was still *transverse*, over the face and occiput. "The child's head was squeezed into a longitudinal form, flattened on the sides, with a deep impression on the cranium *above the ear*." Again he tells us that, after a woman, who was "very much distorted," "had been in strong labor for four-and-twenty hours," he found "the head down to the lower part of the pelvis," viz., past the brim, and applied the forceps first over the face and the occiput, and then, when the perinæum began to bulge, over the sides of the head. "The head was of a lengthened form, and contorted to one side, and there was a deep impression *above the ear*."

Lachapelle contends, as the result of close observation, that "the head of the fœtus, driven through a narrow and solid pelvis, is fractured and indented near one of the frontal bones. This accident is not uncommon, and we shall cite many examples of it."[3] Again, she writes, "I have remarked with Smellie (tome ii. p. 533) that, in cases of natural labors in pelves *considerably* distorted, the depression is no longer found on the frontal region (*sur le front*), but near the temporal region."[4] Out of a number of cases reported by her I select the following: Case I. Occiput

[1] Gazette Obstétricale, Juin 20, 1876, p. 179.
[2] Collection of Cases, 1778, Collection xxvii., No. 2, Cases iii. and iv.
[3] Eleventh Memoir, p. 423. [4] Ibid., p. 429.

to the left; the anterior fontanelle *was easier to reach than usual.* A dead child was born naturally; it had the frontal boss indented and broken.[1] In another case,[2] the occiput was to the right; conjugate 3.25 English inches; a dead child was born naturally after a long labor. "The *right frontal bone*, although without fracture, offered a very marked depression which was undoubtedly due to the jutting promontory." In a third case the conjugate measured 3.25 English inches; position of the child's head not known; after six and twenty hours of labor the forceps was applied along the sides of the pelvis; "extraction prompt and easy." The left *frontal and parietal* bones were found fractured.[3] A fourth case[4] was a primipara, with a conjugate of about 3.5 English inches, and with a second position of the vertex. The forceps was applied to the sides of the head, but failed to extract; the blades were, therefore, withdrawn and applied in each ilium. There was now no further delay in delivery. The child weighed five pounds, and died twelve hours after birth. "The right *frontal* was depressed and fractured by the sacral promontory." In a fifth case,[5] the conjugate measured 3.25 English inches. The occiput looked to the left ilium; the sagittal suture lay transversely and on the sacro-vertebral angle, with the right parietal affronting the inlet.

Siebold describes a typical case of unaided labor in a pelvis of 3.5 inches.[6] At the first labor, a dead child was delivered by the forceps. At the second labor, the head presented transversely, with the occiput to the right ilium. By means of ergot a dead child was expelled. It had lesions and furrows at both temporal regions. At the third labor, the head presented transversely "with the small fontanelle to the left and the large to the right." "It was also *greatly extended* to the right," viz., the forehead dipped. After many hours of hard labor, the head was driven past the promontory, and then the vertex rotated anteriorly to the first position. Thereafter the child was soon delivered. It weighed seven pounds, was dead, and had the left temple denuded of its cuticle, at a point on a level with the small anterior inferior fontanelle (Casserius's fontanelle). At the autopsy, four fissures were found; one on the left side of the coronal suture (on the frontal bone), and three on the parietal bone. In the same journal, Carus reports an analogous case, in which there was a furrow of the parietal and a fracture of the frontal bone. Schöller, of Berlin,[7] relates a unique case of labor in a primipara with a narrow pelvis. At the end of three days the woman was delivered of a living child. "In the middle of the left parietal, and in the neighborhood of the left temple, the skin was abraded and the bone depressed." "The bone beneath died, and a portion as large as a sixpence of the whole thickness of the parietal bone exfoliated, leaving the dura mater exposed. The destruction of the frontal bone was less considerable." The Transactions of the Medical Society of Erlangen contain the history of a case of spontaneous delivery in a narrow pelvis, reported by W. J. Schmidt.[8] The child was still; its skull had a fracture and a depression on the left frontal region. In the same paper analogous examples are given by H. A. Hirt and Jörg. Another case of natural labor, in a pelvis measuring

[1] Fourth Memoir, p. 154.
[2] Eighth Memoir, p. 189.
[3] Eleventh Memoir, p. 475.
[4] Ibid., p. 469.
[5] Ibid., No. i. p. 463.
[6] Revue Médicale, 1833, tome i. p. 301; from Siebold's Journal für Geburtshülfe, B. xi. S. 404.
[7] Berlin. Medicinische Zeitung, September, 1841.
[8] Jörg, Schriften zur Kentniss des Weibes, Bd. ii. S. 123, 130.

about three inches, is given by Osiander.[1] After the most violent expulsive pains a dead child, weighing six and a half pounds, was born, with fracture of the left parietal and frontal bones. Danyau, whose excellent paper[2] has furnished me with some of these references, gives the following remarkable history of a woman whose pelvis was found after death to measure 2.75 French inches: Her first child was delivered dead by the forceps. In five succeeding labors each child offered by the breech, and one was born alive. In her seventh and eighth labors the head presented, and the children were born without aid, but dead. In her ninth labor she was brought to the Maternité. The head came first, and for four hours stayed locked at the brim. Its sacral side was then felt suddenly to bend in, and the same pain drove it down to the vulva. The child was dead. Its skull had a deep depression overlapping the left coronal suture, and shelving obliquely backward to the boss. The death of the mother on the fourth day permitted a careful measurement of the pelvic diameters.

Levret[3] writes of a natural labor in a narrow pelvis in which the "face-half" of the head was obliquely furrowed. Antoine Dugés contends,[4] that "The bones of the skull are often fractured with depression by the sacro-vertebral angle of the mother. It is one of the frontal, or, indeed, one of the temporal regions, which is the most ordinary site of these lesions." As a voucher for this statement, he gives an example of a living child born spontaneously, after a labor of thirty-six hours, through a pelvis measuring 3.25 English inches. The occiput presented to the right, and the right temple showed a red and contused furrow.[5] In another case, the left frontal bone was so depressed as to push the corresponding eye nearly out of its socket. Velpeau declares,[6] that "the depression of the parietal or of the frontal, with or without fracture, has been observed at the Maternité of Paris." Budin, a most careful observer, met with a case of spontaneous delivery in a flat pelvis measuring about 3.4 English inches in its conjugate.[7] The occiput presented to the left, and a small living child was born twenty hours after the rupture of the membranes. On the left frontal region it had a deep furrow, which started from the coronal suture at the anterior fontanelle, and ran obliquely backward toward the base of the skull. Chailly-Honoré holds[8] that "The depressions of the skull have their site ordinarily on the frontal or the parietal bones," and are often the result of natural labor in a reniform pelvis. Casper, after giving a list of writers on this subject, whose works are unfortunately beyond my reach, says that the skull-fractures caused by unaided labor are commonly on the parietal bone "and usually *transversely* to the sagittal suture." Sometimes they are found "stretching from the frontal bone more or less parallel to the sagittal suture."[9] From these citations, it follows that the site of the bone-lesions caused by a natural head-first labor in a flat pelvis, is found, not at one of the poles of the biparietal diameter, but either directly over the

[1] Handbuch der Entbindungskunst, Bd. ii. Abth. 2, S. 206.
[2] Journal de Chirurgie, par Malgaigne, Janvier, 1843, p. 42.
[3] Accouchements Laborieux, Paris, 1762, p. 119.
[4] Manuel d'Obstétrique, Montpellier, 1840, part v. sect. i. p. 306.
[5] Thèse, No. 64, 1821, pp. 51, 54.
[6] Accouchements, Paris, 1835, tome ii. p. 588.
[7] De la Tête du Fœtus, Paris, 1876, p. 58.
[8] Accouchements, Paris, 1867, p. 950.
[9] Forensic Medicine, New Sydenham Society, vol. iii. p. 118.

track of the coronal suture, or on either side of it, and generally parallel to it, and at *right angles* to the sagittal suture.[1]

After turning in a flat pelvis, or in one narrowed mainly in its conjugate diameter, a very analogous cranial mechanism takes place. The partial extension of the wedge-shaped head throws the biparietal diameter to one side of the promontory, and places in the conjugate of the brim the small bisauricular (bimastoid) diameter—viz., that portion of each temporal region lying directly in front of the ear. Now, upon traction, if the occipito-frontal diameter of the head be short, or the occipital portion of the pelvis roomy, the head will pass in the transverse (bis-iliac) diameter of the brim, and in a state midway between flexion and extension, while the transit-furrow will be vertical, running directly upward from the smaller bisauricular diameter to the larger bitemporal diameter, close to and parallel to the coronal suture. If the head be large or the brim shortened in its transverse diameter, the resistance to the descent of the hind-head tends to bring about two movements; the one of flexion, and the other of occipito-frontal displacement towards the forehead side of the pelvis. The head should, therefore, pass in the resultant of these forces and of traction; viz., in a line running somewhat obliquely from the front of the ear, or from the anterior inferior angle of the parietal bone, to the vault posteriorly. But this transit-line is more vertical than in the given case of unaided head-first labor, being very generally found very near and almost parallel to the coronal suture, sometimes indeed in the very gutter of this suture-track. For the sacral side of the head being, under traction and propulsion, quickly bent in and fixed by the promontory, resists occipito-frontal displacement; while free lateral bulging at uncompressed points prevents any exaggerated lengthening out of the occipito-frontal diameter. Furthermore, suprapubic propulsion together with two movements of unremitting traction, first in the axis of the outlet and then in that of the inlet, cants the base of the skull over the edge of the promontory, causes its sacral side to be nipped high up, and makes its pubic side revolve around the promontory as a centre of motion, and descend over the glib under-surface of the pubis; in other words the head is, as I have elsewhere expressed it, warped around the promontory.[2]

Now, since this mechanism of a head-last labor in a narrow pelvis is yet mooted, I shall try to prove it by such clinical histories as note the site of the pressure-furrow. Smellie,[3] after turning in a narrow pelvis, and making traction on the child's body "with all my strength," delivered a living child, which did well, although it had "a depression of the temporal bone *before* the ear, and the frontal and parietal bones pushed outwards." Madame Lachapelle[4] advises turning in narrow pelves in

[1] In order to facilitate the work of others in this direction, I append the following references to papers on fractures of the fœtal cranium, which I have not been able to consult: Procés verbal de la distribution des prix, June 20, p. 64, et seq.; London Medical and Surgical Journal, March, 1834, p. 145; Charles West, in Medico-Chirurgical Transactions, 2d series, vol. x. 1845, p. 404; Siebold's Journal für Geburtshülfe, 1825, B. v. S. 224, B. xi. S. 400; C. F. Hedinger, über die Knochenverletzungen bei Neugebornen in Med.-gericht Hinsicht, Leipzig, 1833. The following authors have also written on the same subject: Chaussier, Meissner, Siegel, Ollivier, D'Angers, Viardel, Haller, Bosc, Berchols, Deventer, Dionis, Roederer, Baudelocque, and Stein.

[2] Philadelphia Medical Times, March 20, 1875, p. 386.

[3] Cases in Midwifery, Collect. xxxiv., No. ii., Case ii., p. 237.

[4] Pratique des Accouchements, Paris, 1825, Eleventh Memoir, p. 429.

order to make the head descend midway between flexion and extension, so as to get the "temporo-auricular diameter" in the conjugate. From several cases of head-last labor in narrow pelves, which she narrates, I select the following: Case I.[1] Primipara; conjugate 2.75 French inches; vertex presentation; forceps slipped; prolapse of the cord; version; head extracted with difficulty and with a sudden jerk (*secousse brusque*). A large girl was born alive, but did not live long. The head had a deep depression on the left frontal bone. Case II.[2] Second position of breech; conjugate 2.5 French inches; child weighed five pounds and had been dead for some time. The head had "a deep depression of the right parietal in its anterior portion and near the temple, caused by the sacral promontory." Case III.[3] First position of breech; "For more than ten minutes the head resisted the most violent tractions" (*aux efforts les plus violens*). Child did well for twenty-four hours and then died in convulsions, from the cranial lesions. An autopsy revealed the separation of the right parietal bone from the temporal along its whole inferior border.

J. J. Phillips[4] reports three cases of successful version in contracted pelves after failure with the forceps. In one of them the child had a "marked depression on the left parietal and frontal bones." Here the transit-line followed the course of the coronal suture. Carl Ruge[5] gives six cases of fracture of the frontal bone, and one along the temporo-parietal suture, after version. In the seven of my own cases[6] in which the pressure-furrow is noted, the site was near or on the coronal suture. The late Prof. E. Martin, of Berlin, says[7] that, after version, "These impressions or depressions of the respective parietal bone are *always* to be found upon its anterior portion, more or less near the coronal suture." Dr. R. Barnes avers[8] that "The mark of pressure or indentation against the jutting promontory is *always* seen at one end of this short diameter:" viz., the bitemporo-frontal. Dr. I. E. Taylor, of New York, in describing a case of labor in a pelvis measuring 2.5 inches in its conjugate diameter, states that "By version and propulsion from above the pubes, the head was flattened or indented at the junction of the parietal and frontal bones, which is the usual place."[9] Many other examples could be cited, but further evidence is needless.

From this mechanism it is clear that version closely imitates the processes of nature, for by it the head enters and passes the conjugate by its shortest diameter, where four yielding bones meet; where four wide sutures converge; where the fontanelle is large and always open; where overlapping is most free, and where compressibility is greatest. Since the resistance is limited mainly to the margin of the brim, that once passed (and generally with a well-marked jerk), there is usually no further obstacle to the descent of the head. It is now brought into relation with new pelvic diameters, and the greater friction of the broader and harder surface of the hind-head brings about the needful movements of flexion and of rotation. To effect this the line of traction must be changed to that of Carus's curve, and finally to one at right angles to the mother's body.

[1] Op. cit., Eleventh Memoir, p. 481. [2] Ibid., p. 486.
[3] Ibid., Fourth Memoir, p. 157. [4] Lancet, March, 1871, p. 404.
[5] Bulletin Général de Thérap., Août 15, 1875, p. 130.
[6] American Journal of Obstetrics, August, 1875, p. 197.
[7] Richmond and Louisville Medical Journal, April, 1870, p. 421.
[8] Obstetric Operations, London, 1871, chap. xvi., p. 237.
[9] Transactions of the New York Academy of Medicine, September, 1875.

But there are mechanical advantages which turning possesses over a cephalic presentation in a narrow pelvis. Outside help, that of traction and of propulsion, can be invoked without in the slightest degree interfering with th natural mechanism. Again—and this must not be overlooked—by turning, the head enters the brim by the very short bis-auricular (bimastoid) diameter of its base, and passes the conjugate by the larger but more compressible bitemporal diameter of its vault; that is to say, the small end of the wedge is first nipped. In the corresponding head-first labor, the reverse to this takes place; for the head enters the conjugate by its bitemporal, or the vault portion of its bicoronal diameter. From repeated measurements taken shortly after birth, I find that the average length of the bisauricular diameter is 2.6 inches, and that of the bitemporal diameter 3 inches—a difference of nearly half an inch.

From this description of the cranial movements, it follows that turning should be limited to cases of antero-posterior shortening of the brim. For, in other kinds of narrowing, the previously described mechanism, so essential to success, could not take place. In the generally and uniformly narrowed pelvis, the transverse (bisiliac) diameter of the brim would probably be too short to permit the after-coming head to pass in a state midway between flexion and extension; while, on the other hand, if the head were forcibly flexed, a longer diameter than the bitemporal would be jammed into the short diameter of the brim. Again, from the narrowing of all the diameters of the whole pelvis—brim, bony canal, and outlet—the after-coming head would be detained at every stage of its descent, and could not, therefore, be delivered soon enough to avert fatal asphyxia. To save the life of the child, then, the forceps, and not turning, is here needed. As a corollary to this, less power and time are needed after version to deliver an average head in a flat pelvis, than a large head in an average pelvis; for, in the former, the resistance is marginal, and limited to a single osseous point; in the latter, diffused more or less, not only over the whole bony brim, but over the pelvic canal and outlet.

How to tell a uniformly narrowed pelvis from one narrowed mainly in its short diameter, becomes, then, an important question. Apart from careful digital examinations, much may be learned from measuring the distance between the anterior-superior spinous processes and that between the crests of the ilia. In a standard pelvis these distances average respectively 25 cent. (9.8 inches) and 28 cent. (11 inches).[1] But what should mostly be relied on is the position of the head. When conjugate narrowing alone exists, or when it preponderates, the occipito-frontal diameter lies parallel to the transverse diameter of the brim, with the anterior fontanelle low down. A head so situated implies conjugate narrowing and transverse amplitude of the brim. When the pelvis is uniformly narrowed, or when transverse (bisiliac) narrowing preponderates, the head will be found high up and very strongly flexed. This happens because the resistance at the brim is equal on all sides, and the shorter arm of the occipito-frontal lever must therefore descend. The sagittal suture, for the same reason as in a standard pelvis, will be found running more obliquely than transversely. Such cranial conditions show that the transverse diameter of the brim is too short to admit the occipito-

[1] Carl Martin, Monats. für Geburtsk., Dec. 1867.

frontal diameter of the head, and that version is consequently inadmissible.

Turn now to the mechanism of labor when the forceps is used at the brim of a flat pelvis. If the blades are applied to the fronto-occipital, or to the fronto-mastoid diameter, the head, which is lying midway between flexion and extension, is—unless firmly nipped in the conjugate—forcibly flexed, and either the biparietal diameter, or some diameter longer than the bitemporal, is made to enter the short diameter of the brim. Again, the fronto-occipital compression to which the head is subjected, not only prevents the natural lengthening out of the long diameters, but it causes bulging of all the cross diameters. This is, of course, a mechanical disadvantage, one which increases the difficulty of the head's transit, and which can be overcome simply by a brute tractile force—such as puts fœtal life in jeopardy. In addition, the fronto-mastoid application tends to force a premature anterior rotation of the hind-head. Also, the line of traction on the forceps-handles will invariably be at an angle to the axis of the superior strait, and the brunt of the traction is wasted on the pubis. Finally, such an application, by flexing the head or by shortening its fronto-occipital diameter, so widens the iliac spaces that prolapse of the cord is very liable to occur. "Statistics show that out of five applications of the forceps above the superior strait, no fewer than two are complicated by a prolapse of the cord. I," continues Chailly-Honoré, "have met with it in the ratio of twice in three cases."[1]

On the other hand, when the blades are applied along the sides of a head at the brim of a narrow pelvis and lying in its transverse (bisiliac) diameter, flexion is inevitable.[2] So essential, indeed, is this movement for the accurate adjustment of the blades, that some practitioners try

[1] Accouchements, 1867, p. 643.

[2] The statement often appears in foreign medical works, that this application is impracticable whenever the head lies in the transverse diameter of the brim. This is a mistake; for, although the long curved forceps then becomes virtually a straight one in so far as the pelvic curve is concerned, this application can be very generally made with a properly constructed instrument, provided the head be not locked. With the Hodge or the Davis forceps I have frequently made this application, and have repeatedly seen others make it. Hodge and Meigs, it is true, condemned it; but they denied, not its practicability but, its advisability. Baudelocque, Capuron, and Gardien, warmly advocated it, and it is now the habitual practice of some of the best obstetricians of Philadelphia. It possesses the great merits of securing the firmest grip, of compressing the head in its least vulnerable diameter, and of lessening the pressure on the bladder. Since the mode of making this application is peculiar, a description of it may not come amiss. If the occiput look directly to the left ilium—and this is the most common cephalic position in the simple flat pelvis—the woman is turned over on her back, and her coccyx made to project over the edge of the bed. The right (female) branch is first introduced in the right side of the pelvis, with the convex surface of its blade looking obliquely to the sacrum. By a rapid downward sweep and a spiral twist of its handle, together with upward pressure on the convex edge of its blade by the fingers in the vagina, it is rotated very nearly half of a circle over the forehead to the side of the child's head under the pubis. The left (male) branch is now so held at its lower convex edge by the tips of the fingers of the right hand, that its handle hangs down. While held in this position, the blade is introduced, as far as it can be made to go, in the right side also of the pelvis, over the child's left temple but under the shank of the female blade. The right hand is next carried to the still pendent handle, which it raises, and upon which it makes upward pressure. This movement, combined with a guiding pressure from the fingers of the left hand in the vagina, makes the blade glide up over the sacral side of the head. When the branches are united, the lock should press firmly on the tuberosity of the left ischium. Should the occiput look to the right ilium, the same general rules are to be observed. But now both blades are to be introduced in the *left* side of the pelvis—the left (male) blade first, and under the pubis; the right (female) with its handle pendent. The lock should now press firmly on the tuberosity of the right ischium.

first to flex the head, either by the hand, by the vectis, or by one blade of the forceps. In my own experience, however, the act of locking and that of grasping the handles, usually bring about the needful amount of flexion ; for success in such an application of the forceps presupposes a certain amount of mobility of the head, which insures its flexion.

Hence the large biparietal diameter, or one of its congeners, must inevitably pass directly through the narrow conjugate. But, apart from its width, the hind-head is the least compressible portion of the cranium. The parietal bosses, being projections, as their name indicates, and being also the centres of ossification, are the most unyielding points of the vault. They are in fact eburnaceous when compared with other portions of the parietal bones. The posterior fontanelle is small, not open, and not infrequently ossified. The sutures are narrow. Only three bones meet there, the occipital and the two parietals. But the occipital bone goes to form the hard cranial base, and is incompressible. The compressibility of the hind-head is therefore limited to the equitation of the parietal bones. But being shored up and buttressed by the unyielding occipital bone underneath them, they cannot readily overlap or bend in. Hence the tedious traction needed for forceps-deliveries, when compared with that required after turning. Transit-furrows are rare, but, when present, they run anteriorly from the boss either obliquely toward the cranial base, or parallel to the sagittal suture. In a recent case of forceps-delivery this was very strikingly shown. The diagonal conjugate, measured by the rule and finger, gave a length of four inches. The sagittal suture ran transversely to the left ilium, with the large fontanelle dipping down so low as to make it almost the presenting part. The blades of the forceps were applied to the sides of the head, which flexed on bringing the handles together. After strong traction of about two hours' duration, made alternately by the attending physician and myself, the head was finally delivered. It was much flattened, and presented on its sacral side the well-marked furrow of a fracture running from the parietal bone obliquely to the mastoid process *behind* the ear. The child was still; the mother died four days afterwards from peritonitis, and I now regret not to have turned. Dugés[1] also describes such a fracture of the parietal boss in one of his forceps cases. Fractures and furrows at this site would far more frequently happen in instrumental labor, were it not that the hard boss so resists the direct pressure upon it as either to flatten the whole parietal bone, or to cause lesions of it at weaker, although unimplicated, points.

But there are mechanical objections still stronger against the forceps when compared with version. The base of the skull being unyielding, the head cannot be compressed so readily by catching the vault with the forceps. For the effect is then to direct the compressive power toward the unyielding base. But by turning, the compressive power is directed toward the yielding vault. Further, from the peculiarity of construction of the forceps, the compression exerted by it is necessarily diffused over the whole surface covered by the blades. Now, in a bilateral application, the tips of the blades cover the bones of the cheeks, which are incompressible and so narrow as to need no compression whatever. Hence, not only is this amount of compressive power misapplied and sheer waste, but it detracts by so much from that applied to the offending diameter—the biparietal. Thus the surface covered by the blades not only involves

[1] Manuel d'Obstétrique; Montpellier, 1840, part v. sect. i. art. i. p. 306.

unimplicated diameters, but it is too broad to be very materially reduced by compression. Even when the handles are lashed, but little more is gained, and that little at the expense of a prolonged, disorganizing pressure upon the brain. Baudelocque's exceptionally powerful forceps were, with lashed handles, found by their inventor barely to reduce the biparietal diameter as much as four lines, although the force applied was so great and so prolonged as to spoil the three instruments used. It follows, therefore, that the Hodge or the Davis forceps, which are much feebler instruments than the French one, cannot compress the biparietal diameter as much as three lines, and the operator is therefore compelled in narrow pelves to make the anterior portion of the brim and the promontory of the sacrum do most of the compressing work; and that by sheer and prolonged dragging force, which, as in the case just narrated, threatens injury to the mother and death to the child.

Again, the forceps-blades being of equal length and breadth, and being also applied to corresponding surfaces of the child's head, make equal compression upon the pubic and the sacral side of the head. But from the limitation of the bone-lesion in these pelves to the posterior half of the brim, the pubic side of the head needs no compression; for its convexity naturally adapts itself to the smooth concavity of the anterior half of the brim. Therefore just one-half of the effective compressive power is absolutely thrown away. For granting that, in a given case of conjugate narrowing, the forceps reduces the biparietal diameter by three lines, this diminution does not facilitate to the same extent the passage of the head; for supposing that it actually reduces the width of the head by three lines, yet it does not virtually so reduce it. This is evident from the fact that the pubic side of the head needs, not compression, but adaptation, and the bulging tissues around the periphery of the blades will at points separated by their width still impinge on the pubic concavity of the brim, making the clamped portion of the head subtend the symphysis pubis like the chord of an arc. In all the narrow pelves belonging to the museum of the Philadelphia Obstetrical Society, and one of them is under two inches in its conjugate diameter, the pubic side is concave and smooth. After a careful examination of the distorted pelves in the museum of the University of Pennsylvania, I find that the same condition obtains in all that were not affected by osteomalacia. Turn now to the sacral blade of the forceps; it is the posterior half of the brim which is distorted; and to its irregular configuration must the sacral side alone of the head be moulded. Since also the distortion is mainly owing to the descent of the sacrum as a whole, and to the anterior projection of its promontory, the portion of the head needing moulding is a small area of surface corresponding with the sacro-vertebral angle. In other words, it is, when flexion is forced by the forceps, the biparietal diameter alone that needs shortening, and that by the indentation of its sacral pole. But the very width and length of the sacral blade thwarts this indication. This blade flattens the whole sacral surface of the head, but does not mould it. The whole head and included brain are in fact flattened out by the two blades. And this excessive bilateral compression so unnecessarily lengthens out all the other diameters of the head, that every physician has seen forehead bulging out, eyeballs starting from their sockets, and life destroyed by the violent and prolonged compression to which the whole brain has been subjected. In one word, by the bilateral compression of the forceps the pubic blade interferes with the process of adaptation; the sacral blade prevents the utilization of the roomy

concavities on each side of the promontory, and the whole brain is, therefore, subjected to an unnecessary amount of compression.

The catch-word "moulding," which is so much used to designate the action of the forceps, is a misnomer. Moulding here means an outline conformity of the cranium to the kidney-shaped configuration of the brim. To attain this end, it is needful for the pubic side of the head to adapt itself to the smooth concavity of that portion of the pelvis, for the occipito-frontal diameter to be moderately lengthened out, for a limited area of the sacral side of the head to be bent in by the promontory, and for lateral bulging to take place around it in the very roomy sacro-iliac spaces. No moulding of the head, in its true obstetric sense, can possibly occur, unless, as in natural labors or by version, the pelvis itself acts the part of the moulder. Look at a head in the grasp of the forceps applied bilaterally. How is it possible for this steel-ribbed head to alter its shape to that of the brim? How can any bending-in take place within the fenestra of the sacral blade, whose iron rim, by firm pressure, acts as an abutment to the included parietal boss? How can the pubic side of the head adapt itself to the anterior portion of the pelvis? How can the head bulge out at points clamped by iron? The forceps, thus applied, indeed moulds the head, but to its own shape, and not to that of the brim. For how can bow-shaped blades mould a head to a kidney-shaped brim? The thing is an impossibility. Who ever saw a head kidney-shaped which had been delivered in such a manner through a narrow brim? The truth is that the head then passes the brim by being flattened out, and not by being moulded; and, what is worse, since the blades do not exert the needful compressive power, the promontory itself is made to do most of this unnatural work—the work of flattening and not of moulding. When the head is squeezed sufficiently flat, it is delivered by being dragged straight past the promontory, without revolving around it as it does in natural or in version cases.

So true is this that, when a head has been delivered through a small conjugate by the forceps applied bilaterally, never will it exhibit a cast of the promontory by a spoon-shaped depression, and only once have I seen a slight furrow. The whole head is flattened, the bones are separated at the sutures, sometimes so much so as to seem loose in a scalp-bag, but no lesion of limited area is found by a surface examination. Post-mortem examination will, however, reveal fissures and fractures radiating from the base of the parietal boss.

It follows that the hackneyed term of "wire-drawing" the head by the forceps, is also a misnomer. In "wire-drawing," the pincers which seize the end of the wire do not pass through the graduated holes in the draw-plate, but work outside of them. But when the head is seized by the forceps, the pincers, so to speak, must also pass through the bony draw-plate itself. So great a disadvantage is this, that there are on record far more remarkable cases of successful natural delivery in very small pelves, than there are by this use of the forceps. For it stands to reason that when a forceps, whose blades lie from two and three-quarters to three inches apart, is applied to the sides of a head at the brim of a pelvis with a conjugate of less width, the blades will need wire-drawing as much as the head, before they can be made to pass.

So far the use of the forceps. Mark now what happens when the child is turned in a flat pelvis. The wedge-shaped and semiflexed head enters the conjugate by its small bisauricular diameter, and passes it by

its very compressible bitemporal diameter. The very cause of the conjugate deficiency—the jutting promontory—becomes itself the moulder, and the plastic head, unhampered, unclamped, unribbed by bars of steel, being also drawn upon by *outside* traction, accurately moulds itself to the shape of this small bony barrier. Meantime the pubic side of the head, already presenting its convexity to a concavity, is made by lateral pressure to fit snugly the anterior half of the brim. Bulging takes place at all the uncompressed points, and the whole head becomes concavo-convex, being rapidly moulded to the kidney-shaped configuration of the whole brim, while backward traction and supra-pubic propulsion cause the pubic side to revolve around the promontory as a centre of motion. To my mind nothing can be plainer. How, otherwise, is the absence of vesical disturbance, after version, explainable? How the fact that, as I have seen and others attest, it will take as many minutes, less minutes indeed, to deliver the head by version, as it has taken hours of unsuccessful traction with the forceps? Even without these facts and without a cloud of witnesses to sustain them, an *à priori* reasoning would compel me logically to the inevitable conclusion that version possesses great mechanical advantages over the forceps.

One word in regard to the obliquely ovate pelvis. If the hind-head present in the less roomy side, by turning, the soft and voluminous breech is very likely to adapt itself and pass through the more roomy side. This tendency, together with a knowledge of the fact that the foot drawn on usually rotates to the pubic arch, ought, very generally, to enable the practitioner to place the occiput in the larger half. Yet so great an advantage is it to turn, that Barnes and Strassmann declare[1] that they have saved the children, even when they had failed by this operation to effect the desired change of position.

The use of the vectis, or tractor, in narrow pelves has been so highly lauded by Boddaert, of Brussels, Raffaele, of Naples, Fabbri, of Bologna, Marchant, Herbiniaux, Coppée, Beytter, Frayes, and other Dutch, Belgian, and Italian writers,[2] that a few words in regard to the action of this instrument are needed.

When the head is transverse, this instrument should be passed over the pubic mastoid region; when oblique and anterior, over the pubic side of the occiput; and when oblique and posterior, over the pubic fronto-temporal region. At first it is used as a lever of the "first kind," the left hand on the shank representing the fulcrum, and the right hand becoming the power by raising the handle toward the pubes. In other words, each hand acts in opposite directions—the left one mainly to protect the pubis from pressure. As soon as the handle is raised high enough for the blade to secure a good hold, the right hand is kept at rest to become the fulcrum of a lever of the "third kind," while the left acts as the power. This compound action of traction and leverage, peculiar alone to the vectis, here meets several important indications. As a lever, this instrument compresses the head from before backward in its lateral diameter, and aids the moulding of its free sacral surface to the posterior distortion of the brim. By its action as a tractor, it hooks down the pubic side of the head, making it revolve around the promontory of the sacrum as a centre of motion, and roll over into the pelvic cavity.

[1] Obstetric Operations; by R. Barnes, 1871, chapter xvi. p. 241.

[2] Cazeaux, edited by Tarnier, 1874, p. 998; Congrés Médical de France, 1865, pp. 584, 589; Traité des Accouch., par Hyernaux; Bruxelles, 1866, p. 699; New Sydenham Society's Biennial Retrospect, 1869–1870, p. 409.

From the close resemblance of this mechanism to that of an unaided head-first labor, and to that of an aided head-last labor, I am led to think that the vectis, in skilled hands, will be found more efficient than the forceps in dislodging a head from the brim of a pelvis narrowed mainly in its conjugate diameter. Below the brim, and in the generally narrowed pelvis, it cannot of course compete with the forceps excepting in certain anomalous positions and presentations. But in selected cases this neglected single-bladed instrument is, in my opinion, destined to take rank over its double-bladed fellow. For instance, in 1863, the late lamented Fabbri, of the University of Bologna, quickly delivered with the vectis, through an artificial pelvis, a head which S. Tarnier,[1] although using all his strength, was not able to budge with the forceps. The head was then replaced, and the forceps re-applied, but with the same want of success. Professor Tarnier, who is the narrator, then resorted to the lever, and, much to his surprise, delivered the head "with astonishing ease" (*avec une étonnante facilité*). This experiment was repeated, and invariably with the same result.

To sum up then the mechanism of natural and of artificial labor in a flat pelvis:—Nature makes the first-coming and the after-coming head enter the brim by extension; the forceps by flexion. With the former the head engages in the short conjugate by its shortest and most compressible diameter; with the latter by its largest and most unyielding diameter. By nature and by version the head revolves around the promontory as a centre of motion; by the forceps it is dragged straight past, over this osseous point. By the former the head is moulded to the outline configuration of the brim; by the latter, when applied to the biparietal diameter, it is moulded more to the shape of the blades than to that of the brim. By the one the brain lesion is local and limited to the area bent in by the promontory; by the other the area of undue compression is increased. By nature and by version the compression is that which is needed and no more; by the forceps it is more than is needed.

Regarding then alone the mechanism of labor in narrow pelves, to which the scope of this paper is strictly limited, the following conclusions are reached:—

I. The unaided first-coming head and the aided after-coming head observe in a flat pelvis precisely the same general laws of engagement and of descent. Hence, version here means art *plus* nature.

II. The forceps, however applied in a flat pelvis, antagonizes more or less with the natural mechanism of labor. Hence, the forceps here means art *versus* nature.

III. The aided and the unaided first-coming head observe in a uniformly narrowed pelvis precisely the same laws of engagement and of descent. But version violates these laws. Hence, the forceps here means art *plus* nature; version, art *versus* nature.

IV. At or above the brim of a flat pelvis, the fronto-mastoid or even the fronto-occipital application of the forceps interferes less with the moulding of the head, and violates less the natural mechanism of labor, than the biparietal application.

V. In the flat pelvis, the vectis aids the natural mechanism of labor, and, therefore, meets the indications better than the forceps.

[1] Cazeaux's Obstetrics, 1874, p. 996.

Discussion on Dr Goodell's Paper.

After the reading of the preceding paper, the President, Dr. Robert Barnes, of London, said:—The paper is now open for discussion. I desire, however, first to say that it is the ablest paper that I have ever heard or read upon the subject. I am aware of the difficulty in treating this question, for the passage of the fœtus through the pelvis, and the obstructions to its progress from deformities, are not only to be studied clinically, but, as the conditions are mechanical, by *à priori* reasoning. Yet, after all, experience is the best guide, whether the method proposed is clinical or empirical. I shall be glad to hear from the members of the Section, and may suggest as heads for the discussion (1) the mechanism of labor when unaided, and (2) its mechanism when aided by art. I am glad to see in the room Prof. Barker, who has possibly more special knowledge on this subject than any one I know of.

Dr. Fordyce Barker, of New York, said:—I believe that Obstetrics has begun to make itself an exact science, and I am sure that investigation and the presentation of papers such as we have heard will have a tendency to make it more and more exact. I am sorry that I did not hear the first part of the paper, for I should feel like studying it carefully before commenting upon it. I think that the vectis in certain cases of contracted pelves is preferable to the forceps, and I am pleased to find that Dr. Goodell agrees with me upon this point.

Dr. William H. Byford, of Chicago, said:—I have greatly enjoyed the reading of Dr. Goodell's paper, but as to criticism, I think mine would be valueless. I have in my writings advised the use of the vectis in cases of the after-coming head, where it has not entered the pelvis; but I use the forceps where the head is fully in the pelvis. Since hearing Dr. Goodell's paper, I feel disposed, in the case of kidney-shaped pelves, not only to turn, but to use the vectis instead of other instruments. Obstetrics is a science that is almost entirely mechanical, and for that reason I think, with Dr. Barker, that it will ere long become an exact science. I regret very much that I have not had the time or the opportunity to study the valuable paper which has been presented, and the subjects treated of in it.

The President, Dr. Barnes, said:—It must be apparent that in certain cases of narrow pelves one must resort to the induction of premature labor; but that point has not been referred to. The value of this operation has been called in question of late years, and it may be well to inquire how does the mechanism of labor bear upon those cases in which lives are thus saved? I should like to have the experience of those familiar with cases in which the child has lived after the induction of labor.

Dr. William Lusk, of New York, said:—In the main I agree with the conclusions of Dr. Goodell. I have, however, applied the forceps along the sides of the pelvis, and have, in some cases, dragged the head down to the floor of the pelvis, without flexing it, both fontanelles being nearly on a level. It has always been my habit, after the conjugate diameter has been passed, to wait for the head to flex and the occiput to rotate, before renewing traction and completing the delivery. If continuous traction be kept up, then all the statements of the paper are justified; but if occipito-frontal compression be applied at the brim, and traction made only during the pains, the child's head will generally pass without being flexed, and delivery will be effected in accordance with the mechanism described by Dr. Goodell. When the head is fixed in the brim, I would use the forceps with no apprehension of flexing it.

Dr. Goodell, in reply to a question, said:—I have no experience with the instrument of Chassagny. But all the distinguished obstetricians in France are leaning in the very questionable direction of importing the labor-saving instruments, the straps, pulleys, and capstans, of veterinary obstetrics, into human obstetrics. In 1872, Chassagny bore off the Montyon prize for his

appareils de traction soutenue, and since then many others have been invented. It is a significant fact that these instruments have originated in France, where version is bitterly opposed. It is also a significant fact that the only eminent French obstetricians who oppose them are Blot, who practises version to save the life of the child in narrow pelves, and Pajot, who turns in order to crush the base of the skull by his cephalotribe.

Dr. Alexander R. Simpson, of Edinburgh, said:—The flat pelvis presents peculiar advantages for version. One would expect the greatest advantage when the base of the skull, which is the narrowest part, comes in relation to the narrowest part of the pelvis. I have seen this, in my own practice, again and again. Reference has been made to Budin and his series of mechanical experiments on the comparative force needed by version and by the forceps in dragging the child through the pelvis. These experiments have somewhat staggered me. In some of them the long diameters were shortened, and in others they were elongated. In Dr. Goodell's conclusions I see allusions made to the application of the forceps to the sides of the head at the brim. But it is an impossibility for the forceps to grasp the sides of the head in the case of a flattened pelvis; for there is no room, and the operation is simply impracticable. It might be possible for one blade of the forceps passed over the occiput and the other over the forehead to so grasp the head as to produce elongation and thus cause the head to pass through a narrow brim, but version is preferable in these cases.

The President, Dr. Barnes, said:—I have practised version for twenty years in preference to using the forceps, and am becoming more partial to it the more I practise it. From the forceps there can be but little benefit except in minor contractions, but in great contractions, where there is any chance of saving the child, I should use all means in preference to the forceps. As to the mechanism of labor in a "head-first presentation," it has been well explained in the paper of Dr. Goodell. I can only add force to those deductions by referring to the frequency of occipito-posterior positions in contracted pelves, a frequency which enforces Dr. Goodell's arguments. Under such circumstances version is the best mode of delivery, but there are occipito-posterior presentations in which, the head having passed through the narrow brim, and the woman being exhausted, the forceps will finish the labor. I have delivered one lady five times, all with the same difficulty of occipito-posterior presentation. In another case in which there was a slight contraction of the pelvis, the child was lost by the use of the forceps, but on two occasions when I turned, the child was saved. It requires practice in these cases of version in a narrow pelvis, and the mechanism, in head-last presentation, will be much better understood by careful study. In turning, there is the danger from dragging on the child's neck, and from the traction on the medulla oblongata, or in some cases from the injury to the brain pulp. I have seen a child's head pulled off by injudicious traction. There is in these cases often a misdirection of force on the part of the physician which jeopardizes the child's life.

[Dr. Barnes explained by a diagram on the blackboard the method of getting the head around the sacral promontory by backward traction.]

The force needed can be augmented, and the mechanism facilitated if some one pushes from above while the physician pulls from below, as this will take off a great deal of the strain on the neck, and the propelling hand can guide the child's head in its rotation around the promontory. I lay great stress on supra-pubic propulsion by the hands of an assistant. The use of forceps on the fore-coming head in contracted pelves, is not based upon sound mechanical principles. In cases of minor contraction, it aids delivery by elongating the head. The direction of thought in obstetrics is now tending to a greater precision in this question of the mechanism of delivery. Sometimes it is found impossible by any mode of delivery to save the child alive; sometimes the woman is too much exhausted for an attempt at version to be made. In these

cases, after perforation and the escape of a few ounces of brain, the forceps can be used for breaking the skull from behind, and it will be found to collapse at once. It cannot be said under such circumstances that the child has been sacrificed, but that all endeavors to save the life of the mother have been used, and that the child has been abandoned only when it has become absolutely necessary. As to the vectis, I deem it of service only in cases of minor contraction; but it has been greatly lauded by some provincial practitioners, who prefer it to the forceps.

Dr. GOODELL, in reply, said:—I was glad to hear Prof. Simpson refer to M. Budin's paper. Its conclusions in favor of the forceps at first staggered me also, but, upon an examination of the experiments, three sources of fallacy became manifest. In the first place, the pelvis used by M. Budin was not an originally flat one, but an iron cast of what was supposed to be a normal pelvis. To narrow the conjugate of this metallic pelvis, Budin had a movable, false promontory so constructed that a sacral screw forced it towards the pubis. Hence the conjugate diameter of the brim was shortened without a proportional lengthening out of the transverse diameter, and every turn of the screw converted the pelvis into a uniformly narrowed, flat pelvis, and not into a simple, flat one. Thus, as his experiments abundantly showed, there was not room enough in the transverse diameter of the brim to admit the occipito-frontal diameter of the head. But when he experimented with children born before term, and consequently with a shorter occipito-frontal diameter, far less tractive-force was needed to deliver them by turning, than by the use of the forceps. Again, this movable and metallic, false promontory was cast from a mould taken from the broad, blunt, and rounded promontory of a normal pelvis, and not from the narrow, sharp, and jutting promontory of a flat pelvis. Hence the needful indentation of the head was gained by this broad and blunt knob of a promontory with very great difficulty. In the third place, Budin's iron pelvis was placed on a table and near its edge, to represent the dorsal decubitus of a woman when artificially delivered, while the tackle used by him for these experiments was fastened to a staple driven, not into the floor near the legs of the table, but into the wall. Consequently, not only no backward traction could be made on the legs of the child, but the traction was so directed as to cause the head to revolve around the pubis, and not around the promontory, as the centre of motion. In reply to Dr. Lusk I would say that, if the head be well nipped in the conjugate, the fronto-mastoid or the fronto-occipital application of the forceps will not ordinarily flex it. But in other cases it commonly flexes the head, and always tends to do so. When the blades are applied to the sides of the head, the vertex always dips, and the biparietal diameter enters the conjugate, because such an application could not be made if the head were tightly nipped. I wish finally not to be misunderstood as recommending version to the exclusion of the forceps. The forceps is for obvious reasons generally my first choice; besides, version cannot always be made; in some cases, indeed, should not be attempted. But in the paper which I have read, I have avoided all such side-issues, and have approached the subject from a purely mechanical stand-point.

THE MANAGEMENT OF CONVULSIONS, IN CHILDREN, DEPENDING UPON A HIGH TEMPERATURE OF THE BODY.

BY

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OF CHATHAM, ONTARIO, CANADA.

GENTLEMEN:

I purpose, in this short paper, to ask your attention to the management of convulsions, in children, depending upon a high temperature of the body. The subject will not be thought unworthy of consideration by any one who has seen this complication resist various plans of treatment, and prove fatal after hours of intense anxiety on the part of all concerned. The great frequency and fatality of convulsions, in children, should be a strong incentive to all physicians to avail themselves of every mode of reducing the mortality. In this city during the five years from 1844 to 1848 inclusive, 1729 children, under fifteen years of age, died of convulsions; a higher mortality than attended either cholera infantum, marasmus, pneumonia, or hydrocephalus, during the same years. It is neither possible nor desirable in this paper to enter into a lengthened discussion of all the phenomena connected with this subject. This has been done by most modern authors, and by none more lucidly or ably than by your distinguished countrymen, Meigs and Pepper.

It is admitted by all that eclampsia is a frequent complication in many febrile diseases, as scarlatina, measles, pneumonia, malarious fevers, etc., and by nearly all writers great prominence is given to the particular poisoning of the blood in these affections, as the chief factor in the causation of the convulsive fits which so often complicate them. While admitting that certain septicæmic conditions may not be without influence in producing the convulsive attacks, I believe that the simple elevation of temperature, accompanying these diseases, is sufficient, in most cases, to account for them. In this connection it is worthy of note that those febrile diseases in which convulsions are common, are those usually marked by a high degree of temperature, although the specific poison in each may be quite different; and that in other diseases, as smallpox, syphilis, and diphtheria, in which the blood-poisoning is very virulent, but the temperature not very high, convulsive seizures are comparatively rare. The experiments of Dr. Brunton prove that heat acts as a direct excitant to the heart, causing it to beat more frequently, directly as the elevation of temperature, and that cold has the opposite effect. These results are due to the influence of heat and cold upon the cardiac nerves, and from the extreme nervous irritability accompanying fever, as evinced by restlessness and liability to convulsions, it is fair to conclude that heat and cold act similarly on the cerebro-spinal system. While an elevated temperature causes increased frequency in the heart's action, it weakens the force of its contractions, a fact that should not be overlooked in the administration of some therapeutic agents which, however useful under other circumstances, become highly dan-

gerous when the heart has become weakened from any cause. This is especially true of aconite, veratrum viride, chloral hydrate, and bromide of potassium.

From observations upon thirty cases, of which I have preserved notes, the following propositions may be deduced: (1) That the nervous susceptibility of children under ten years of age strongly predisposes them to convulsions; (2) that in children of that age a temperature of 103° Fahr., and upwards, is very liable to excite an attack; (3) that the severity of the attack bears a direct ratio to the intensity of the fever; (4) that convulsions are more liable to occur during the first few hours of the pyrexial seizure, than at any subsequent period of its course; and (5) that whatever reduces the temperature, modifies or arrests the convulsions.

Regarding the first four propositions, few will differ, but while the fifth may be generally admitted, there is great diversity of opinion as to the best manner of accomplishing the reduction of temperature. The means at our disposal may be classed as internal remedies and external applications; the former including veratrum viride, aconite, digitalis, quinine, and salicine, with probably some others not as much used nor as well understood, and the latter consisting of cold applications to the surface of the body. Most of the remedies of the first class exercise a very depressing influence on the heart, and so are not applicable to these cases, quinine being almost the only one upon which any reliance can be safely placed. It generally happens, moreover, that in all severe cases of convulsions the child cannot swallow, and then remedies of the first class must be dispensed with altogether unless we resort to their administration hypodermically, a proceeding not warranted by the results obtained.

For the antipyrexial action of quinine, veratrum viride, aconite, and digitalis, I would refer to the experiments of Dr. Roberts Bartholow, of Cincinnati, which have been published in one of a series of clinical lectures edited by Dr. Seguin, of New York. From a study of the physiological action and therapeutic influence of these remedies, it is plain that their modus operandi, in reducing the temperature, is quite different from that of cold applied to the surface of the body. In the former, the result is accomplished by interrupting the conditions which render rapid chemico-vital change in the capillary system possible; in the latter, it is accomplished by the simple abstraction of heat already generated. By the former, it is accomplished slowly, imperfectly, and with great risk to the patient; by the latter, the same is done rapidly, perfectly, and, in at least the great majority of cases, without risk. It is chiefly with the view of illustrating the curative influence of remedies of the second class that I have ventured to ask your attention even for a few minutes. For this purpose I have transcribed from my case book the following cases:—

Case I.—July 3, 1876. M. Arnold, two years of age, strong and well developed, was taken suddenly ill last evening with dysentery and high fever which lasted all night, and at seven o'clock this morning had a pretty severe convulsion. At eight I saw him for the first time, and found the temperature 103° F., and the child restless. I gave a large dose of castor oil, and also one-third of a drop of tincture of aconite, to be repeated every hour while fever lasted. Another convulsion occurred at ten o'clock, and a third at half-past ten, when I arrived in time to check its violence with chloroform. At noon, the castor oil had operated well. At 2 P.M., convulsions returned, and continued for

two hours with no intermission, although the patient was partially under the influence of chloroform during the time. At 4 P.M. they were as violent as possible, the temperature was 105° F., the pulse 150 and very weak, and death seemed imminent. I now put the child into a tepid bath, and rapidly cooled it to 50° F. by the addition of ice and iced water. At the end of ten minutes the breathing became easier, in fifteen minutes the temperature was 103° F., and in twenty minutes it had fallen to 99° F., and the pulse to 110 beats per minute. I then dried the child and wrapped him in a blanket. He was very much better, but the breathing was still somewhat noisy and labored, and there were two slight fits during the first half hour succeeding the bath; the temperature also began to rise again, and had reached 101° when I again put him into the same cold water for five minutes. The temperature now fell to $98\frac{1}{2}$°, and the pulse in the same time to 100 beats per minute. The child slept naturally for half an hour, and awoke apparently quite well. There was no return of fever, and no further treatment was required.

One can scarcely avoid the conclusion that the convulsions in this case were caused by a high temperature alone. The previously healthy state of the child, the freedom of the alimentary canal from anything irritating, for the castor oil had acted thoroughly two hours before the most violent attack came on, the absence of any other appreciable, centric, exciting cause, and the almost instantaneous cessation of the convulsions when the temperature had fallen below 99°, all support this conclusion.

Case II.—Sept. 26, 1872. P. T., æt. 8 years, was never ill until noon to-day, when he had a chill for a few minutes, followed by fever and convulsions, which still continued when I arrived at one P. M. His temperature was 106°, and his pulse 140, full, and regular. The attendants had just removed him from a warm bath and wrapped him in a blanket. It was impossible to get him to swallow anything. I at once applied cold to the head, administered an enema, and put him under the influence of chloroform. This modified the violence of the spasms, but they returned immediately when the effects of the anæsthetic were allowed to pass off. The enema acted well, and the chloroform was continued, but the temperature was unchanged, the pulse became gradually weaker and more frequent, and after three hours the patient died. As this boy had been well until now, and as no other exciting cause could be traced, it is not unreasonable to conclude that the high temperature led to the fatal result.

Case III.—July 12, 1876, 10 A. M. C. S., a well-nourished male child, five months old, has had diarrhœa for three days, and the mother thinks fever also, but was not very ill until yesterday afternoon, since which time he has neither nursed nor slept, but has constantly uttered half suppressed cries. The child is pale, the hands and feet quite cool, and the skin dry. Gave a purgative dose of rhubarb and soda.

3 P. M. The rhubarb has acted on the bowels three times, the last evacuation having been quite natural in color. The hands, feet, and legs are quite cold; the pulse almost imperceptible; the pupils not larger than pins' heads; the thumbs contracted; the face pinched, and of a leaden hue; and the thirst most intense. The axillary temperature, to my surprise, was 105° F., for I had been deceived by the coldness of the extremities and the general appearance of the patient, and did not expect to find the temperature so high. I now gave the child half a drachm of brandy, and placed it in a tepid bath, adding water from the well until the whole was quite cold. In ten minutes the temperature fell to 103°, and the child went to sleep for the first time in thirty hours. When the temperature had fallen to 100°, I wrapped the child in a blanket, and he continued to sleep most of the afternoon and was not thirsty. As the temperature fell, the pulse became slower and fuller, and the pupils larger.

8 P. M. Temperature 103°, pupils natural; the child sleeps well and looks placid. Bath repeated, and temperature reduced in five minutes to 99°.

13th, 10 A.M. Child rested well all night; nurses; temperature 103°. As he appeared quite easy, I merely ordered a dose of castor oil.

11 A.M. Child has had two severe convulsions in the last ten minutes, and is insensible, and the temperature has risen to 105½°. Repeated the cold bath, and reduced temperature to 98½°, when the child became quiet and slept. After this the temperature never rose above 101°, the bath was not repeated, and in a few days the child was well.

The coldness of the extremities and the weakness of the pulse in this case made me hesitate before resorting to the cold bath, but as death seemed imminent, and as the axillary temperature was so high, I concluded that any other means would fail, and that some risk was justifiable. It is remarkable that during the hour preceding the convulsions, the temperature rose from 103° to 105½°. I could cite a number of cases resembling the three here detailed, half of which would go to show the superiority of the treatment by cold baths over every other, when the temperature is high; and my friend and co-delegate here, Dr. Murphy, informs me that his experience with this plan of treatment entirely coincides with mine.

Where other indications for treatment exist, as malaria, teething, offensive accumulation in the alimentary canal, etc., it of course becomes necessary to attend to these also. In pursuing the plan I here recommend, I have always been guided by the thermometer, never resorting to the bath when the temperature has been below 100°, and always removing the patient from the water when the axillary temperature has fallen to nearly the normal standard of health.

It is much better to have the water tepid at first, on account of the excitement and alarm produced by sudden cold. I have not ventured to use the cold bath in convulsions complicating pneumonia, my experience being chiefly confined to those occurring in connection with dysentery, diarrhœa, cholera infantum, and malarious fevers. In these diseases, when the fever is high, although there be no convulsion, I have found no remedy that approaches the cold bath in its power of allaying nervous irritation and procuring quiet rest, and I am fully persuaded that all who resort to this mode of treatment once, will do so again.

Discussion on Dr. Holmes's Paper.

After the reading of the preceding paper, the President, Dr. Robert Barnes, of London, said:—Where there is an increased temperature in children, there is always some trouble with the blood, poisoning, or something else, which leads to convulsions. The question is, what develops convulsion? what is the first cause? It has been found that as the temperature rises, there is an increased liability to convulsions, and that as the temperature sinks, there is a decrease in this liability. Do we find that high temperatures always produce convulsions? In children, there is an exalted nervous irritabilty like that in pregnant women. Do they always have an increase of temperature? I have watched cases in which there has been no rise whatever—in uræmic convulsions after scarlet fever, for instance. In simple pyrexia I believe that cold water can be used to reduce the temperature; I have wiped the surface of the body with it, and found it to act well. The cold water acts by evaporation, and consequently reduces the temperature. The cold bath is a powerful remedy; its use by the Germans is a revival of a remedy first employed by Dr. Currie.

Dr. Thomas F. Rochester, of Buffalo, N. Y., said:—I have paid consider-

able attention to infantile disorders, and think I can corroborate the views advanced by Dr. Holmes in many respects. The subject is one which requires more attention than is paid to it. The reduction of the temperature by water, I regard as more safe than by the use of drugs. In my own city there have been many children lost, during the hot season, from what is called summer-complaint, which is ascribed wholly to the elevation of the temperature, but this I deem an error. The thermometer rises to 90° or 100°, the sky is blue and the sun bright, and yet the mortality is not as great as it is when the atmosphere cools off. Some years ago we had, in my city, a hot, dry spell, with but little sickness among the children; a hail storm came without any premonition, and at once infantile disorders set in. The diminution in the heat had caused the whole trouble. I have not examined the temperature before and after such atmospheric changes, but to me it seems conclusive that the fall in the temperature produces more diseases of children than the rise. I speak of convulsions as associated with the diseases of children, and make my remarks only in that connection.

Dr. Holmes, in reply, said:—I have lost a good many children from high temperature; and now think it well to reduce the frequency of the heart beats, for, as they become slower, the exhaustion lessens, and the beat becomes more forcible. My views are embodied in the short paper presented, and are based upon experience gleaned from my own practice.

OVARIOTOMY BY ENUCLEATION.

BY

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It is now about seven years since I announced to the profession that ovarian tumors could be removed by enucleation, and invited my professional friends to make trial of the proposed plan, describing, as well as I could, what I had done and the conclusions I had formed.[1] The idea that a tumor having such large arterial supply, could be removed without clamp, ligature, or cautery, though at first startling, received considerate attention, and, both in this country and in Europe, its successful results have led some of the most distinguished operators, not only to make trial of it, but to speak of it in high terms of commendation.

From the numerous reports and papers upon the subject, I discover that the exact manner of enucleation is not yet distinctly understood; some have spoken of a *clamp* after enucleation, others have spoken of cutting, the very thing that is to be avoided, while others still have limited the detachment of the pedicle to two or three inches above its base, thus showing me that I have never been fully understood as to the method of removing ovarian tumors by enucleation, a plan which, my experience convinces me, if properly understood and executed, possesses advantages over all others, and is of almost universal application.

It is well known that the ovarian tumor is surrounded by a peritoneal covering; that the pedicle, proper, usually divides into three or four parts, passing up over the walls of the tumor in bands of variable width, which contain vessels, often of large size, and which gradually diminish in thickness and in the size of the contained vessels, until finally they are lost in simple thickened portions of peritoneal covering. The peritoneal investment is not closely attached to the cyst, but separates readily, just as the peritoneum separates elsewhere in the pelvic cavity, being immediately lined by the subserous cellular tissue; thus no vessels of any considerable size enter the cyst. The tumor separates from its attachments with remarkable readiness, so much so that, in several instances, it is reported to have escaped the grasp of the operator and fallen spontaneously from the pedicle, accident thus plainly indicating the natural and proper method of removal.[2] The capillary vessels thus broken do not bleed, for the band contracts, and corrugates the larger trunks, while the broken-off capillaries ooze a little for only a minute or two, and a dry napkin, applied for a short time, is all that is required. The fear of hemorrhage is wholly unfounded, and I now say, without hesitation, that the danger of bleeding after this mode of proceeding is vastly less than the danger of slipping of clamp or ligature, in other methods in which

[1] Buffalo Medical and Surgical Journal, June, 1869.
[2] British Medical Journal, Nov. 26, 1870, p. 577; Braithwaite's Retrospect, July, 1871 p. 212.

the vessels are divided in their trunks. In this they are separated only in their extreme branches, and cannot give troublesome hemorrhage; it is seldom that any vessels are torn large enough to be seen as vessels or points of hemorrhage, and torsion is all that can be required in almost any case. If care is taken not to wound the vessels with either trocar, knife, or scissors, there will be no bleeding.

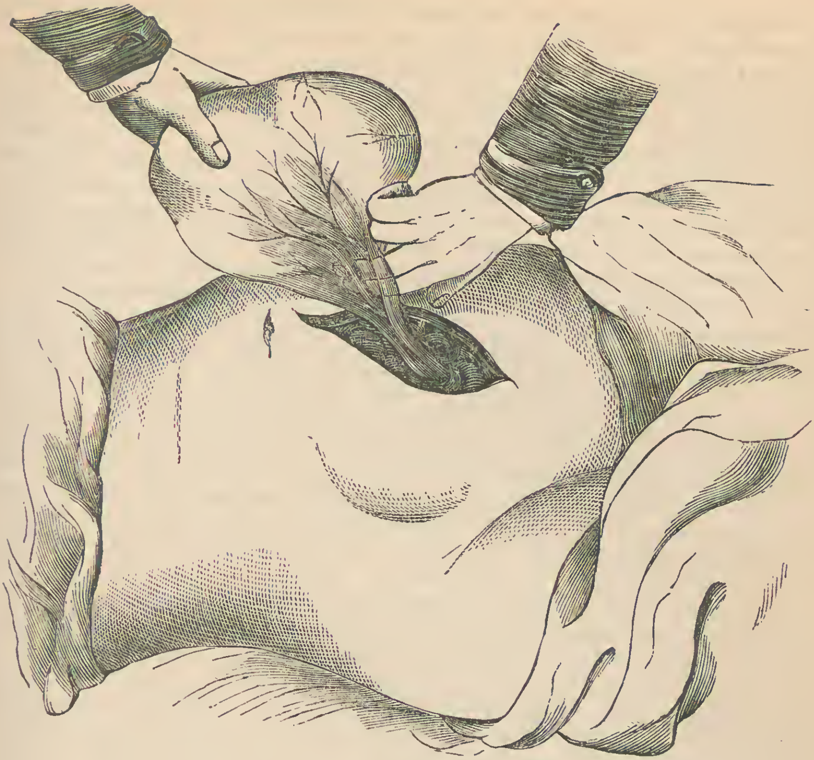
The tumor being removed, the operation is completed; there is no clamp to be used, for there is nothing to clamp; the pedicle requires no attention except to be carefully handled, and to be restored, as nearly as possible, to its original place; if the usual conditions are present, no drainage is necessary; and the incision may be closed as accurately as possible.

The bands referred to are to be grasped where they commence to diverge, raised from the cyst, and traced out to their terminations, often nearly around to the opposite side; the cyst is thus separated from its vascular supply, which is contained in these bands, other attachments being separated in the usual manner. Care must be taken not to wound or divide the vessels in their trunks, and, although the attachments will be found at points very strong, they can be forced off, or even with care a small piece of the cyst may be left attached to the pedicle, and no harm result, for it has a vascular supply, and is living tissue like the pedicle itself.

The method of enucleation has the further advantage that if upon trial it is for any reason thought impracticable, a clamp may be still applied, or the pedicle may be cauterized or tied with a ligature just as well as if enucleation had not been attempted. Few rules in surgery have no exceptions, and, though I believe that all ovarian tumors can be, and should be, removed by this simple method, supplemented when necessary by torsion or ligature to small vessels which bleed, still, to provide for all possible contingencies, I would assure the operator that a trial of enucleation is no hinderance to the subsequent adoption of any other plan that may be preferred, so that, while everything may thus be gained, nothing can be lost.

The accompanying cut, from a drawing by Dr. Edward N. Brush, who has several times assisted me in operating, will give a very fair idea of the procedure. The fingers of the operator are represented beneath a vascular portion of the pedicle, separating it from the walls of the tumor. This separation, as has before been observed, is to be carefully made, until the vessels are traced to their termination. To make the illustration plainer, the tumor is represented as raised from the abdominal cavity and supported by the hand of an assistant, but, of course, where extensive adhesions are present, this is impossible, and the risks of removal are greatly augmented. Formerly the operation in such cases was abandoned. When adhesions exist, they are to be separated, and the process continued to the pedicle.

There is no need to point out the advantages of this plan. Those who have studied the history of ovariotomy, and who are familiar with the difficulties and objections which may fairly be urged against the ordinary methods of procedure, will at once perceive that, if enucleation is successful, there is no pedicle to keep open the lower angle of the wound or to drag upon the parts—no unfavorable adhesions of the pedicle, no wires to be discharged by suppuration, and no crusts of burned tissue to be provided for. The abdominal cavity has simply been opened, and the diseased part removed; all that is left behind is capable of life.



It has been supposed that enucleation was designed exclusively for cases of extensive adhesion or short pedicle, in which no other plan could be adopted, thus lessening the number of incompleted operations. Certainly the method is adapted to such cases, but, instead of being reserved as a last resource, it should be chosen first, and the case regarded as most favorable in which it can be successfully accomplished. My surgical friends who have seen the operation, unite in regarding it as the most natural surgical procedure possible. To see it, is to be convinced of its entire feasibility and safety, while its advantages are too apparent to require a moment's consideration.

The following case first attracted my attention to this mode of treatment. I was invited to remove an immense tumor from the person of Mrs. Foster, of Cattaraugas County, N. Y. It was of many years' standing, and had been repeatedly tapped, but at length its contents became too thick to be drawn through the largest sized canula, and the patient's distress too great for endurance. The tumor was multilocular and very large, weighing, as nearly as could be ascertained, almost one hundred pounds. It was attached, throughout its entire circumference, to the omentum, intestines, walls of the abdomen, and all other parts with which it came in contact. These attachments were not so firm but that they could be broken up, and, with great care, the tumor was separated from the surrounding parts until the pedicle was reached. The process of enucleation had been carried on so extensively and successfully that encouragement was afforded for its continued trial; the pedicle was large, and extended over a wide surface, but by gentle and patient effort it was separated from its entire attachment to the tumor, and the immense growth removed without the use of a single ligature. The terminal branches of the vessels of

the pedicle gave out no more blood than issued from the vessels of the adhesions, and none of them required ligation. All hemorrhage soon ceased, and the incision was closed by the interrupted suture. The success of this procedure was complete, and the patient for nearly two weeks did not present a single untoward symptom, so that her recovery was considered almost certain. But she then began to lose her relish for food, grew weak and desponding, and died on the twentieth or twenty-first day after the operation. The fatal termination of this case detracts nothing from the success of this mode of treating the pedicle; indeed, so remarkable were the size and attachments of the tumor, that any attempt at recovery was surprising, and yet the feeble, emaciated, exhausted patient, continued to improve long enough to show that the manner of treating the pedicle was, at least in her case, unobjectionable.

On the fourth of June, 1873, I was called, in consultation with Dr. Mixer, of Buffalo, to see a patient, aged twenty-three, in whom he had diagnosed an ovarian tumor. After careful examination, Dr. Mixer's diagnosis was confirmed, and an operation was decided upon. The patient had suffered from pain in the right iliac region for about a year, but had menstruated quite regularly, though not to the usual amount, and had not noticed any enlargement of the abdomen until about six months previous to my seeing her, since which time the tumor had grown quite rapidly; she had never been tapped. On Monday, June 17, I operated, assisted by Drs. S. F. Mixer, C. C. Wyckoff and son, E. R. Barnes, and E. N. Brush. Dr. Mixer administered chloroform, which was afterwards replaced by ether. An incision about four inches in length was made in the abdominal wall, when the tumor was found adherent to the anterior surface of the abdominal cavity for a circumference of about nine inches. The attachments were carefully separated, but the walls of the tumor in this locality were so thin that it was accidentally ruptured, and some of its contained fluid escaped, whereupon a large trocar was immediately plunged in, and the contents of the cyst evacuated. The mass was now lifted from the abdominal cavity, and the pedicle brought into view. It was found to be of medium length, and its vessels were spread out over the tumor. I next introduced my finger beneath the central portion of the pedicle, at its least vascular part, between it and the sac, and by gentle manipulation separated it from the cyst. Two vessels only bled sufficiently to attract attention, and these were easily controlled by torsion. All further hemorrhage soon ceased on exposure of the part to the air, and, the cavity of the abdomen having been sponged out, the pedicle was returned without the application of a single ligature or other mechanical means to prevent bleeding. The slight oozing of blood caused by the separation of the adhesions from the abdominal wall ceased spontaneously. The wound was closed with the interrupted silver suture, supported by adhesive plaster, and warm flannel cloths were applied with bandages over the abdomen. The tumor was multilocular, and weighed eighteen pounds. On recovering from the anæsthesia, the patient complained of nausea, and vomited frequently. One-fourth of a grain of morphia was administered by hypodermic injection, and repeated once in four hours, and the patient was also given brandy and beef-tea. The following is a report of her condition as I found it on seeing her occasionally with Dr. Mixer. The temperature was not taken.

June 17, evening; pulse 120.

June 18, morning; pulse 108; vomiting at night.

June 19, morning; pulse 108; less vomiting.

June 20, morning; pulse 98; no vomiting; slept well; urinated without the use of the catheter; no swelling nor tympanites; wound was healing rapidly.

June 21; pulse 94; no symptoms of constitutional disturbance; slept and ate well.

June 24; pulse 84.

June 27; stitches removed; patient well.

During the whole course of recovery the patient declared that, with the ex-

ception of the nausea experienced during the first two or three days after the operation, she felt better than at any time just previous.

The pedicle in this case was of medium length, but too short to admit of its being brought out of the wound and secured by a clamp, without dragging upon the uterus, especially if any portion of it had been taken away with the tumor, as is the case when it is severed by the knife in the ordinary mode of performing ovariotomy. The vessels of the pedicle were large, and could be felt to pulsate plainly; and it is doubtful whether severing these with the cautery would have controlled the hemorrhage. Silk and metal ligatures have been employed successfully in many cases, and have been left in the cavity of the abdomen without producing any ill effects. Their employment, however, leaves within the peritoneum a foreign substance, which in most cases cannot but produce more or less inflammation, and any method which will allow the return of the pedicle to the abdomen, free from sources of irritation, must meet with approval. The question has been asked: "Can the pedicle be easily separated from the tumor?" In the case just narrated, the separation was accomplished with as great facility as attends the separation of cystic growths from surrounding parts in other portions of the body, and was easier, in fact, than was the detachment of the adhesions from the abdominal walls.

Prof. Logan, of the Medical Department of the University of Louisiana, in a private letter, speaking of an operation which he had performed for the removal of an ovarian tumor, the solid portion of which weighed sixteen and a half pounds, says:—"I was surprised at the facility with which the enucleation was effected; not a single vessel of sufficient size to throw a jet of blood presented itself, and no ligatures were, of course, required." In this case also, there was a rapid convalescence, with not a single bad symptom.

Upon these and similar facts is based the suggestion that the pedicle can, in ovariotomy, in some cases at least, be separated without ligature or cautery, thus avoiding many of its dangers. At first, this proposition may appear startling, and surgeons who have tied large vessels in the operation, or who have witnessed the fearful hemorrhage which sometimes takes place from the slipping of the clamp or ligatures, may regard it with some surprise, and may perhaps, without trial, look upon it as wholly impracticable; I should myself, probably, hold such an opinion, had I not had the opportunity of demonstrating to myself its entire feasibility.

An ovarian tumor is generally composed of a firm dense cyst, containing fluid of varied color and composition. It may, or may not, have a solid portion, but usually it does have more or less of a body, the remnant of an enlarged or degenerated gland. Upon the surface of this cyst is spread out the vascular, fibrous, cellular, and other tissues which compose the pedicle, but only the terminal branches of the vessels enter the cyst wall; the vessels may be quite large at their origin, but soon they are numerously divided, and enter the cyst, if at all, only when of capillary size. The attachment of the pedicle to the cyst is more easily broken than any one would imagine who had not tried the experiment in the manner described, and I am confident that the same efforts which are made to break up the adhesions to the peritoneum, omentum, and other parts, would, if extended to the pedicle, many times be equally successful.

If this method can be adopted without hemorrhage or other difficulty, its advantages are apparent. The pedicle can then be returned to the abdominal cavity without any of the objections which have been urged against this procedure. There is no ligature to be discharged by ulcerative process, or to become encysted, or to induce inflammation. There are no purulent or inflammatory products to be in any way removed or provided for; the pedicle is wholly living tissue, and has no irritative qualities which render its return to the abdominal cavity objectionable.

My object will have been wholly accomplished if the feasibility of enucleation in ovariotomy has been shown with sufficient clearness to insure a trial of the method by other surgeons.

Discussion on Dr. Miner's Paper.

After the reading of the preceding paper, the President, Dr. Robert Barnes, of London, said:—I am glad to hear the paper, as I was the first to introduce the method of enucleation into England. I think it a good method, but it cannot wholly supersede other modes of dealing with the pedicle. In some cases, enucleation is the only mode of removal practicable; but these are exceptions to the rule. I must say that I do not share Dr. Miner's dread of leaving silk and silver ligatures in the peritoneal cavity, for I have frequently seen them left there without mischief. I have also seen the perchloride of iron used to sponge bleeding points left after the sundering of strong adhesions, and without any of those formidable results which some writers attribute to its passage through the Fallopian tubes after intra-uterine injections. In ovariotomy, the great thing is security against hemorrhage; and that, I think, is best gained by the use of the clamp or the ligature. The operation of ovariotomy demands still more study than has been given to it, and I congratulate Dr. Miner upon having very materially added to the stock of knowledge regarding it.

Dr. James P. White, of Buffalo, said:—I am cognizant of twenty or thirty cases in which enucleation has been used. It is not a very difficult operation, but in some cases it is impossible—for instance, where the growth of the tumor has been rapid and the pedicle is short and large. In these cases, the cyst cannot be safely enucleated. I have no doubt, however, that enucleation is often the best method of treating ovarian tumors, but the subject merits a great deal of study and consideration, and the cautery, the clamp, the ligature, and enucleation, all deserve attention, and should be severally used according to the special indications of individual cases.

Dr. E. R. Peaslee, of New York, said:—We should not confine ourselves to any one method of treatment in the removal of ovarian cysts. Nor is enucleation always feasible. In some cases, short and large vessels enter into the cyst directly from the pelvis, and do not become capillary. In these, enucleation will not answer, because it cannot arrest the hemorrhage. Such an instance I have met with, and the patient bled to death. Again, owing to the thinness and the friability of the cyst-wall, it will sometimes break down before the adhesions can be broken up. But I feel under great obligations to Dr. Miner for introducing this method, for I have removed tumors by enucletion, which I am confident could have been removed in no other way. In most cases the ligature can be used, and I then see no particular advantage to be gained by enucleation. But I would adopt it in cases of cysts adherent to the liver. I am myself inclined to the use of the ligature, which I generally cut close. As I have only once seen the pedicle slough, I do not share Dr. Miner's fears on that score.

Dr. G. Kimball, of Lowell, Mass., said:—I have tried enucleation but once, but believe that I might have saved some lives had I known of this method

earlier in my practice. I feel very much indebted to Dr. Miner for his suggestions on this subject. Dr. Keith's great success has been obtained with the cautery, and I think that the profession is still at sea as to the proper method of dealing with the pedicle. I myself generally use the clamp, but I adapt the treatment to the particular case.

Dr. White said:—As a rule, I prefer cauterization, because it leaves fewer foreign bodies in the cavity of the abdomen; and I am sure that, in my practice, I have seen as many children born from women who have lost one ovary as I have lost cases of ovariotomy. I think that the treatment of these tumors should be eclectic, and that each case should be separately studied with a view to its treatment.

Dr. Alexander R. Simpson, of Edinburgh, said:—In regard to the application of the cautery, Dr. Keith has used it very successfully; personally, I am ready to use any method that the case may demand. The great strength of Dr. Keith lies in the thorough preparation of his cases, and in the care which he takes with them. There is always a difference in operators, and Dr. Keith does not consider minutes wasted that will prevent hemorrhage into the peritoneal cavity.

Dr. Theophilus Parvin, of Indianapolis, said:—Enucleation is of use when the pedicle is too short for the clamp. I have had two cases: one complicated with pregnancy was attended by considerable hemorrhage. This I checked by the application of flannels dipped in hot water. In such cases I can recommend hot water, and I referred to the same agent in the discussion of yesterday on uterine hemorrhage. I think, contrary to Dr. Miner's statement, that it is settled that there is no peritoneal coat to an ovarian tumor, as there is no peritoneal covering to the ovary itself. Enucleation is of value where there are peritoneal adhesions, but I believe that it can be very often dispensed with by the use of hot water. I recognize its advantages, but do not think that it is the only treatment for ovarian tumors.

Dr. Miner, in reply, said:—I agree that possibly in some cases enucleation may not be of service, and that torsion, the ligature, and even the cautery may have to be used. But in ordinary cases of ovarian tumor, I deem it to be a plain piece of surgery; and I have myself never seen a case in which the cyst could not have been removed by enucleation. Where there is bleeding, I resort to torsion; but enucleation does not interdict the subsequent use of the clamp, the ligature, or the hot iron.

THE TREATMENT OF FIBROID TUMORS OF THE UTERUS.

BY

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In opening the discussion on The Treatment of Fibroid Tumors of the Uterus, a question that has for many years engaged my attention, it will be imposssible to present the subject properly in the short space of time allotted to me; and I must, therefore, ask your indulgence if, in adhering closely to the text and compressing my material, I should fail in satisfactorily elucidating the several points in this paper.

I beg, also, to premise that, in bringing this subject before the Section on Obstetrics, I shall not go into the detail of every method of treatment. This I think would be out of place before an assembly of such enlightened men, who are already familiar with its written history. I shall merely refer to my own experience. Every gentleman, who will engage in this discussion, has an experience of his own, and if each one will bring his personal contributions, able architects of our profession will rear a structure out of these materials which no individual could erect from his own observations, and which will stand as a lasting memorial of this day's work. In this way we will be likely to arrive at the best practical results for the benefit of Science, Art, and Humanity.

My methods of treatment have been surgical and medicinal. In the use of medicine I have principally confined myself to iodine, ergot, and muriate of ammonia. Many years ago I lost faith in iodine, and now seldom use it, and only as a local application. As early as 1845, I introduced the use of ergot in the treatment of uterine fibroids, and have ever since employed it. In proof of this I may refer to my essay on Fibrous Tumors of the Uterus, published in the Transactions of the American Medical Association for the year 1853. Ergot, no doubt, acts in two ways in influencing the nutrition of fibroids of the uterus: (1) on the muscular tissue of that organ, and (2) on the capillary circulation of the tumor itself by contracting its smaller vessels. Notwithstanding this action of the medicine, I have never been so fortunate as to see a fibroid quietly disappear under its sole influence, although in many cases the size has been diminished, while in others no effect has been produced. The reason of its variability in action may be explained in the course of this paper. Recently, Hildebrandt has introduced the hypodermic use of ergot, and with results, in his hands, quite extraordinary. I cannot, however, conceive how this agent can accomplish more by its subcutaneous employment than by its administration by the natural passages. It certainly acts more promptly through the cellular tissue. But it is questionable whether this increased speed of action is a sufficient compensation for certain inconveniences to the patient from this mode of its application, as well as for the greater tax on the time, convenience, and patience of the medical attendant.

In the consideration of the treatment of uterine fibroids, it must also be kept in mind that spontaneous cures may result in consequence of

fatty degeneration; that diminution in the size of the tumors is often observed in advancing age, through senile atrophy of the muscular fibres; that fibrous induration sometimes occurs, the muscular tissue becoming scarcer as the connective tissue hardens, and that this induration is followed by calcification and an arrest of growth.

With these preliminary remarks, I will proceed to discuss the subject of treatment more definitely. In doing so understandingly, it will be necessary to keep in view the relative position of the tumor with the uterus. Hence I will make the somewhat arbitrary division into the following heads:—

I. Tumors usually accompanied with hemorrrhage, embracing (1) fibroids occupying the vaginal canal; (2) fibroids within the cavity of the uterus; (3) interstitial, submucous fibroids; (4) interstitial fibroids proper; (5) recurrent fibroids.

II. Tumors usually not accompanied with hemorrhage, including (1) interstitial, subperitoneal fibroids; (2) sessile, peritoneal fibroids; (3) pedunculated, peritoneal fibroids; (4) interstitial, cervical fibroids; (5) myomatous degeneration of the uterus; (6) fibro-cysts of the uterus.

I. Tumors usually accompanied with Hemorrhage.

(1) *Fibroids expelled from the Cavity of the Uterus, or occupying the Vaginal Canal.*—The treatment of these tumors will vary according to their size. When small, with a slender pedicle, the tumor may be grasped as high as possible on its neck by means of Luer's polypus forceps, or a similar instrument, and twisted off, or, in other words, removed by torsion.

When larger, with a pedicle not thicker than a finger, it may be seized and brought down with some force so as to stretch the pedicle, which may then be severed at as high a point as possible with Cooper's hernia bistoury, or a probe-pointed bistoury properly wrapped and protected.

When quite large, distending the vagina or filling up the pelvis like the head of a child, a small obstetric forceps may be applied to it, and then placing the patient under the influence of an anæsthetic, with the assistance of supra-pubic pressure, the tumor may be gradually brought through the os externum. Having accomplished this, one of several plans may be adopted for the purpose of detaching it: (*a*) By means of the bistoury the proper coat of the tumor may be severed all around, about an inch or so beyond the insertion of the pedicle, and then the upper end of the tumor enucleated from its peduncular attachment; (*b*) the substance of the pedicle may be directly severed by the knife; (*c*) the pedicle may be divided by the *écraseur*. I have used all these methods, and it is best to be prepared for either operation, as the surgeon must be governed by the circumstances of each case. When it can be accomplished, I prefer the first method, or that by enucleation of the pedunculated end of the tumor.

Immediately after the removal of these large tumors we should use a tampon, charged with persulphate of iron or other efficient hæmostatic and antiseptic, adapting it well against the amputated stump, and allowing it to remain from twenty-four to forty-eight hours. The raw and, usually, inflamed surfaces should afterwards be treated through the

speculum by suitable applications until they are brought into a normal condition.

It may be noticed that I have not referred to the ligature as one of the methods. I have long since abandoned this plan, preferring immediate ablation of the tumor.

(2) *Fibroids entirely within the Cavity of the Uterus.*—The treatment of fibroids inclosed within the cavity of the uterus must depend somewhat on their size and locality. When large and of long duration, having distended the body of the uterus and dilated the cervix as does the head of a child in the last months of gestation, an examination by the finger will discover a mere uterine ring, through which by firm pressure, particularly when counteracted by supra-pubic support, a tumor within may be detected. This no doubt is the condition precedent to the first form of fibroid which we have considered, and the lapse of time in all probability, if the patient should hold out, would accomplish the same state of things. It is, however, not always safe to wait, but safer to facilitate measures by imitating the processes of nature. To make the tumor more accessible, the os uteri should be dilated by the usual means, and at the same time ergot administered to excite and maintain the contraction of the expulsive fibres of the organ, and in this way also aid in opening the uterus. Or, instead of using dilators to open the os uteri, the action of ergot may first be established, and afterwards the tense edges of the os may be nicked with a bistoury, thus successively dividing the circular fibres, and accomplishing the object in much less time. As soon as the mouth of the womb is sufficiently expanded, a small obstetric forceps may be introduced and applied to the tumor in the same way as it is to the head of a child. Traction should now be made on the tumor under the influence of an anæsthetic, and if the circular fibres, as will probably be the case, should resist its escape, the bistoury must be employed until all resistance is overcome, and the tumor is brought into the cavity of the vagina. It is now in the position described under the first head of our subject, and may be managed accordingly.

There is, however, a very important difference between a tumor passing from the uterus into the vagina spontaneously and one brought there by violence, and that difference involves a very grave practical point. This will be at once apparent when it is stated that in the former case the tumor alone occupies the vagina, and that in the latter it is very apt to be accompanied by an inverted uterus. This is particularly liable to occur when the pedicle is attached to the fundus. Usually too, in vaginal fibroids, we find a moulded and elongated pedicle which is readily and safely managed, but in the tumors under consideration the pedicle is usually sessile, and affords but small space for any surgical appliance. Hence, the inverted fundus uteri is in great danger of injury and mutilation, and unusual care is required in amputating the tumor. I believe the safest mode is to enucleate the upper portion of the tumor from its proper coat, and thus free it from its uterine connections. Of course, after this is done, the inverted uterus should be at once replaced after proper styptics have been applied to the raw surface, and the case treated upon general principles.

These fibroids, however, are not always as simple and as easily managed as has been just described. From some cause or other the tumor may have formed adhesions to the surface of the uterus, independent of its original, peduncular attachment. These adhesions may be so situated as to interfere materially with the dilatation of the os uteri, or with the

introduction and application of the forceps. It is necessary, under these circumstances, as soon as they are recognized by the finger, to sever them with the bistoury until the surface of the tumor is free. If, in consequence of such adhesions, or of a lateral origin of the pedicle, the ordinary forceps cannot be applied, Museux's forceps, or appropriate hooks, may be substituted to seize and bring down the tumor after the os uteri is sufficiently dilated.

Besides the fibroids referred to, there are other tumors in the cavity of the uterus which are entirely concealed, and, either from their diminutive size or a very adipose abdomen, cannot be detected. The cervix uteri may be intact, and there may be no bulging of the uterine wall recognizable by the finger in the vagina. Yet the symptoms may be so diagnostic of their presence that we are bound to be governed by them alone. In such cases we must dilate the cervical canal by sea-tangle, sponge tents, etc., until we are able to pass the index-finger and explore the interior of the uterus, and get access to the tumor. The dilatation must now be carried further, so as to admit, in addition to the finger, the necessary instruments for the removal of the growth. These are usually a strong, slender forceps, or hook, and a long-handled, probe-pointed bistoury, or a probe-pointed, semicircular knife. A small wire *écraseur*, passed above the forceps, will answer the same purpose. If the tumor have a small pedicle, it may be removed by torsion.

(3) *Interstitial Submucous Fibroids.*—These are tumors which, originating in the wall of the uterus, develop mainly towards the mucous surface. Like the two preceding forms, they more urgently demand the surgeon's attention in consequence of inducing uterine hemorrhage. When the cervix is not implicated in this class of tumors, and the body of the uterus alone is involved, it is best not to be too precipitate in resorting to surgical means for relief. As long as hemorrhage can be controlled, and blood saved to the patient, it is best to depend upon the judicious employment of ergot and other remedies, rather than to resort to a hazardous use of the knife. A tumor thus situated, with muscular walls on one side of it and a mere mucous covering on the other, will be in a favorable condition for ergot to gradually expel it from its muscular bed, and force it into the uterine cavity, and, in proportion as this is accomplished, we secure the aid of all the longitudinal fibres of the uterine walls in propelling the growth against the circular fibres of the cervix, and in causing a gradual opening of the os uteri—thus finally reducing this form of tumor to the second variety before considered.

If, however, exhausting hemorrhages cannot be controlled, and fatal consequences threaten, other and more decided measures must be adopted. The os and cervix should be well dilated, so as to get free access to the interior of the uterus, and to be able to command the surface of the tumor. As soon as this is done, efficient doses of ergot must be administered, and, when its action is established, a long, probe-pointed bistoury, guided by the index finger of the left hand, should be passed up flatwise into the uterus, as in using a sound, and carried over the face of the tumor until it reaches its upper border. An assistant should now grasp the lower part of the abdomen and steady the uterus. The bistoury is then turned with its edge against the tumor, and boldly pressed into it and drawn downwards through its dense tissue, so as to slit open both its mucous covering and its proper coat, burying the knife well in the fibrous mass, and carrying it downwards until it makes its exit at the lower border of the tumor. As the knife passes through the dense tissue,

a decided grating is perceptible. It is a remarkable and interesting fact that, if hemorrhage have existed before this incision, it will cease after it has been made, and it will sometimes be found that when nothing else will arrest hemorrhage, a free use of the knife will accomplish it. Immediately after the section, an attempt should be made to enucleate the tumor from its bed, which can often be accomplished if the tumor be not too massive. Should it not be removed at once, it is apt soon to become disorganized and dead, throwing off a very offensive, dirty, watery discharge, and endangering septicæmia. Ergot must be continued, to maintain the contraction of the uterine tissue, antiseptics administered by mouth, lungs, and vagina, and the patient sustained until the tumor is removed by the surgeon, or is discharged in a softened mass resembling wet tow. The danger to be apprehended after this operation is blood-poisoning from dead matter. This is very much diminished when the tumor is removed by one operation, but, even then, the extent of raw surface remaining after the tumor is taken away, sometimes causes toxæmic symptoms.

(4) *Interstitial Fibroids Proper.*—These are tumors which originate deeply in the substance of the uterine wall, and during their growth and development are surrounded by a pretty uniform coat of uterine or muscular tissue, neither impinging on the mucous lining on the one side nor on the peritoneal covering of the organ on the other. As a general rule, the demand for active treatment in these cases is not as great as in the submucous varieties. These intra-mural tumors, however, are the very ones most amenable to the mode of treatment that has recently claimed the attention of the profession, viz., the hypodermic use of ergot. I can understand how the tonic contraction induced by ergot can impede nutrition in a tumor so located, but I cannot conceive how absorption could be promoted under a similar amount of compression obstructing the absorbent vessels. In the treatment of interstitial fibroids I think it best to use ergot periodically and not constantly; employing it in anticipation of and during the menstrual periods, and associating with it the continuous use of an alterative sorbefacient, such as muriate of ammonia. If ergot acts mechanically in destroying the life of a tumor, then, when that is accomplished, the stress or pressure should be removed, so that this dead matter may be absorbed. As long as this dead material is hermetically encased in living tissue, unaffected by atmospheric influences, it is not likely to become offensive or toxic, and may be carried off by the absorbents like other molecules of dead matter in the regular processes of repair, without infecting the general system. As there is a tendency for fibroids to increase in size before each menstrual period, in consequence of the greater determination of blood to the organs of generation, so there is a diminution in their size after menstruation subsides. Hence ergot is calculated to act beneficially, when given in time, in arresting this supply of blood to the tumor; and yet we must not always attribute the diminution in size, which usually follows menstruation, to the influence of ergot, as it is a natural physiological sequence.

These interstitial fibroids often assume a great size, and in doing so stretch the cavity of the uterus to an unusual length, increase the vascularity of the organ and the size of the vessels pervading the corresponding mucous covering, and, during the menstrual period, these unsupported vessels are rendered turgid and congested, and, giving way, produce copious and dangerous hemorrhages. When this is the case, and danger cannot be averted by medication, surgical means are demanded. A bold, free inci-

sion through the muco-muscular envelope of the tumor, and through its proper coat, burying the knife into the fibroid substance, will be attended by an arrest of the bleeding. This may be followed by an enucleation of the tumor—the several steps of the subsequent operation being the same as detailed under the head of Interstitial Submucous Fibroids.

Sometimes the interstitial fibroid, even when very small, may produce the most violent symptoms, both local and reflex, leading even to death; and may be so situated that, under certain circumstances, its existence may not be suspected, and that, even if correctly diagnosticated, it could not be removed by any ordinary operation. This is particularly the case when the tumor originates in the lateral part of the fundus uteri and becomes intra-ligamentous. Such cases, fortunately, are extremely rare, and I will endeavor to illustrate them by the detail of one which recently proved fatal:—

Mrs. D. G. S., aged 34 years, was brought from the interior of the State to Philadelphia, and placed under my care. She was small in stature, but excessively corpulent, particularly in the abdominal wall. For a long time she had suffered the greatest agony and distress, and had had frequent and most violent convulsions. All the symptoms were distinctly traceable to the uterus. There was decided endo-metritis, and the slightest touch of the sound would cause terrific spasms. There was also extreme tenderness confined to a small spot in the hypogastric region, on the right of the linea alba, so that she could not tolerate the most gentle pressure. The treatment during the winter of 1874–75 relieved the endo-metritis and several of the reflex symptoms, and, although she had not fully convalesced, she went home with the expectation of gradually regaining her health. But she again became worse, and her husband, an intelligent physician, conducted the treatment, but without controlling the symptoms. I visited her several times during the winter of 1875–76, and I never saw a greater sufferer. The spot in the hypogastrium was the point of trouble. She had not been able to lie down for months, and the right thigh was kept flexed all the time. Her suffering was so constant and intense that both she and her husband begged me to open her and remove the womb. She required the constant use of the most powerful anodynes to afford the least comfort. Finally symptoms of cerebral congestion supervened, and in a few days after, on the twenty-third of March last, death came to her relief. In exposing the abdominal cavity after death, about four inches in thickness of adipose tissue had to be penetrated. The uterus was enlarged and highly congested. Its cavity was three and a half inches long. The lining membrane was normal. In the right wall of the fundus was an interstitial fibroid, not over two inches in diameter, surrounded by a dense wall of uterine tissue, and so closely adherent to its capsule that it could not be shelled out or enucleated. The capsule had evidently been in a state of inflammation, and this was thought to be the cause of the intense and fatal suffering. Both openings of the Fallopian tubes were obliterated.

In reference to this interesting case I wish to state that I had appointed the very day on which I witnessed the post-mortem examination to visit the patient to decide upon the feasibility of extirpating the uterus. In consequence of the extreme amount of abdominal fat, as shown by the examination, this would have been very difficult, if not impracticable. For the same reason it was impossible to make a correct diagnosis of the case, and the fibroid discovered after death was not at all suspected to exist before. Of course the case might possibly have been made out by the barbarous practice of exploring with the hand through the rectum. If such an extreme case were again to present itself to me,

particularly in a thin patient, I should think it my duty to remove the diseased organ.

(5) *Recurrent Fibroids.*—The recurrent fibroid is polypoidal in character, and is supposed to be reproduced, after its removal, from the same pedicle. These cases are very troublesome, because, after one tumor is removed, there may be repeated recurrences of others, resembling in this respect, and in this only, the several forms of malignant disease. The operation for their removal is the same as for that of the ordinary uterine polypus, but, as this is a much more grave trouble, I wish to submit to the Section a proposition embracing more extensive surgical interference. This proposition is based upon the treatment of two cases of very great interest, which I beg permission to relate.

In the first case I removed eleven successive tumors, after which the patient recovered and had no return.

On December 27, 1860, I visited Mrs. T. W. S., aged 31 years. Menstruation for the first four years was small in quantity and free from pain, but afterwards was accompanied with extreme agony during the first day. She married at the age of 24 years, in one year after gave birth to a child, and had a miscarriage afterwards. The menses were regular, having only missed the last period. In April, 1858, she first discovered something wrong in not being able to pass water. A large quantity was drawn away by the catheter, which operation had to be repeated. Several physicians saw her, and diagnosticated malposition of the womb, and very violent efforts were made to restore it to its normal position, but without effect. June, 1860, she had a sudden attack of severe pain, which so entirely disabled her that she could not walk for several weeks, suffering much of the time with the most dreadful bearing-down pains. After a period of intermission the expulsive pains again returned with extreme violence, and, on November 28, 1860, while using the catheter, she felt a tumor in the vagina which she supposed to be the womb. The pain now diminished, but the size of the tumor gradually increased, and it finally protruded through the os externum. During the above period she had copious discharges from the vagina of bloody clots and a greenish, slimy, and offensive fluid, changing usually about twenty-five napkins in a day.

When I saw the patient she was perfectly blanched and anæmic, with a small thread-like pulse, and evidently in a state of toxicohæmia. A tumor, as large as a small adult head, protruded from the vagina, and was lying between the thighs. It was in a gangrenous condition, and gave off a most offensive odor. The index finger, passed into the vagina in front, came against the anterior lip of the os uteri about two inches up. It was stretched across the front face of the tumor, which was continued into the uterus as far as the finger could reach. The sound struck the fundus uteri one inch above the anterior lip. The vagina behind the tumor was also explored. A *cul-de-sac* was reached at the distance of one and a half or two inches, formed apparently by the inverted vagina coalescing with the tumor. The growth had clearly been developed in the posterior wall, and had been extruded by the uterine efforts from the cavity of the organ. The circulation was still maintained in the posterior portion of the mass. The pedicle was about two inches thick. With the assistance of Drs. Drysdale and Burpee, I removed the mass by means of the *écraseur*. Although much decomposed, its weight was one and a quarter pounds. For several weeks the patient continued to be very ill with septicæmic fever, but finally fully recovered.

April 17, 1862, the patient consulted me again, stating that she had remained well until three months before, when she had had a copious puruloid discharge, and that one week before seeing me another tumor had begun to protrude. On examination, I found that the relative positions of the tumor and uterus were now different. The former, instead of being behind the os tincæ, was anterior to it, and the expanded posterior lip could be distinguished in the hollow of the

sacrum. This second tumor increased to a considerable size, and was removed in the same way. At the same time a hard tumor was felt occupying the hypogastric region. A few weeks after the removal of the second tumor it was followed by a third, which was also taken away. A mass still occupied the hypogastric region.

The following December she was taken with excessive hemorrhage, requiring the use of the tampon. Subsequently quite a large tumor was expelled from the vagina, and at the same time the supra-pubic tumor disappeared. This, the fourth tumor, was removed by the *écraseur* on the 9th of January, 1863, and the operation was followed by a rapid recovery. June 1, 1863, another large tumor, the fifth, was removed in the same way. January 21, 1864, the sixth tumor, less in size, was removed. I could plainly trace this one into the cavity of the uterus, and the anterior lip of the os stretched over it, the sound entering anterior to the mass two and a half inches. At the same time I recognized a hard tumor in the right inguinal region. August 24, the same year, the seventh tumor was removed. The eighth, ninth, and tenth were subsequently removed up to the 20th of May, 1866. The eleventh tumor was taken away on the 18th of August, 1867, with the assistance of Dr. J. Nicolaysen, of Christiania, Norway, now Professor of Surgery in the University of Norway. After this no tumor could be detected anywhere.

After the eleventh operation there was no return, and the patient regained her health, at least for two or three years, after which I lost sight of her.

In reviewing the above case—its extraordinary features, its long duration, its imminent perils, and its almost endless recurrence—the question has often presented itself to me: Would not extirpation of the uterus at an early period have been not only justifiable but preferable?

A question also of great interest—the pathology of the so-called Recurrent Fibroids—might be discussed with profit, but this is not compatible with our present subject. Both questions will be in a measure involved in the detail of another case, which I think would have exhibited the same features as that above narrated, but which was terminated quickly and happily by a different mode of treatment:—

On May 15, 1874, Mrs. C. T. was brought from the interior of Pennsylvania for treatment. She was 30 years of age, had first menstruated at the age of 14 years, and had continued to be regular until her marriage in 1870. Two years before marriage she began to suffer from a burning sensation in the pelvic region, and from bearing-down pains, which gradually grew worse. Soon after marriage menstruation became more copious, and, eighteen months after, she was seized with great pain, accompanied with excessive flooding. Subsequently she was confined almost constantly to bed by copious hemorrhage, intense suffering, and constant nausea and vomiting. She had never conceived. Prostration, emaciation, and anæmia were very strongly marked.

The uterus was enlarged to the size of a four-months' pregnancy, the cervix fully expanded, and the os, round and ring-like, merely admitted the point of the index finger. A hard, smooth tumor could be felt within. I immediately put her on the use of iron and ergot, but, notwithstanding, she had an overwhelming attack of flooding and pain. By the 22d of June the condition of the patient and of the os uteri was such as to warrant me to proceed with an operation. Assisted by my son, Dr. W. Lemuel Atlee, I introduced a forceps into the cavity of the uterus, and, grasping the tumor, made traction while the patient was under the influence of an anæsthetic. As the os was not sufficiently open to allow the tumor to escape, it was nicked by the bistoury as it tightened over the descending mass, which finally was delivered into the vagina with a sudden slip, and afterwards through the os externum. The *écraseur* was now applied, but, proving to be faulty, was removed, and the pedicle was severed by the bistoury. While preparing for this part of the

operation, I was struck with what appeared to be an unusual elongation of the pedicle, which was dotted over with several molluscum-like bodies. Apprehending that this might be the uterus itself inverted, I was careful to make the section close to the tumor. Immediately afterwards I tamponed the vagina with strips of muslin wet with Monsel's solution of iron.

The operation was followed by intense pain, and in the course of two or three hours by hemorrhage, which was controlled by removing the first tampon and substituting small sponges. There were also incessant sickness and vomiting, which, the patient said, always followed the loss of blood or the use of anæsthetics. These symptoms were subdued by opiates in the course of twenty-four hours. The pulse increased in frequency, and the case looked rather alarming. Next day the sponges were removed, an examination was made, and a rough, irregular, tumor-like mass still occupied the vagina. I could not divest myself of the apprehension of its being an inverted uterus. The parts were so very tender that a satisfactory examination could not be made. This soreness continued for eight or ten days.

The regular menstrual period came on the first week in July, accompanied with very little pain and a moderate discharge. On the 13th I repeated the examination, and found the same state of things. The os grasped the tumor, and a sound passed up entered the uterine cavity the normal distance of two and a half inches. From this examination I inferred that there was no inversion, and that there still remained a portion of the original tumor. Pressure over the pubes did not enlighten me. Perhaps a finger in the rectum would have disclosed the true state of the case. I seized the body with a strong double tenaculum, brought it partially through the os externum, applied the *écraseur*, and on working the instrument found the tissue unusually resisting, requiring much force and time in getting through it, and indeed having to finish the operation with the knife. On examining the mass, after its removal, I was surprised to find it to be the body of the uterus.

[The specimen was then exhibited. The mucous surface was dotted over with several small tumors in different stages of development.]

Great pain and sickness of the stomach succeeded the operation as before, but did not last so long. Much less bleeding accompanied the amputation, and none followed it. The patient had a rapid and excellent recovery, grew fat and hearty, and, with the exception of slight coccyodynia, has been in the continued enjoyment of perfect health. Menstruation has not occurred since. The speculum reveals a perfect os tincæ, and the sound enters only one inch.

Although the treatment of the above case was ultimately conducted on an error of diagnosis, does it not teach a valuable practical lesson? If the history detailed to you this day explains and illustrates the pathology of the so-called recurrent fibroid of the uterus, and if each abnormal body on the surface of the womb is liable to be successively developed into a life-endangering tumor, will not the amputation of the body or the entire removal of such a diseased organ be the best mode of treatment, keeping in view the comfort and life of the patient?

II. Tumors usually not accompanied with Hemorrhage.

(1) *Interstitial Subperitoneal Fibroids.*—These are tumors having the same relation to the serous coat of the uterus as the interstitial submucous fibroids have to the mucous coat. They are imbedded in uterine tissue except where they present towards the abdominal cavity, and there they are covered only by peritoneum. Their locality renders them much less serious in their consequences to the patient, and does not call so

imperatively on the surgeon for interference. Still, circumstances might arise in which relief would be demanded. Treatment in these cases will accomplish a great deal, either with or without reference to an ultimate surgical operation. I have just hinted at the action of ergot in the submucous fibroids. It is the same, in an opposite direction, in the subperitoneal tumor. The tendency of any foreign body, when acted upon by the contractile efforts of uterine tissue, will be towards the point of least resistance. Hence ergot, acting forcibly on the muscular substance of the uterus, in which the fibroid is deposited as in a closely-fitting nest covered merely by a delicate serous membrane, must, as a necessary result, gradually diminish the bed of the tumor, and as gradually cause the mass to encroach upon the abdominal cavity. I have no doubt that nature herself sometimes converts this kind of tumor into a sessile or even pedunculated fibroid. And hence, by following her teachings, by means of ergot we are able greatly to aid her in accomplishing such results. Supposing, however, that this result has been reached, the tumor has not been destroyed nor diminished, but only changed in its locality, for I very much doubt whether ergot has influenced its growth in this situation. Still, our patient has been benefited by relieving a very sensitive and irritable organ of a troublesome tenant, and by placing the tumor in a position more accessible to the resources of surgery if the case should ever demand it.

In interstitial subperitoneal fibroids, it would be extremely hazardous to invade them by the knife from the interior of the uterus, and in this way attempt their enucleation or even disorganization and devitalization, as their thin serous coating would not be a sufficient barrier to protect the abdominal cavity from being entered, which would necessarily cause fatal results. It would be much safer, after the administration of ergot, to open the cavity of the abdomen, expose the surface of the tumor, divide its peritoneal coat with the knife, and enucleate the mass from its muscular bed. Should bleeding occur, the vessels could readily be secured as they are in ovariotomy, and to guard against oozing and accumulations in the abdominal cavity a siphon-drain could be carefully placed and brought out of the lower angle of the wound. At the same time, if deemed necessary, a communication between the bed of the tumor and the cavity of the uterus could be made, and thence through the cervical canal with the vagina, and a siphon introduced so as to assist in carrying off fluids also in that direction. These precautions would go very far in preventing septicæmia and fatal inflammation.

(2) *Sessile Peritoneal Fibroids.*—These are tumors which present bodily into the abdominal cavity, but are closely seated on the exterior surface of the uterus, and have a broad base. They do not sink deeply into the uterine tissue itself, and do not elongate the uterine cavity. They may be removed in the same way as the subperitoneal variety, but the same precautions need not be exercised with regard to the uterine bed which receives the tumor. Indeed, in both varieties, a peculiarly formed tube of silver, glass, or hard rubber, around which the detached coats of the tumor could be fastened or ligated, might be secured with one end in the bed of the tumor, while the other could be carried out of the lower end of the abdominal wound, thus acting as a siphon in drawing off the fluids, or as a conductor for the introduction of antiseptic washes from without. By the time the ligatures should have become loosened, the drainage tube could be removed, and a fistulous track would remain to fill up and heal by granulation and cicatrization.

In cases of ovariotomy, when complicated with small sessile fibroids, I have not hesitated to enucleate them before closing the incision, being careful, if there were any oozing, to carry a tent from the raw surface to the lower angle of the wound.

(3) *Pedunculated Peritoneal Fibroids.*—These are fibroids having a distinct pedicle, either membranous or myomatous, attached to the exterior surface of the uterus, and contained within the cavity of the abdomen. These tumors, being beyond the limits of uterine action, are not usually influenced by the use of ergot, but do sometimes yield to the sorbefacient action of muriate of ammonia uninterruptedly maintained for a long time. If they fail to yield to medical treatment, and induce such severe symptoms as to destroy personal comfort or jeopard life, they may be removed, as are ovarian tumors, by abdominal section, and the pedicle secured by the clamp or ligature. The operation need not be described. I may, however, remark that, in proportion to the size of the tumor, the incision through the walls of the abdomen must necessarily be larger than that required by ovariotomy, and that the pedicle will need more careful attention. Should the pedicle be short, we can add to its length by incising the proper coat of the tumor around its base, two or three inches beyond the stem, and shelling it off and making it part of the pedicle. I have several times removed these tumors in this way.

(4) *Interstitial Cervical Fibroids.*—These tumors occupy the vagina, but are not ejected from the cavity of the uterus, and consequently are not pedunculated, and are not capable of being treated in the same way as uterine polypi. They are surrounded by the tissue of the cervix, and are imbedded within it. When small, resembling a bullet submerged in the cervical structure, a simple incision carried through the tumor, so as to bisect it, will often be sufficient to lead to its decay and destruction. To make its disorganization more certain, the surfaces of the cut fibroid may be touched through the speculum with a pencil of vegetable caustic or of *potassa-cum-calce;* or, better, the small mass may be entirely enucleated by the point of the finger after opening its capsule. When the tumor is large, swelling out a portion of the cervix to the size of a walnut or an orange, it will be necessary to make a free slit in its envelope and enucleate the mass at once. These tumors are so accessible to the finger that this procedure can readily be accomplished, provided their capsule has never undergone inflammation. In all of these cases the vagina should afterwards be kept carefully cleansed by antiseptic washes, and the inflammation of the cervical mucous membrane, which usually accompanies these tumors, should receive proper treatment.

(5) *Myomatous Degeneration of the Uterus.*—The uterus sometimes seems to be overwhelmed with fibroids filling up the interstices everywhere, and even to harbor at the same time every variety of tumor heretofore considered. When such a condition exists, it is not to be expected that medicinal means alone will be of much use, nor can the surgical treatment of each, individual, diseased mass be contemplated, but if anything is to be done, it must be the entire extirpation of the affected organ. As these cases cannot always be clearly diagnosticated, the surgeon must be prepared to vary his original course of action during the progress of an operation. He may commence with the expectation of removing only a pedunculated fibroid, or even an ovarian tumor, but will sometimes end by extirpating the entire uterus and its appendages. I will relate the following case:—

On August 8, 1875, I visited, with Dr. Kurtz, Mrs. J. A. D., of Reading, Pa. She was 44 years old, had menstruated at 15 years of age, and had always been regular. When 27 years old she had married, and had had two children, and one miscarriage between them. In 1872 menstruation had become more free, and in the summer of 1874 she had had two or three attacks of bleeding during the menstrual intervals. In 1872 she had first noticed a tumor in the right groin. It was hard and movable, and fell over on turning her body. For two years its increase in size had been gradual, but during the last twelve months very rapid. As the tumor enlarged, her general health failed, and she rapidly emaciated.

At the time of my visit, the abdominal enlargement was equal to that of an eight-months' pregnancy. It was irregular in shape. There appeared to be two tumors, or one with a deep sulcus dividing it. The mass was hard, but slightly elastic, and upon the left side of its fundus there was a harder knob the size of a walnut. The two lobes were movable on each other. The uterus occupied the back part of the pelvis, admitted the sound two-and-a-half inches, and was not much influenced by manipulating the abdominal growth. While the sound was in the uterus, I discovered still another and much harder mass in the superior strait of the pelvis, which I diagnosticated as an intra-mural fibroid, but had some doubts in consequence of the abdominal enlargement having masked the examination. The diagnosis was obscure, but I inclined to the opinion that the abdominal tumor was pedunculated, and hence rather favored an operation.

The patient's health continuing to fail, she solicited relief. September 1, 1875, I visited Reading, and met Drs. Kurtz, Brooke, Ulrick, Dundor, Kuhn, Coblentz, Cleaver, Weidman, Brodhead, and student Ellis Kurtz. The division of the tumor had now disappeared, and the mass was more consolidated. An incision seven or eight inches long was made below the umbilicus through a very attenuated abdominal wall. On exposing the tumor, the characteristic mahogany color at once disclosed its uterine nature, and further examination developed its real character: fibroid degeneration of the body of the uterus. There being no adhesions, the whole mass was easily rolled from the cavity of the abdomen. The body of the uterus was now found to be occupied by intra-mural fibroids, while the cervix was normal. Both ovaries were seated on the lower portion of the enlarged uterus; the right one being healthy, and the left cystic, and as large as a walnut. A strong needle was now armed with a double ligature of strong plaited English silk cord, and with it the cervix uteri was transfixed just above the insertion of the vagina and below the ovaries. Each ligature was securely tied on its respective half of the cervix, and each was again made to encircle the whole cervix and firmly secured. The tumor, consisting of the uterus and both ovaries, was now cut away, leaving a large button beyond the ligature. To make it doubly safe the stump was embraced in Atlee's clamp, and maintained outside. The cervical canal was plainly seen on the face of the cut pedicle, into which a probe was passed about three-eighths of an inch, and a finger introduced into the vagina entered the os tincæ and felt the grasp of the clamp. The return circulation of the tumor, which escaped on excising it, was prevented from entering the abdominal cavity by protecting sponges. The wound was closed with iron-wire sutures. The patient made an excellent recovery, and now enjoys good health.

In considering the propriety of extirpating the uterus by abdominal section, we must judge each case by itself. The condition of the pelvis, vagina, and cervix uteri, is of primary importance. If the cellular tissue is indurated in consequence of inflammation or other cause, or the vagina shortened and rigid, or the cervix occupied by fibroid deposits, it will not be a case favorable for operation. If, however, the pelvis is free from morbid deposits, the vagina long or extensible, and the cervix in a normal state, the chances of life may be considered almost equal to those of ovariotomy.

(6) *Fibro-cystic Tumors of the Uterus.*—The fibro-cystic tumors of the uterus, as far as my observations go, are developed upon the exterior surface of the organ, and are usually pedunculated. They are constituted of fibrous tissue, and are more or less cancellated in structure, and in consequence of their greater impressibility are called soft fibroids in contradistinction to the hard tumors heretofore considered. They grow sometimes to an enormous size. These tumors, in my opinion, are entirely beyond the influence of ergot, while they seem at times to be controlled in their growth by muriate of ammonia. I have had no experience in the treatment of uterine fibroids by means of galvanism, but I should suppose that this would be the kind of tumor most amenable to such an agent. There are, however, surgical means, both palliative and radical, which I have employed with benefit. The palliative treatment consists in penetrating the abdominal wall with a long, large-sized trocar and canula, thrust into the centre of the tumor, withdrawing the trocar and passing a strong sound through the canula, and with it breaking up the interior, cancellated structure in every direction, and drawing off all the fluid possible. Usually after such a proceeding the tumor will diminish in size to a considerable extent. The operation, at proper intervals, may be repeated. As this treatment is original with myself, I must ask permission to illustrate it with a case:—

On April 22, 1863, I was consulted by Mrs. A. E. S., of Washington City, D. C. She was 42 years old, had commenced menstruating at 14 years of age, and had been married at the age of 17. She had had three children, the youngest then 18 years old. About four weeks before seeing me, she had been seized with very acute pain in the right groin, which lasted for twenty hours, and was only relieved by strong opiates. This was followed by an enlargement of the right side. She was a fine, healthy-looking lady, disposed to corpulency, and exhibited no unusual abdominal prominence. An elastic, cyst-like, smooth tumor was detected occupying the hypogastric and right inguinal regions, and rising above the umbilicus. It was at that time diagnosticated to be an ovarian tumor, and she was advised to let it alone.

September 24, 1868, she consulted me again. The abdomen then was very much larger than that of a woman at the full period of pregnancy. It was smooth, elastic, compressible, semi-fluctuating, and of uniform shape. The uterus was normal in size, and movable, independently of the mass. In company with Dr. David Burpee, I passed the large trocar into the tumor below the umbilicus, but no fluid escaped. The sound was then introduced through the canula, and passed round in several directions. It was very evident that in manipulating with the sound many fragile septi were broken up, which proved to be the walls of cells containing fluid. Two or three gallons of fluid, stained with blood, were removed in this way, and it coagulated on exposure to air. The operation was followed by no unpleasant symptoms, and afforded great relief. Up to this time the case is reported in detail in my work on the Diagnosis of Ovarian Tumors, page 285. Subsequently the tumor was invaded in the same way at distant intervals three times, and each time with beneficial results—the last being in March, 1871. I saw the patient in November, 1872, when her general health was good, and her size very much diminished, although the tumor was still there. During all this time she had used the muriate of ammonia internally, and the combined treatment had had a very happy effect on her condition. Letters from her assure me that she still remains in comfortable health, that the size of the tumor is not nearly so great, and that its further development is probably arrested.

I have treated other cases of fibro-cystic tumor of the uterus in the same way with similar results, but have never entirely cured them by

these means. In one case, I cut up the interior structure with a small knife introduced through the large canula, and afterwards injected acetic acid. This was followed also by a decided diminution of the tumor, and by no unpleasant consequences.

The only radical cure for these tumors I believe to be extirpation. In their removal, the surgeon must keep in mind their peculiar pathological relations with the uterus and with the peritoneum, as these differ materially from those of ovarian tumors. As far as my observations have gone, the fibro-cystic tumor originates from the wall of the uterus at the junction of the cervix with the body, at or just below the point where the peritoneum is reflected from the uterus to form the recto-vaginal *cul-de-sac*. As the tumor enlarges it raises the pelvic peritoneum above it, and, dissecting it from its natural attachments, elevates it into the abdominal cavity far above the pelvis, so that a real peritoneal *cul-de-sac* almost surrounds the enlarged tumor between the umbilicus and pubes. The tumor now is covered by a peritoneal coat above, while its pelvic portion is free from peritoneum. The reflected peritoneum, therefore, acts like a coronary ligament in binding the lower portion of the tumor in the pelvis. To illustrate the operation, I will relate the case of Miss E. E., aged 57 years. The same case is detailed for another purpose in the work referred to above, page 279, but the manner of operating is omitted:—

On June 5, 1868, in the presence of Drs. Mitchell, Brinton, Thompson, and Keen, and assisted by Drs. Drysdale and Burpee, an incision seven inches in length was made through the linea alba down to the tumor. Passing my hand through the incision, I found the tumor to be adherent to the abdominal wall; but these adhesions were readily broken up. I now particularly explored the interior of the left side, in the previously ascertained locality of the elevated uterus, but soon encountered an additional tumor, resembling an hypertrophied spleen and filling up the whole left side from the hypochondrium down into the iliac fossa. In carrying on the investigation downwards, over the large tumor, my hand was interrupted in its descent by a *cul-de-sac* of reflected peritoneum, extending around the tumor midway between the umbilicus and pubes, and firmly fixing it in position. It was, therefore, plain that the fundus of the tumor was covered by a coat of peritoneum which had been lifted from the pelvis by its development upwards. The membrane was vascular and thickened. I next cut boldly into the tumor, in the line and to the extent of the external incision, and, passing my hand into the gap thus made, broke up the whole interior of the mass, freeing it of one or two gallons of yellowish fluid, which coagulated on exposure to air. After thus reducing the size of the tumor, I succeeded in shelling off the whole peritoneal coat, to which were attached several folds of intestines, and by this means relieved it of all points of attachment in the abdominal cavity. The entire mass was now readily dislocated and turned out of its bed. It was found to be attached by a fibrous pedicle, an inch to an inch and a half thick, to the uterus on the right side, at a point between the insertion of the vagina and that of the uterine peritoneum. The pedicle was short, almost sessile. It was transfixed by a strong double ligature, which was tied both ways and also encircled the whole pedicle. The tumor was then excised, so as to allow a large button beyond the ligature.

The remaining tumor now presented itself at the opening. It sprang from the posterior surface of the left broad ligament just where it joins the posterior part of the uterus. The Fallopian tube coursed along in front of the pedicle, was wrapped around it, and its fimbriated extremity rose up on the tumor, and was attached to it. Several adhesions to the walls of the abdomen were broken up, and the tumor was turned out. The pedicle was very short, but was clamped, the Fallopian tube being included, and the tumor was removed.

The abdomen was afterwards carefully cleansed with soft sponges, as well as the pelvis, which was nothing but a raw surface having no peritoneal lining. The right ovary was found to be healthy, the left being absent and evidently involved in the small tumor. A large amount of loose peritoneum, which had been removed from the large tumor, now occupied the lower portion of the abdominal cavity. It had contracted very much, and appeared more thickened. It was allowed to arrange itself. The uterus was enlarged to more than twice ts normal size. On its anterior wall was a small sessile subperitoneal fibroid. I opened its capsule, enucleated, and removed it.

The clamp was arranged across the middle of the wound, the ligated pedicle in the lower end, and five wire sutures were introduced above and five below the clamp; the fundus uteri occupying a position against the interior surface of the wound between both pedicles. Several ounces of blood were lost, but not sufficient to affect the pulse. One vessel was controlled by torsion, and one by silk ligature, cut short and dropped in. The large tumor was fibro-cystic, weighing thirty-five pounds. The small one was an ovarian fibroid, and weighed over five pounds. The patient made an excellent and rapid recovery, and is now in the enjoyment of perfect health.

In presenting this subject for discussion, I am aware that I am open to criticism for having made such an arbitrary subdivision of fibroid tumors of the uterus. It is, however, apparent that this has been done for practical reasons. A much simpler classification would have been into subserous and submucous, or *extra-muscular*, and interstitial or *intra-muscular*. But as the question to be considered was the *treatment* of fibroids, I preferred arranging these tumors as we find them in patients, so as to make the paper purely clinical and practical. In making the above division, it must not be inferred that each case can be classified under one or other head, for several or even all forms of fibroids may be present at the same time. According to my observation, however, the intra-muscular tumor is most likely to be solitary, while the extra-muscular is more frequently multiple.

As a consequence of the limited time allotted to this paper, and which has already been exceeded, I have viewed this subject only from a personal stand-point. In omitting to mention the various modes of treatment of which I have no experience, I mean no disrespect or disparagement to any of my medical brethren, who, perhaps, may have had better success than myself. These methods, it is hoped, will be stated by their advocates. With regard to the use of one agent, which has recently been extensively used, I may be excused for repeating that I commenced the use of ergot in 1845 in the treatment of fibroid tumors, and have continued to employ it beneficially ever since. I have, also, for many years, used muriate of ammonia alone, and in connection with ergot, and with such results as have satisfied me of its value. The dose is ten grains three times a day. A solution of one drachm to one pint of water is used twice a day as a wash to the abdomen and to the vagina, and an apron of silk oil-cloth is worn next to the skin. Neither patient nor physician should tire in its use, using it for months and even years, if the tumor does not disappear. I am satisfied that it often arrests the growth; so does ergot. It sometimes causes its entire disappearance; I have yet to see this result from ergot alone, except in mechanically expelling polypi. It sometimes fails to produce any appreciable results; so does ergot. I attribute the influence of muriate of ammonia to its alterative, resolvent, and sorbefacient power, as well as to its alkaline properties. I am well aware that the remedial power of muriate of ammonia has been

denied on the ground that inflammatory or plastic deposits have been mistaken for fibroids. I do not pretend to possess any more diagnostic tact than others, but the same remark will apply to all other remedies and to all other persons. Even if it accomplishes so much under an error of diagnosis, it certainly is a great boon to suffering humanity.

I now present the following propositions, which will throw the whole subject of treatment open to discussion:—

I. Fibroid tumors of the uterus, although frequently unaffected by treatment, are sometimes cured, and often controlled in their growth, by medicinal and other agents.

II. Fibroid tumors of the uterus which are entirely harmless to the patient, should never receive surgical treatment involving the least danger.

III. Extra-muscular, submucous tumors usually demand surgical treatment.

IV. Hemorrhage, caused by a fibroid, uncontrollable by other means, may be, and should be, arrested by the knife.

V. In the surgical removal of fibroids, it is of the greatest importance to extirpate the living tumor.

VI. When the living tumor cannot be removed, as is usually the case when the capsule has undergone inflammation, antiseptics must be diligently employed.

VII. Extra-muscular tumors of the subperitoneal variety, when involving health and life, should be removed by abdominal section.

VIII. In cases of recurrent fibroids, it is justifiable, under certain circumstances, to amputate the inverted uterus, or to remove the organ by abdominal section.

IX. In exceptional cases of uterine fibroids, extirpation of the uterus by abdominal section is warranted.

X. As a general rule, in all cases of fibroids which destroy the comfort of a patient, or hazard life, and are not amenable to other treatment, surgical interference is to be commended.

Discussion on Dr. Atlee's Paper.

After the reading of the preceding paper, the President, Dr. Robert Barnes, of London, said:—This is a very important subject, and one upon which we need all the light possible to get. We need not care so much about the successful cases as about the failures, and also the reasons for the failures. We should not rush into an operation when there is no hope of benefit. From the distinguished physicians present, I think that some ideas may be developed in a surgical way that will be of use to humanity.

Dr. John L. Atlee, of Lancaster, said:—I believe that abdominal section for the removal of fibroid tumors has been decided upon as a necessity in some cases. I once met with a case in which the enlargement of the abdomen was equal to that of an eight months' pregnancy, and was hard on the right side. I diagnosed an ovarian tumor. Two days afterwards the operation was performed. On opening the abdomen I encountered a dark-brown cyst, such as I had never noticed in cases of the kind. I drew off six pounds of fluid, which looked like an infusion of coffee-grounds, and succeeded, with some difficulty, in surrounding the pedicle with a clamp. On examining the tumor, I found it to be of unusual character, and uterine. It weighed, in addition to the six pounds of fluid drawn off, about as many pounds of a solid mass. Hemorrhage had occurred in the tumor on one side, and had bent it over the

uterus until it was of a half-moon shape. I confess that I was disappointed in my diagnosis, but the patient did well. If through an error in diagnosis I was successful in removing such a tumor, I do not see why it is not a legitimate operation to perform abdominal section for fibroid tumors, as well as in ovariotomy. In another case there was an ovarian cyst on the right side, and a subperitoneal uterine fibroid weighing six pounds on the left. Both pedicles were included in the same clamp, and the woman did well.

Dr. Alexander Dunlap, of Springfield, Ohio, said:—The treatment of fibroid tumors is now on trial before the profession. I have been watching the question for thirty years, and my experience does not entirely coincide with that of the author of the paper which has been read. Many tumors require no operation at all, for after attaining a certain size some disappear, while others stop growing, and give us no further trouble. I think that if the mind be withdrawn from the diseased organ, in nine cases out of ten the tumor will remain quiescent and give no annoyance. I have known of cases in which the tumor was as large as a uterus at full term, without giving much inconvenience. Fibroids often cause hemorrhage, a thing to be guarded against. I have found that ergot contracts the muscular coat, and cuts off the source of blood from the tumor. I differ also as to methods of operation. Dr. Atlee uses the knife to remove the tumor; I prefer the *écraseur*, because it prevents hemorrhage. In some cases enucleation is effective, but I have lost two cases from want of proper drainage, septic inflammation having supervened. I would propose a new method of dealing with the pedicle in cases requiring the removal of the uterine tumor. This consists in dissecting the peritoneum off of the lower half of the tumor, and in removing the latter by the *écraseur* very close to the os internum. Strong silk threads are then passed at various points through the edge of this cup-shaped peritoneal covering. Next the cervical canal of the uterine stump is dilated, and a small speculum introduced per vaginam. Through this the ends of the silk threads are passed, and the peritoneal coat is drawn by them through the uterine canal into the vagina. Thus no raw surface can remain within the abdominal cavity, for the stump is covered by peritoneum, and all discharges become external. In some cases of hemorrhage I have found it of advantage to lay the patient on an inclined plane, with the head low down, so that the tumor can be pressed from the pelvic into the abdominal cavity; the proceeding is very easy in these cases. I have never been able to stop the growth of a tumor by ergot, but it is of use in checking hemorrhage.

Dr. G. Kimball, of Lowell, Mass., said:—I concur in the opinion of Dr. Dunlap, that these tumors should be let alone, except when it is absolutely necessary to use the knife. They sometimes disappear spontaneously, and it requires good judgment to tell what cases need operation. Such as do not involve the vagina or the neck of the uterus are the only ones that can be operated upon. A surgeon would be almost guilty of homicide who should attempt an operation in some cases in which the fibroid fills the vagina and invades the cervix, and in which hemorrhage would inevitably result, for there would then be no pedicle which could be ligated. In one case I removed the womb on account of a sessile subperitoneal fibroid, and secured the pedicle with a silk ligature the ends of which were brought out externally. For eighteen months the ligature ends remained *in situ*, and then disappeared internally, as the patient averred. Instead of the ligature I now use the wire *écraseur*, which I fasten externally by adhesive straps, after searing the stump with the hot iron. In this manner I am able to control all hemorrhage without having much of a pedicle left. Very few cases authorize extirpation of the uterus. I have seen good results from ergot, but none from the muriate of ammonia in subperitoneal fibroids. But one of my cases got entirely well after using for a year intra-uterine injections of a solution of eight grains of iodide of potassium in an ounce of water.

Dr. E. R. Peaslee, of New York, said:—Fibrous tumors should be consi-

dered apart from other tumors. They either produce hemorrhage or they do not, and this depends greatly on their site. When there is no hemorrhage I do not consider that we have any right to interfere. In the treatment of these growths, I have used the muriate of ammonia with good effect, but I do not think that we can expect much from ergot, except in cases of interstitial tumor. It might possibly affect the uterine muscles, and cut off the nutrition of a subperitoneal fibroid, but I have little faith in most of the ways suggested for its use. I would reject the use of ergot entirely except in cases in which contraction was an absolute necessity. There is no hemorrhage so great that it cannot be stopped for a time, and this time is important, as it gives a chance to think whether the case really needs a surgical operation. Women very rarely die from hemorrhage in these cases, but more frequently from septicæmia. In ordinary cases of hemorrhage I give ergot in combination with the tincture of the ferric chloride. In severe cases I introduce a tent into the cervix, and keep it *in situ* by means of a vaginal tampon. When septic symptoms show themselves, I consider repeated, intra-uterine, disinfectant injections as of paramount importance. I regard it as a safe rule not to interfere with fibroids. In regard to cutting into a fibroid, I have found that an incision through its mucous capsule is valuable in arresting hemorrhage, but I have never used enucleation because I am afraid of subsequent bleeding. In such cases I use the ligature and antiseptic washes. I would suggest to Dr. Atlee instead of the term "recurrent fibroid," which he has used in his paper, that of "multiple fibroid." In the discussion of this subject, fibro-cysts should be separated from fibroids proper; the former generally spring from the posterior wall of the uterus, and drag the organ upward, elongate it, and bend it forward; this does not occur in cases of simple fibroma, and constitutes a diagnostic point. I do not think that we are justified in removing a fibroid unless the patient is in such a condition that she will die without the operation. With fibro-cysts, the operation is often necessary, and if, in a case of ordinary fibroid, life were threatened, I should not hesitate to extirpate the uterus.

Dr. H. Lenox Hodge, of Philadelphia, said:—No reference has been made to what I consider very important, namely, accurate statistics of operations for the removal of these tumors by abdominal section. It would be of great value if each surgeon would state how many times he had operated, in how many cases the operation had been successful, and in how many it had been fatal. This has been done in regard to ovariotomy, and the same reasons apply for doing it in this case. Dr. Péan, of Paris, has published such statistics. His results are very favorable. They as yet stand alone. No one else has approached the success which he reports. I have myself, as yet, never operated in a case of this kind, and have never met with a case which seemed to require it. On the other hand, it would also be of value to know the history of cases which have not been operated upon. I have seen many of these tumors of the uterus, and have the record of some of my father's cases, dating back many years, which are still under my observation. In my experience, the number of deaths resulting from such tumors when let alone is very small; and, as the patient approaches the age of fifty years, the tumors often begin to diminish. In some of my cases, no trace of the old tumors can now be detected. One of the dangers, dreaded by some, is inflammation, resulting in septicæmia; this I remember to have seen in only a single case. Another danger is hemorrhage; this I have always been able to control. I have seen very good results follow the use of hypodermic injections and suppositories of ergot. The ergot, according to my observation, acts well in all the varieties of these tumors, including the subperitoneal. I believe that it acts by contracting the bloodvessels, rather than the muscular tissue, of the uterus. There is a remedy, however, which is far superior to ergot. It is a surgical one, and consists in pushing up the tumors out of the cavity of the pelvis, and maintaining them, by proper pessaries if need be, above the brim of the pelvis. This relieves the dysuria and tenesmus so common from pressure of the tumor on

the bladder and the rectum; but more than this, it lessens in a most remarkable manner, or arrests, the hemorrhage. The cause of this hæmostatic effect is somewhat obscure and difficult to explain. It seems probable, however, that there being no longer pressure from the firm pelvic walls, the circulation becomes unembarrassed, and, the blood returning by the veins, the congestion of the uterus is diminished.

The President, Dr. ROBERT BARNES, of London, said:—We must have adequate reasons for these operations. The question is, whether we should allow patients to die, or use all the means at our command to save life. This discussion will be a help to all of us. In my own practice I last year lost three patients from a sort of necrosis which supervened upon the incision of the capsule of submucous tumors, and an attempt to enucleate them. This unfortunate experience has made me very shy of undertaking any such procedure unless the whole tumor can be removed at one operation. If the tumor were too large to be approached by the vagina, I should advocate gastrotomy as soon as the woman's life was threatened.

Dr. ATLEE, in reply, said:—I beg to return thanks for the attention given to my paper. All of the points which I have advanced have been sustained by the very distinguished gentlemen who have taken part in the discussion. Physicians are bound to aid a patient when in extremity, and should employ the means which they know to be of use; we have no right to let a disease alone; our business is to relieve the patient.

THE THREE MOST IMPORTANT OBSTETRICAL INSTRUMENTS.

BY

T. LAZAREWITCH, M.D.,

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[COMMUNICATED BY THE SECRETARY.]

A BLUNT hook,[1] a parallel forceps, and an embryotome, devised by Prof. Lazarewitch, and presented by him to the Congress, under this title, are offered to the Section on Obstetrics.

The Blunt Hook is bent at both ends in the same direction, and has a hole drilled in one end, and two holes in the other. Apart from the uses to which the ordinary, obstetric, blunt hook is devoted, this one possesses the important advantage of enabling the practitioner to replace a prolapsed funis, and to carry a fillet around the thigh of the child, or a noose around its ankle.

The Parallel Forceps weighs only one pound, has narrow blades, is but eleven inches in length, and when closed gives a divergence of two and a half inches at the hollow of the blades. The handles lie parallel to one another, and are locked by lateral contact. The lock, consisting of a tenon and mortise, is so loose as to permit freedom of movement to the child's head. By the shortness of its handles, this instrument cannot exert much compressive or extractive power. To compensate for this deficiency, a strong silk loop is attached, either just above the lock when compression is needed, or at the ends of the handles when merely powerful traction is called for. By this loop, traction can be made also in any direction, and the forceps converted into a lever whose fulcrum is represented by the hand at the handles. The inventor attributes the following advantages to his parallel forceps:—

"Either half may be introduced first with equal facilities for locking.

"After the first blade is introduced, the way is not obstructed against the second, as it is with the crossing forceps, which is especially important in cases in which the head is high up in the pelvis, and the vagina imperfectly dilated.

"The locking is effected with perfect ease, and without risk of pinching the soft parts of the mother, which are quite clear of the lock.

"Whatever force of traction may be applied, in all cases immediate pressure of the head with the blades may be avoided on account of this possibility of accommodating themselves to the head and the mother's parts. This cannot be so perfectly effected by any other forceps."

The Embryotome consists of a hollow shaft, armed with a steel beak at one end, and with a screw at the other, which opens and closes the beak. The instrument is introduced closed. The beak is then opened

[1] [An abstract, only, of Professor Lazarewitch's paper is here inserted. The original manuscript, with elaborate illustrations and samples of the instruments described, is deposited in the Museum of the Obstetrical Society of Philadelphia.—EDITOR.]

and made to bite into the cranium. By turning the shaft on its longitudinal axis a portion of the skull is then torn off. By repetitions of this process the whole vault is broken up. For this instrument, Prof. Lazarewitch claims not only the property of lessening the bulk of the head, but that of acting as an efficient tractor, and of being available for severing the neck, or any portion of the body, in a difficult cross-labor.

ON THE NATURE, ORIGIN, AND PREVENTION OF PUERPERAL FEVER.

BY

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THE total number of deaths in the city of New York for the nine years from 1868 to 1875, inclusive, was 248,533. Of these, 3342 were deaths either from diseases complicating pregnancy, from the accidents of child-bearing, or from diseases of the puerperal state; or, in other words, very nearly one in seventy-five of all the deaths occurring during that period, was the result of the performance of what we are in the habit of regarding as a physiological function.

A careful study of the records preserved by the Health Department of the city has enabled me to classify these 3342 deaths into:—

(1) Those due to child-birth proper, under which head I would place deaths from shock, from protracted and unusually severe labor, from convulsions, from flooding, from rupture of the uterus, and, for the sake of convenience, those from extra-uterine pregnancies;

(2) Deaths from eruptive diseases, from phthisis, and from the various non-puerperal, inflammatory disorders complicating child-birth. A few cases recorded as typhus, typhoid, and intermittent, I have included under puerperal fever, on account of certain inherent difficulties in the differential diagnosis. Should exception be taken to this action, I have only to state that the entire number thus recorded is much too small to palpably affect any conclusions which this report may contain;

(3) Deaths from a variety of diseases, occurring during pregnancy, in which the pregnant state was, in all probability, an active cause of the unfavorable result;

(4) Deaths from miscarriage (I use the term as I find it employed by the majority of physicians, in their reports, to designate all cases of interrupted pregnancy during the first six months), in which either intercurrent affections, the lack of proper medical assistance, actual want, or criminal malpractice, have been the chief factors in determining the fatal issue. To be sure, in many of these instances, death is due to the supervention of peritoneal complications, septicæmia, and pyæmia, assimilating them closely to cases of puerperal fever. I do not, however, include them under puerperal fever, because their origin is not involved in mystery. Two words tell the whole story, viz., neglect and violence. The explanatory term, retained placenta, testifies to the one, and the certificate of the coroner to the other. We all know how common fatal cases of metria are, in large hospitals, after delivery at full term. After abortions, on the contrary, they are rare. Thus the uterine wards of the Bellevue Hospital have always contained a large complement of women, who have entered the hospital in a very low state induced by some of the sequelæ of abortion. Of these, in nine years, two only have died. Dr. George K. Johnston

reports in seven years, in the Rotunda Hospital in Dublin, but one death in 234 cases of abortion, and that the result of mitral disease of the heart. I have, for the sake of convenience, exhibited the precise proportion of deaths due to each of these several causes in tabular form. Collectively they will be seen to have numbered 1395 cases, or about 42 per cent. of the entire number.

TABLE I.—*Deaths occurring during pregnancy, confinement, and the puerperal state, in New York, between the years* 1867 *and* 1875, *inclusive, excluding those from metria.*

	1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	Total	Total from all causes.
Childbirth[1]	18	13	20	20	37	38	22	19	16	203	
Convulsions	55	36	47	46	54	62	30	51	27	408	
Uræmia[2]	...	4	8	3	5	5	7	29	22	83	
Flooding	16	35	13	25	20	24	20	25	26	204	
Placenta prævia	...	4	10	5	11	9	14	11	18	82	
Protracted & instrumental labors	4	5	4	2	...	...	7	10	20	52	
Rupture of uterus	4	4	3	4	5	5	6	5	11	47	
Extra-uterine pregnancy	1	1	...	5	3	...	1	4	...	15	
Miscarriage[3]	13	10	12	19	36	27	24	30	26	197	
Pregnancy[4]	4	2	4	1	5	5	3	2	5	31	
Phthisis	3	2	1	2	6	8	8	4	5	39	
Smallpox	...	...	...	...	1	9	2	...	7	19	
Scarlatina	...	...	...	...	2	4	1	4	2	13	
Erysipelas	...	...	...	...	...	...	1	...	1	2	
Total	118	116	122	132	185	196	146	194	186	1395	3342

[1] Under *childbirth* are included deaths from shock, from intercurrent non-puerperal diseases, from complications, such as tumors, cancer, disease of the heart, and the like, and all cases reported under childbirth which proved fatal within twenty-four hours of delivery, not elsewhere mentioned.

[2] A large number of deaths from *uræmia* are reported, in which it is not stated whether convulsions existed or not.

[3] Under *miscarriage* are reported all cases in which pregnancy was interrupted previous to the seventh month.

[4] Under *pregnancy* are given deaths, from non-puerperal causes, in which pregnancy existed and probably contributed to the fatal issue. The number of deaths from phthisis is specifically given in the report, as are those from smallpox, scarlatina, and erysipelas, subsequent to 1870.

(5) There thus remain 1947 cases which, variously reported as puerperal fever, puerperal peritonitis, metritis, metro-peritonitis, phlebitis, phlegmasia dolens, pyæmia, and septicæmia, furnish the subject for special inquiry in this paper. All these varieties we will term briefly puerperal fever, attaching to the term, however, no special significance. The diagnosis of anatomical lesions from clinical symptoms alone, unverified by post-mortem examination, is, in the most experienced hands, only approximative. As usually reported by the practitioner, these lesions reflect for the most part his individual theory, or early training. As a matter of convenience then we propose to employ the name puerperal fever as a general term to cover all the febrile conditions peculiar to the puerperal state.

In the 1947 cases of puerperal fever, I have included cases of patients reported to have died of puerperal mania, the non-febrile form rarely proving speedily fatal. That this rule is not universal, I readily admit. Thus, I remember a case occurring in the Bellevue Hospital, in which the

maniacal patient was confined in one of the cells of the hospital. During the night, she got out of bed and broke the windows. The night was bitterly cold. The patient sat up in her night-dress with the cold air streaming in upon her. On the next morning she was seized with pneumonia, and died. A few cases of death, too, reported from insane asylums do not belong in this category. Giving to the term, puerperal fever, then, this broad significance, we find that in nine years it was the cause of nearly one one-hundred-and-twenty-seventh of all the deaths occurring in the city. It is to be regretted that the returns of births in the city are imperfectly rendered. The actual number of births during the last nine years may, however, be roughly set down at 284,000. This estimate is based upon the assumption that the natural birth-rate is thirty-three to the thousand,[1] an estimate which errs upon the side of liberality. The total number of deaths then to the entire number of confinements would be at least in the proportion of one to eighty-five, or, from puerperal fever alone, in the proportion of one to one hundred and forty-six. These figures proclaim more eloquently than any words can do, the necessity of frequently reviewing the state of our knowledge as to the nature of puerperal fever, and of inquiring as to how far it is due to causes which are amenable to human control.

At the outset, it is proper to recall that, as the various names applied to puerperal fever indicate, it is, with rare exceptions, associated at some period of its progress with certain inflammatory processes which have their starting-point in the generative apparatus. The lesions most commonly found in post-mortem examinations consist of catarrhal inflammations of the mucous membrane lining the vagina and uterus, with simple ulcerations, or those presenting a diphtheritic character, inflammation in the pelvic cellular tissue, metritis, local and general peritonitis, inflammation of the lymphatics and veins, thrombosis, pleuritis, pericarditis, valvular affections of the heart, abscesses, and other destructive processes in the parenchymatous organs, and purulent collections in the joints. As a rule, the occurrence of one or other of these inflammatory processes is synchronous with the first outbreak of fever.

When we come to analyze our cases of puerperal fever, we find two general groups, the one *infectious*, the other *non-infectious*.

I. *The Non-infectious Group.*—Every one who has seen much of hospital practice, has of necessity observed instances of puerperal inflammations and febrile conditions, often of a severe type, which have possessed this distinctive peculiarity, that they have in no wise visibly affected the health of puerperal patients in their vicinity. They possess this feature in common, viz., that in them the symptoms of blood-poisoning are absent, or are present only to a subordinate extent, and as a late feature of the disease. The nature of cases belonging to this group may be best expressed by an examination of the known causes which call them into existence. I give the following from a vast number of recorded cases, without pretending, however, that they exhaust the list:—

(1) Inflammations may arise from traumatic injuries, such as rupture of the cervix uteri during labor, those resulting from unskilfully performed operations, and from the bruising or crushing of the soft parts of the mother in long labors, especially in cases of moderate pelvic deformity.

[1] The census for 1870 gives to New York a population of 942,292. The average of annual increase from 1860 to 1870, was 15,000. I have taken 15,000 as the probable average increase since 1870. The inaccuracy in this calculation is not considerable enough to materially affect the proportions.

(2) The imprudence of patients or their nurses. Thus I have frequently been able to trace, in hospital practice during the winter season, severe cases of cellulitis, pelvic peritonitis and general peritonitis, to the patient's getting out of bed, dripping with perspiration and clad only in a night-dress, and going thus bare-footed over a cold uncarpeted floor to the water-closet. Even in private practice, like instances of wilfulness are not unknown; nurses too, especially those who are reputed by the fair sex to know more than any doctor, are fertile sources of trouble.

(3) Moral causes are frequently effective in producing serious disturbance. In studying the records of Bellevue Hospital for the year 1873, I was greatly impressed by the fact that in 240 deliveries in married women there were but two deaths from puerperal fever, while in 209 in non-married women there were thirteen deaths. From the Bureau of Vital Statistics I learn that one-eighth of the deaths from puerperal causes occur among unmarried mothers. Dr. Johnston dwells much upon a similar state of affairs existing during his mastership at the Dublin Rotunda Hospital. In a paper on "The Genesis of Puerperal Fever,"[1] I reported an instance in which five women were confined in the same ward, by the same attendants, and under absolutely the same conditions, with the exception that one chanced to witness the sudden death of a patient from rupture of the uterus. While the other four women did well, this one, after a period of great mental agitation, developed peritonitis and died. It has been suggested that such cases occur only in the records of hospital practice. This is not so. During the past summer I saw in consultation, in a remote and very healthy rural district in which puerperal diseases were unknown, a case of severe fever, lasting eight days, which was traceable to the mother's anxiety and fret over the illness of one of her children. Dr. Alfred Wiltshire, of London, after writing to me an account of a poor woman who died in less than twenty-four hours in collapse, brought on by a rough remark from her husband, adds, "I have seen many other similar though less severe cases, and I confess that my sympathies are deeply touched whenever I have to attend women who have cause for depression during labor."

(4) I have, in a number of cases, had occasion to notice the influence of old adhesions existing between the serous coverings of the pelvic organs, due to a by-gone peritonitis, upon the pregnant and puerperal states. The two following instances will serve as illustrations:—

The first was that of a young wife, who, a few months after her marriage, had a miscarriage. Unfortunately a portion of the ovum remained in the uterus, where it became the source of protracted hemorrhage and some febrile disturbance. A couple of months later, however, the offending body was removed, after preliminary dilatation of the os, by a distinguished physician who was summoned in consultation. A sharp attack of pelvic peritonitis followed, from which, however, the patient so far recovered as to enjoy a period of reasonable health. Then she became again pregnant. In the sixth month of gestation she began to lose flesh rapidly, and had repeated chills, with profuse perspirations, and a continuously high temperature with evening exacerbations. Living at the time, however, in a New Jersey rural district, her symptoms were attributed to malarial influences. I was consulted concerning the case in the eighth month, and on hearing the history predicted an inflammation of the kind under consideration; and found, as I had anticipated, on examination, a large and distinct tumor reaching from the left iliac fossa to the upper margin of the kidney, which was evidently the product of chronic

[1] American Journal of Obstetrics, vol. viii., Nov. 1875, p. 371.

cellulitis and peritonitis. Soon after, she gave birth to a premature but living child. In a few days subsequent to her confinement, the lady died. Every circumstance in the clinical history pointed to the connection between the slowly developing inflammation during pregnancy and the earlier acute attack of peritonitis.

In the second case I was called to see a patient suffering from puerperal convulsions, in whom gestation was complicated by a large fibro-cystic tumor of the uterus. After administering chloroform, I dilated the os, and delivered the child by version. Hemorrhage followed, which was, however, promptly arrested. The three days following, my patient had no bad symptoms. On the fourth, she complained of pain on the left side, which gradually extended over the abdomen. Meantime the temperature rose, the pulse became rapid, and on the following day she died. The post-mortem examination showed no lesions of either the uterine cavity, the cervix, or the vagina, which were all normal in appearance. That portion of the uterus, however, which had undergone fibro-cystic degeneration, and which was found to weigh between six and seven pounds, was covered with fibrinous flakes. Everywhere the tumor was glued to the adjacent serous surfaces by adhesions of long standing, and strong, organized bands, many of them the size of a goose-quill, connected it with neighboring organs. There could be no doubt, on examining these appearances, that the peritonitis had been caused by the dragging of the heavy tumor upon these adhesions, after the evacuation of the uterus and the loss of the support of the abdominal walls.

(5) It is proper to take into consideration the vulnerability of individuals. All puerperal women are said to tremble in the balance between a physiological and a pathological condition, and it is no unfounded assertion to say that trivial causes which would be inoperative in many cases will turn the scale in others. We see this exemplified in first births, and in women who have borne children in excess. In this connection, the question started by Dr. Nathan Allen, of Lowell, as to what is "The Normal Standard of Woman for Propagation,"[1] becomes of real interest.

(6) It is to be borne in mind that the zymotic fevers, not peculiar to child-bed, may provoke in the puerperal woman the same inflammatory lesions that are commonly associated with puerperal fever.[2] This is in conformity with the well-known surgical experience that a febrile paroxysm from any cause exerts an unfavorable influence upon a wounded surface. Olshausen[3] has, however, recently shown that pelvic inflammations and peritonitis are somewhat rare in cases of scarlatina complicating the puerperal state. He likewise adds affirmative evidence to the view advanced by Braxton Hicks,[4] that scarlatina, after a long period of latency or incubation during pregnancy, can make its appearance two or three days after delivery, and, as a rule, with slight throat symptoms. The discussion in the London Obstetrical Society[5] has done much to draw attention anew to the effects of infective diseases upon the puerperal state. I should prefer, however, to exclude well-defined cases of typhus, typhoid, and scarlatina, complicating child-bed, from the

[1] Amer. Journ. of Obstetrics, April, 1876, p. 1.

[2] Vide Hervieux, Traité Clinique et Pratique des Maladies Puerpérales; Art. Maladies Accidentales, pp. 1073 *et seq.*

[3] Untersuchungen ueber die Complication des Puerperium mit Scharlach und die sogenannte "Scarlatina Puerperalis;" Archiv für Gynækologie, Bd. ix. Heft 2, 1876.

[4] Contribution to our Knowledge of Puerperal Diseases; Obstetrical Transactions, vol. xii. p. 44.

[5] Obstetrical Transactions, London, vol. xvii.

category of puerperal fever, unless coexistent local lesions should involve the pathogeny in doubt.[1]

Now as regards the foregoing dangers to the puerperal woman, it is rare that the physician is enabled to employ preventive measures; and these are necessarily not general, but those suggested by his tact, skill, and wisdom, for independent application to each case.

II. *The Infectious Group.*—The infectious form of puerperal fever is characterized by blood-poisoning. The poison is of a septic nature. The usual points of introduction of the poison are the lesions of the parturient canal. This does not, however, exclude other points of entry, and clinical experience renders it highly probable that, under certain conditions, the poison may be primarily introduced into the blood through the respiratory and digestive organs. This form is contagious, but all cases of infection are not contagious in an equal degree. It is customary for many to limit the term puerperal fever to the members of this group; as, however, there is as yet no common understanding of authors among themselves, or among practitioners in general, as to the precise limitation of the term; as, indeed, hardly any two physicians mean precisely the same thing when they speak of puerperal fever, I shall endeavor to avoid confusion by not using the term in a specific sense.

That the infectious diseases of child-birth are of septic origin, there is now abundant evidence. The question of the identity of puerperal fever and septicæmia is largely one of definition. It is a matter of ordinary experience that the retention of a small bit of the membranes within the uterus will produce fetid lochia; and, as the result of infection, a febrile condition, which, as a rule, subsides with the expulsion of the offending body and the use of disinfectant washes. A virulent form of fever is not unfrequently occasioned by retained coagula, or placental *débris*, which have undergone decomposition. I was once sent to see a puerperal patient, suffering from fever, on the fourth day following her confinement. On entering the room I found the stench intolerable; turning down the sheets, I discovered that the patient was lying in a decomposing mass, and learned that her doctor had forbidden, after the birth of her child, the removal of the soiled linen and blankets. The patient died in the third week from pyæmia multiplex.

Haussmann[2] reported a case of auto-infection in the rabbit, which terminated fatally. A portion of the membrane, retained in the left cornu, led to diphtheritic losses of substance in the lower portion of the vagina, to hemorrhagic enteritis, and to peritonitis. The same author produced death from septicæmia by injecting into the gravid uterus of the rabbit serum from the abdomen of a rabbit which had died from infection. The post-mortem examination showed the muscles filled with granules, and the peritoneum injected, but no fibrino-purulent exudation. Injections into the uterus of pus from the abdomen of a woman dying from infectious puerperal disease, produced no effect upon rabbits two weeks gravid, while in the second half of pregnancy premature delivery and

[1] There does not appear to be any very good reason why a well-marked case of scarlatina occurring in a puerperal patient should be termed puerperal fever, more than one of smallpox. Dr. George K. Johnston informs me that the cases of scarlatina occurring in the Rotunda Hospital of Dublin have never started epidemics of puerperal fever among the lying-in patients. I remember but one case of the kind in the Bellevue Hospital, and this did not affect unfavorably patients in the vicinity.

[2] Entstehung der übertragbaren Krankheiten des Wochenbettes; Beiträge zur Geburtshülfe und Gynækologie, Bd. iii., Heft 3, S. 345.

death occurred, in one case in one and a half, in another in two and a half days. In the animal which died in thirty-six hours there was commencing perimetritis and peritonitis, while in the one dying after the lapse of sixty hours the abdomen was found to contain fibrin and pus.[1] D'Espine injected into the uterus of a rabbit, which had just produced her young, pus from the abdomen of a woman who had died from puerperal disease two days before. This was subsequently followed by other injections of fetid fluids during the four days following. On the twelfth day the animal died. The autopsy revealed peritonitis, most marked in the pelvic cavity, inflammatory alterations in the vagina, uterus, and tubes, small abscesses in the body of the uterus, softened clots in the veins of the broad ligaments, and infarctions of the liver.[2] Schüller found that subcutaneous injections of septic material in female animals, during pregnancy, produced a diphtheritic, ulcerative process on the uterine surface, which determined the separation of the placenta; diphtheritic patches, likewise, were found in the cornua of the uterus.[3]

Thus we find that in the human subject, and in experiments made upon animals, septic poisons introduced into the system following or near delivery produce lesions similar to those found in puerperal fever. As a further coincidence, we notice that, as in puerperal fever, the lesions from direct, septic poisoning have nothing characteristic about them, producing in one case pyæmia, in another partial peritonitis, in another general peritonitis, in another diphtheritis, while in others the lesions are comparatively trivial—these differences being due to differences in conditions which are but imperfectly understood.

My friend, Prof. Barker, in his magnificent work on "The Puerperal Diseases," which is, as every one knows, a masterpiece in the way of clinical observation, mentions, as a distinction between septicæmia and puerperal fever, that, while puerperal fever is contagious, septicæmia is a non-contagious affection. I have the record of the following case which militates against this theory. A physician in New York delivered a woman of a putrid fœtus. The woman died of septicæmia. Two days after attendance upon the first case, he attended a second patient, who likewise died of septicæmia. Dr. Barker states that by inquiry among surgeons he cannot learn that septicæmia follows in their wake as does puerperal fever in that of the obstetrician, and yet Sir James Simpson asserts it to have been the case in his younger days.[4] The vital statistics of New York, for the last nine years, show that a sequence of deaths following confinement in the practice of individuals has become very rare, and, let us hope, that if the enlightenment of these later days has enabled the surgeon to avoid such a misfortune, in the end the same profit may accrue to the practitioner of midwifery.

But it is to be remembered that the conditions in a puerperal patient, and in one upon whom a surgical operation has been performed, are not identical. Samuel, in speaking of immunities and dispositions to septic poisoning, says:—"The statistical frequency of septic, puerperal

[1] Ibid., p. 394.

[2] Contribution à l'Étude de la Septicémie Puerpérale; Paris, 1873, p. 28.

[3] Experimentelle Beiträge zum Studium der septischen Infection; Deustche Zeitschrift für Chirurgie, Bd. vi., S. 141.

[4] "I have repeatedly heard of instances of a rapid succession of surgical fever cases and disasters in the practice of the same surgeon, while the other surgeons in the same locality had their patients recovering as usual." On the analogy between Surgical and Puerperal Fever; Simpson's Obstetric works, edited by Priestley and Storer, vol. ii. p. 19.

diseases is due to the length of the parturient canal, to the fact that through this long passage there must pass all the pathological and physiological excretions, and to the soiling of these parts with fingers, instruments, and secretions, which have become the bearers of sepsis."[1] He found, on the other hand, that it was extremely difficult to produce a progressive ichorous condition by daily painting an open stump with a septic fluid,[2] though the same was readily obtained when an infinitesimal quantity of septic fluid was injected underneath a fascia. Dr. Barker is, however, much too keen an observer to ever go far astray, and it is true, as will be mentioned hereafter, that there is a form of septicæmia which is not contagious. The only question to be raised is as to whether it is allowable to restrict the term to this form. As usual in all questions concerning puerperal fever, there is practical agreement as to facts, with divergence of opinions concerning definitions.

Until very recently the whole subject of septicæmia has been in a state of wellnigh hopeless confusion. From Gaspard and Panum, through a long list of experimenters, hardly any two have arrived at precisely similar results. Something like an approach to order has, however, been produced since it has begun to be understood that the effects produced by septic fluids vary with the quality of the poison and the method of experimentation, and that to obtain identity in the result, there must be identity in all the conditions. Thus Samuel has shown that the same organic substance produces different effects at different stages of decomposition; again that the enteritis which is commonly quoted as characteristic of septic poisoning, occurs, as a rule, in animals, when the septic fluid is injected directly into the blood, and is rare when it finds its way into the circulation through the lymphatics, as is the case usually in clinical experiences.[3] There is one experimental point of extreme practical importance too in connection with puerperal septicæmia, viz., that if the injection of a septic fluid be made directly into a vessel, toxic effects speedily follow, but are transitory, unless the amount of the fluid be large, or its virulence exceptional, or the animal very young,[4] whereas very small amounts injected subcutaneously, by developing rapidly spreading phlegmonous inflammation, resembling malignant erysipelas in man, are capable, after a period of incubation, of producing fatal results; or they may, if injected into a shut cavity or underneath a fascia, lead to the development of an inflammation of an ichorous character. In other words, the eliminating organs suffice, under ordinary conditions, to remove from the blood the same amount of septic fluid which would prove fatal if injected into the tissues.[5] To produce similar results the injections into the blood need to be repeated at intervals. This experience leads us to the conclusion that, in the tissues, septic poison possesses the capacity of self-multiplication, and that, in the local inflammation set up, a reservoir is formed from which poison is continuously poured into the circulation.

This capacity of self-multiplication, which septic fluids possess, has

[1] Ueber die Wirkung des Faülniss Process auf den lebenden Organismus; Arch. f. exp. Pathologie, Bd. i. S. 343.

[2] Loc. cit., p. 339.

[3] Loc. cit., p. 349.

[4] Traube und Gscheidlen, Versüche ueber Faülniss und den Widerstand des lebenden Organismus. Schles. Ges. f. vaterländische Cultur, Febr. 13, 1874.

[5] In some instances in which absorption from the tissues is very rapid, the effects of subcutaneous injections may be similar to those produced by injections made directly into the circulation, and the local lesion be insignificant.

recently been found to be coincident with the presence of certain organic bodies, termed variously micrococci, microspores, or sometimes, less specifically, bacteria. All carefully made experiments serve to show that, if a septic fluid be deprived of these organic bodies by boiling, or filtration, while it continues capable of producing inflammation, the inflammation is usually of diminished intensity, and remains local in its character;[1] whereas the microspores, retained upon the filter, possess all the virulent properties of the original fluid.[2] This does not alone necessarily prove that the virus resides in the microspores, for it does not exclude the possibility that both the virus and the microspores remain upon the filter.

So far, attempts at isolating the microspores and cultivating them separately in vehicles, composed of water holding in solution certain inorganic constituents necessary for their healthy nutrition, have been only partially successful in proving them to be the sole source of infection. Some experiments of Tiegel and Klebs[3] were attended with positive results, but Hiller arrived at different conclusions. He found that bacteria washed in pure water were innocuous.[4] But pure water had long before been proven by observers to be inimical to the well-being of the organisms in question. Schüller says that Hiller's experiments prove apparently that while a putrid fluid may be in the highest degree poisonous, its component parts, viz., either the fluid or the bacteria singly, are neither deadly nor poisonous.[5] The fact is that all isolation experiments are subject to what seems an unavoidable source of error. As Davaine noted, early in his observations, the physiological action of bacteria is very dependent on the constitution of the medium in which they are developed, which is in entire harmony with what is known of organisms much higher in the scale. "Many plants," says Burdon Sanderson,[6] "containing active principles, become inert when transplanted from an appropriate soil." Bucholtz, in a series of experiments designed to test the influence of antiseptics upon the vitality of bacteria, found not only a difference between those taken directly from the infusion and those cultivated in artificial fluids, but between bacteria derived from the same source and cultivated in modifications of the nutrient medium.[7] Under these circumstances, all evidence of a positive character is to be regarded as of more value than that which is purely negative.

It is, however, from the constant presence of the round bacteria in infected wounds, and their distribution through the tissues, that the argument in favor of connecting septic symptoms with the bacteria has been mainly deduced. Here the ground is sufficiently solid, and, judged by ordinary laws of scientific evidence, the pathological importance of the microspores may be regarded as established. To be sure, we find them

[1] In filtration through porous earthenware cylinders, the filtrate possesses no phlogogenic properties.

[2] Tiegel, Correspondenzblatt für Schweizer Aertze, 1871, S. 1275. Klebs, Archiv für exp. Pathol. und Pharmakol., Bd. i. Heft 1, S. 35.

[3] Klebs, Archiv für exp. Pathologie und Pharmakologie; Beiträge zur Kenntniss der pathogenen Schistomycetin; Band iv., Heft 3, S. 241 und ff. Tiegel, loc. cit.

[4] Exp. Beiträge zur Lehre von der organisirte Natur der Contagion und von der Faülniss; Archiv für klinische Chirurgie, Bd. xvii. Heft 4, S. 669 u. ff.

[5] Exp. Beiträge zum Studium der septischen Infection; Deutsche Zeitschrift für Chirurgie, Bd. vi. S. 162.

[6] Lectures on "The relations of bacteria to disease;" British Med. Journal, March 27, 1875 See, also, Klebs, Beiträge zur Kenntniss der pathogenen Schistomycetin; Arch. für Pathol. und Pharmakol., Bd. iii. S. 321.

[7] Antiseptica und Bacterien; Arch. f. exp. Pathol. und Pharmakol., Bd. iv., Heft 1 und 2.

in tongue-scrapings of healthy individuals, but tongue-scrapings are poisonous if injected into the tissues. That they do not ordinarily prove so in the mouth, is no more singular than that woorara can be swallowed with impunity. Tiegel has endeavored to show that the round bacteria are found normally in the internal organs of the body.[1] If his experiments should in fact stand the test of criticism, they would only show that a few bacteria may be found in health in the liver and in the pancreas, but never in anything like the same numbers or the same general distribution, nor with the same characteristic groupings, that have been proven for a number of infectious diseases.[2] It is stated that they are sometimes absent from the blood taken during life in septic diseases. As, however, their constant presence has been confirmed in the vessels and glomeruli of the kidney, it is fair to assume that they are filtered out by those organs when the conditions favorable to their development do not exist in the blood. Again it is an open question, which awaits confirmation, whether it be not true, as Hueter claims, that the microspores do not disappear, but are taken up by the blood-globules, rendering the latter adhesive, and predisposing the blood to the stases characteristic of inflammation.[3] Zahn has shown that the inflammation in the mesentery of the frog, in the Conheim experiment, does not take place if the air is first filtered through diluted carbolic acid.[4]

As to the exact manner in which these minute bodies exercise their pernicious influence, whether they operate mechanically, or whether they produce a virus in the process of nutritive activity, or whether, as is probable, both suppositions are correct, we may safely leave as questions to be decided by subsequent investigations. It is enough for us to note that the connection between sepsis and the round bacteria is intimate and vital. Panum, who is often quoted as opposed to what is known as the Bacteria theory, admits as probable that the microsporon septicum is inoculable, appears in the blood during life, multiplies in the tissues, and, in part by production of a special poison, perhaps, and in part by mechanically irritating the tissues, excites inflammation, suppuration, and fever.[5] Bergmann, who once thought that he had found the secret of sepsis in a crystallizable substance derivable from putrid fluids, which he termed sepsin,[6] now squarely accepts the modern doctrine. Virchow has so far given in his adhesion to the new school as to say: "Especially in this connection are to be mentioned the diphtheritic process and the erysipelatous, especially erysipelas malignum. The granular deposit in diphtherically affected tissues, of which I formerly spoke, has more and more proven to be of a parasitic character. What we formerly regarded as simple, organic granules, as infiltration, or exudation, has since proven to be a dense aggregation of micro-organisms which penetrate into the tissues and cells to compass their destruction."[7] Even Billroth, who contends that what he terms a zymoid ferment is the first thing developed in the line of cau-

[1] Virchow's Archiv, Bd. lx. S. 453.

[2] Klebs., Arch. für Path. und Pharmakol., Bd. iii. S. 319.

[3] Allgemeine Chirurgie, Cap. xvii.; Der fieberhafte Process. So, too, Schüller, loc. cit. pp. 168, *et seq.* Birch-Hirschfeld likewise found bacteria in the white globules of pyæmia; Schmidt's Jahrbücher, Bd. 166, No. 5, S. 187.

[4] Arbeiten an der Berner pathol. Institut., 1871.

[5] Das putride Gift, die Bacterien, die putride Infection und Intoxication, und die Septicæmie; Virchow's Archiv, Bd. lx. S. 348.

[6] I have not been able to obtain access to Bergmann's original paper, but make this statement on the authority of Hueter, Allgemeine Chirurgie, S. 543.

[7] Die Fortschritte der Krieg's Heilkunde; Berlin, 1874.

sation, and that the bacteria are a sort of epiphenomenon, concedes that the organisms, by their migrations, may become the carriers of the virus into the interstices of the tissues.[1] I mention the less willing witnesses to the importance of bacteria in disease: I need not recapitulate the names of a host of active advocates of the germ theory.

I have been thus explicit regarding the evidence concerning bacteria in septic diseases, because it places the question of the infectious group of puerperal fever-cases in the following position:—Experiences occurring clinically, as well as those produced upon animals, teach us that certain lesions and symptoms, similar to those we are accustomed to regard as characteristic of puerperal fever, result from septic poisoning. In a large class of cases, however, the connection between child-bed fever and sepsis has been deduced rather from analogy than direct proof. For those who chose to regard such as due to a specific poison peculiar to the puerperal state, there was really no objection. If, however, round bacteria are characteristic of septic poisoning, the question presents itself in a different light, and we have to inquire whether, in the less obvious cases, bacteria are present in puerperal fever in the proportions and groupings that we find them in other diseases due to putrid infection. Now it is precisely proof of this nature that has recently been abundantly rendered.

Waldeyer,[2] Orth,[3] Heiberg,[4] and Von Recklinghausen, found the tissues and lymphatics of the parametria filled with pus-like masses, which consisted, in addition to pus-cells, chiefly of bacteria. Bacteria swarmed in the fluid of the peritoneal cavity. In one case examined by Waldeyer, six hours after death, while the body was still warm, the peritoneal exudation was like an emulsion, and furnished an abundant deposit which consisted almost entirely of bacteria. Orth injected ten minims of peritoneal fluid from a woman dead of puerperal fever into the abdomen of a rabbit. As the animal was dying he broke up the medulla oblongata, and found in the peritoneal fluid enormous quantities of these organisms. In puerperal fever, round bacteria have been likewise found, though in less quantities, in the lymphatics of the diaphragm, and in the fluids of the pleura, the pericardium, and the ventricles of the brain. In post-mortem examinations of fresh subjects, the serous fluids, withdrawn under proper precautions, do not contain round bacteria except in cases of septic infection.[5] Orth found in the purulent contents of the vessels of the funis, in children who died of sepsis, precisely the same formations as existed in the exudations of the mother.

The presence of these germs in puerperal fever serves not only to fix cases hitherto considered doubtful in the category of septic diseases, but it affords the best explanation of the protean phenomena of puerperal fever itself. Steurer, formerly *interne* at the Bellevue Hospital, where he witnessed the epidemic which prevailed in that institution in the year 1874, afterwards made, under the guidance of Prof. Von Recklinghausen, a special investigation of the pathological changes in a similar

[1] Untersuchungen über die vegetaliens Formen von Coccobacteria septica; Berlin, 1874, S. 200.
[2] Ueber das Verkommen von Bacterien bei der diphtheritischen Form des puerperal Fiebers; Archiv für Gynækologie, Bd. iii. S. 293.
[3] Untersuchungen ueber puerperal Fieber; Virchow's Archiv, Bd. lviii. S. 437.
[4] Die puerperalen und pyæmischen Processe; Leipzig, 1873.
[5] Klebs, Beiträge zur Kenntniss der pathogenen Schistomycetin; Archiv für exp. Pathol. und Pharmakol., Bd. iv. S. 441 u. ff.

epidemic which occurred in Strasbourg. From a written communication received by me from Dr. Steurer, many of the following facts concerning the pathogeny of the disease have been derived. These facts, I may add, are fully supported by the investigations of others, and form a most valuable contribution to our knowledge of puerperal fever.

Steurer's cases all presented diphtheritic patches about the vulva, or upon the mucous membrane of the vagina and uterus. These patches were always associated with a loss of substance, and were composed of disintegrated fibrin, white and red blood-corpuscles, and colonies of round bacteria in great abundance. From the patches, the bacteria could be traced between the muscular fibres, and deep down into the canalicular spaces of the connective tissue, where their presence gave rise to cellulitis. From the canalicular spaces they entered the lymphatics, with resulting lymphangitis. In many cases the lymphatics could be traced along the broad ligaments to the ovaries (puerperal ovaritis), and into the subperitoneal tissue of the lumbar region. By perforation of the walls of the lymphatics which directly underlie the peritoneum, they made their way into the peritoneal cavity and excited pyæmic peritonitis, an affection which differs from traumatic peritonitis, and for which the claim has been set up that it is peculiar to puerperal fever. The wide stomata upon the abdominal surface of the diaphragm allowed the facile entrance of the organisms into its lymphatics. Waldeyer found in diaphragmitis the lymphatics of the diaphragm filled with bacteria. And thus following the lymphatic system, if we only admit that the round bacteria are the carriers of sepsis, a fact which hardly admits of dispute, the frequency, in severe types of puerperal fever, of inflammations of the serous membranes—of the peritoneum, the pleuræ, the pericardium, and the joints—finds an easy explanation. We can understand, too, how it is not always altogether accident which determines in different cases the precise serous membranes which are affected.

The ductus thoracicus is the principal channel through which the poison enters the blood. Bacteria are not usually found in the blood during life. A few hours after death they swarm in that fluid. Possibly the rapidity of the blood-currents during life does not favor the multiplication of bacteria. That the bacteria do, however, enter the general circulation during life, is incontestable. Steurer writes, "As the kidneys are the great filters of the human system, I never neglected to examine them, and almost invariably found the glomeruli and arterioli filled with micrococci (round bacteria)." This is in correspondence with what occurs in other septic diseases, and accounts for the albuminuria and interstitial nephritis which often supervene in the advanced stages. We have seen already that, in consequence of septic poisoning, the white blood-globules have a tendency to adhere to the walls of the vessels. This leads to stases in the capillaries, to congestion of the deep-seated organs, and to an increase of blood in the large veins of the trunk. Finally death takes place from apnœa, partly from the inability of the blood-corpuscles to carry oxygen to the tissues, and partly from paralysis of the respiratory nerve centres.[1] Sometimes the bacteria pass directly into the veins, where they give rise to phlebitis. Prof. Von Recklinghausen recognizes three ways in which this may take place: (1) Through a thrombus; and here let me call to mind that it is very common in

[1] Vide Schüller. Exp. Beiträge zur Studium der septischen Infection. Deutsche Zeitschrift für Chirurgie, Bd. vi. Heft. 1 und 2, S. 149 u. ff.

uterine phlebitis to find the uterus large, and the vessels at the placental site filled with soft thrombi; (2) through direct perforation of the venous walls; (3) by being taken up by white corpuscles and by them conveyed into the vessels in the manner described by Cohnheim.

When the bacteria enter directly into the circulation, they sometimes, in passing through the heart, adhere to the endocardium and the valves, causing exudation, ulceration, and decomposition, and thus give rise to the so-called endocarditis ulcerosa puerperalis.[1] In the cases studied by Waldeyer and Steurer, there were diphtheritic patches, serving as the starting points of the puerperal processes. Whether these so-called diphtheritic patches are identical with those which appear in the throat, is an open question. Morphologically, they are so, but, in hospitals, epidemics of puerperal diphtheritis are not associated with throat diphtheritis.

To avoid misapprehension, let me distinctly state that diphtheritic patches are not necessary to the infectious form of puerperal fever. They indicate an unwholesome atmospheric condition, and are somewhat rare outside of public institutions. Orth and Heiberg noticed the same, general, post-mortem changes in those cases in which the patches were absent, as in those in which they were present. My own observations show that they are rarely developed in the early stages of a hospital epidemic of puerperal fever, nor are they to be found in all cases when such an epidemic is at its height. In some of the lying-in hospitals in Europe, puerperal diphtheritis appears, however, to be endemic.

The question as to the extent to which erysipelas and puerperal fever are cognate diseases, is in a fair way to be solved by recent investigation. Orth took the contents of a vesicle, from an erysipelatous patient, which contained bacteria in great abundance, and employed the same for injections under the skin of rabbits. In this way he succeeded in producing in these animals a species of erysipelas malignum. In the subcutaneous œdema, and affected portions of the skin, he found enormous masses of bacteria, so far exceeding in quantity the amount introduced as to prove an abundant new production.[2] Samuel produced similar results by the injection of ordinary putrid fluids containing round bacteria. An affection resembling simple erysipelas he obtained most frequently by the application of fluid to a wound torn open after the second or third day.[3] Lukowski found that erysipelas could be produced by fluid containing micrococci even when putrefaction did not exist. The contents of erysipelatous vesicles containing no micrococci, excited no morbid manifestations. Where the erysipelatous process was fresh and progressing, micrococci were found in great abundance in the lymphatics and canalicular spaces. Where the process was retrogressive, there were no micrococci to be found, even in cases in which inflammation existed to an intense degree.[4] Virchow's testimony we have already given.

Thus we find in surgical fever, in puerperal fever, in diphtheria, and in erysipelas, the presence of a common element which links them together, and which establishes the relationship which has long been recognized as existing between these various processes. Experiments made by competent men, with care and intelligence, serve continually

[1] Heiberg, Die puerperalen und pyæmischen Processe; Leipzig, 1873, S. 22 und 34. Gives references to cases reported by Wirge and Eberth.

[2] Untersuchungen über Erysipel.; Arch. für exp. Pathol. und Pharmakol., Bd. i. S. 81.

[3] Arch. für exp. Path. und Pharmak., Bd. i. S. 335 u. ff.

[4] Untersuchungen über Erysipel.; Virchow's Archiv, Bd. lx. S. 430.

to increase the probability that the bacteria are no chance products, but that they have a vital connection with the diseases designated. Whether these organisms are identical in the different infectious diseases in which they have been recognized, is another question. Billroth complains of the monotonous appearance of always the same forms.[1] When we bear in mind, however, that our best instruments fail to enable us to distinguish the ovum which is to produce a mouse from one that will produce a tiger, though the ovum is at least one hundred times larger than the micrococcus, the argument loses something of its value. Whether identical or not, they all possess the common property of penetrating the tissues, under favorable conditions, of multiplying, and of producing, by their migrations, local inflammations and general infection.

I cannot refrain, in conclusion, from quoting entire the following statement of Panum, which appears to reconcile certain differences in the definitions of the term septicæmia by different authors:—

"The putrid poison may, during life, enter the blood with or without bacteria, especially from wounds, and occasion all the symptoms of septic poisoning; whereas, however, the *bacterium termo* does not appear to occur in the blood during life. *This simple putrid infection does not appear to be inoculable.* Another, as it appears, distinct, specific, pathogenic fungus, the microsporon septicum of Klebs, developed especially in pus (and blood?), perhaps under the predisposing influence of the putrid poison, when the air (as in overcrowded hospitals) contains the latter, or when it is transferred by inoculation, seems on the other hand during life to increase in the blood and tissues, and, in part perhaps, by production of a special poison, in part, perhaps, in a more mechanical way, by penetration, and, under circumstances by its irritative action on the tissues, excites inflammation, purulence, and fever."[2]

Causes of Puerperal Fever.

The Atmosphere.—The effect of a poisoned state of the atmosphere is best observed in the so-called nosocomial malaria of hospitals. At Bellevue I have had frequent occasion to witness febrile outbreaks among the patients in the lying-in service, which were instantaneously arrested by closing the tainted ward and transferring the inmates to a healthy locality. As at these times the nurses, the bedding, and the utensils, remained unchanged, it is fair to assume that the previous, unhealthy condition was not due to the transfer of a poison from patient to patient by the attendants, but to something residing in the air of the vacated apartment. In the inquiry as to the production of this condition, it can be assumed that it is not caused by aggregation alone. The medical wards of Bellevue, always crowded, have been in time of need safe receptacles for lying-in patients. It certainly is not due to the presence in excess of what are generally regarded as the ordinary constituents of the atmosphere. We must, therefore, look for some additional element capable of unfavorably affecting the economy. When the disturbance produced by nosocomial malaria is not arrested by change of locality, and the golden moment is allowed to slip by, the secretions of the patients affected become inoculable. Under such circumstances the epidemic spreads rapidly, and assumes continuously a more and more severe type.

[1] Untersuchungen über die Coccobacteria septica, S. 3.
[2] Das putride Gift., etc., Virchow's Archiv, Band lx. S. 349. I have translated literally. The meaning of the sentences, in spite of the involved construction, is sufficiently clear.

If, during such an epidemic, the external genitals be carefully watched, diphtheritic patches may now and then be observed. At first these patches may not be of any special clinical importance. It is possible that they may rapidly clear off, and thus come to be regarded as of little consequence. When, at length, the epidemic has assumed a pestilential form, these patches, which may make their appearance in isolated cases at any time in a hospital, are rarely absent. I have already dwelt upon the composition of these patches, not because I believe that they are essential to puerperal fever, but because their presence tells the tale of what it is in the atmosphere which accomplishes the charnel-house work. Conditions have been present in the air to favor the multiplication of bacteria, and have fitted them to become the active producers of disease. Can we doubt this? First, the epidemic was mild. If a patient, however, died, her tissues and secretions were filled with bacteria, as has been described. Then the epidemic became virulent, and the lesions of the generative apparatus, especially of the external organs which were most exposed to the air, became covered with patches which were found to swarm with micrococci. I cannot, under the conditions named, but consider it more in accordance with ordinary scientific reasoning to conclude that the micrococci played an important part in the production of puerperal fever, than that the puerperal fever produced the micrococci.

To be sure, bacteria or their spores are always present in the atmosphere, and it may be fairly asked how patients are ever spared from their perverse industry. The answer is that they are not always equally active for evil. Bucholz found that the same bacteria, developed in Cohn's fluid, offered more resistance to carbolic and salicylic acids than those cultivated in an analogous fluid which he had adopted. Distilled water renders the action of bacteria extremely feeble. In experiments upon animals, the results obtained with septic fluids depend in no ordinary degree upon the age of the fluid, the material from which it is formed, and the conditions under which it is generated. Micrococci multiply in hospitals when organic materials favorable to their growth are present in sufficient quantities. Robin and others have demonstrated the existence of albuminoid matters in water condensed upon vessels containing freezing mixtures, and placed in overcrowded wards of hospitals. When the results of crowding become manifest, these albuminoid matters not only impart a peculiar fetid odor and putrefy with great rapidity, but rapidly impart putrefaction to normal blood and healthy muscle with which they are brought in contact.[1] Micrococci both cause putrefaction, and serve as the carriers of septic virus. Hueter found putrid blood a most favorable fluid for septic experiments. It was noticeable in Bellevue Hospital that febrile outbreaks always arose in, and were usually confined to, the ward in the hospital which, by a bad arrangement, was assigned to patients for the first four or five days following confinement, *i. e.*, during the period of the lochia cruenta. As puerperal fever is rare after the fifth day, this at first sight would seem natural. But if a patient was transferred directly after confinement, during one of these unhealthy periods, to the ward containing the patients who had passed the first five days, but had not completed the ten days, she would escape the fever. It was always the same ward that required to be disinfected. In a communicating apartment all the confinements took place; and at all times, therefore, the conditions were present for loading the atmosphere with

[1] Leçons sur les Humeurs; Paris, 1867, p. 195.

the products of decomposing blood. In the summer months, as long as the windows were open all the time, the patients enjoyed immunity from nosocomial malaria. In the early fall, as soon as it became necessary to close the windows partially on account of the cool nights, it was not uncommon for the more trivial disturbances, such as so-called milk-fever, the hospital pulse, and catarrhal affections of the genitalia, to manifest themselves. Through the months of February, March, and April, the mortality was usually greatest. During the winter months there was, as a rule, crowding of patients, insufficient ventilation, the saturation of the air with albuminoid materials chiefly derived from blood, which, under the furthering influence of the heat requisite to make the wards comfortable, entered readily into decomposition. That the latter winter months should prove the most perilous, is in accordance not only with the theory of continuous accumulation, but with the experimental fact that weeks sometimes elapse before a decomposing substance acquires the highest degree of virulence.

Apart from the nosocomial malaria of hospitals, there is reason to believe in the influence at times of certain general wide-spread atmospheric states which affect the entire community. In the year 1871, the mortality from child-bed in New York was 399; in 1872, 503; in 1873, 431; in 1874, 439, and in 1875, 420. Now the excess in the deaths for 1872 was due wholly to an increase in the cases of metria, those from ordinary accidents remaining nearly the same as in the preceding years. The disease certainly did not extend into the city from the hospitals serving as foci, for the mortality that year at Bellevue Hospital was hardly more than half the usual average. There was no especial mortality that year from either diphtheria, erysipelas, or scarlatina, but the aggregate mortality was the largest known in the history of the city. There are no positive data connecting the civil deaths from puerperal fever in 1872 with parasiticism, but the prevalence of epizootics, of epidemic catarrhal affections, of peculiarly fatal forms of pneumonia, and other diseases which are now attributed to the presence of minute organisms in the atmosphere, renders such a source highly probable.

It is proper to say here that, though the argument is very strong in favor of regarding the genitalia of puerperal women as the exclusive point of entry into the system of infectious materials, it seems impossible at the present time to make all the facts coincide with such a theory. I have the records of a number of cases occurring during an epidemic of puerperal fever, in which patients were either attacked with fever previous to parturition, or in whose cases the unusual length of labor, the frequency of post-partum hemorrhage, and the imperfect contraction of the uterus, immediately after confinement, were signs of some abnormal influence exercised upon the economy at an early period of labor, previous to the existence of traumatism. That deleterious materials may find other channels for entering the system than a wounded surface, is evidenced by the cachectic condition not unfrequently produced in physicians by too assiduous attendance in dissecting-rooms and places in which post-mortem examinations are conducted. One severe and rapidly fatal case of puerperal fever, which occurred in Bellevue Hospital, I find it impossible to attribute to any other cause than that the woman, for five months previous to her confinement, served as a helper in a lying-in ward. The post-mortem examination disclosed no special local lesions, but her symptoms were those of intense septicæmia. It does not yet seem quite time to give up the idea that, under exceptional circum-

stances, the respiratory, and probably the digestive, tracts, may allow the passage of materials of a septic character.

Inoculation.—Another and frequent source of puerperal fever is by direct inoculation. Any material of a septic character, introduced into the genital passages of a woman during or after confinement, may produce a general infection of the system. But the point upon which I wish especially to dwell is that it is possible to trace epidemics of puerperal fever directly to carrying puerperal poison from patient to patient, through the medium of attendants. In such cases, changes in wards and the most rigid sanitary precautions avail but little, as long as the affected *personnel* is continued in charge. Unless this fact is fully recognized, all the cleverest devices in hospital construction will fail to prevent the occurrence of disasters. In epidemics this source of danger is especially to be guarded against, as septic poison is increased in intensity by successive inoculations. Davaine[1] showed that when a number of animals were poisoned, the one from the other, while from ten to fifteen drops of putrid blood were required to produce death in the first animal, one ten-trillionth part of a drop was sufficient in the twenty-fifth animal of the series, and in puerperal fever epidemics a similar augmentation in the deadliness of the poisons generated by patients is observed.

The nurses in hospitals and in private practice are usually the carriers of contagion. In studying the records of New York City for nine years, I find, however, that the occurrence of two deaths from puerperal disease, following one another so closely as to lead to the suspicion of inoculation, occurred to thirty physicians; a sequence of three cases occurred in the practice of three physicians; one physician lost three cases, and afterwards two, in succession; one physician had once two deaths, once three deaths, and twice four deaths following one another; finally, a physician reported once a loss of two cases near together, then of six patients in six months, and then of six patients in six weeks. Thus in the practice of more than 1200 physicians, in nine years, I find, excluding cases occurring in hospitals, that the experience of thirty-six only lends color to the idea that puerperal fever is due to criminal neglect on the part of the medical profession. Undoubtedly in many of these cases, too, the responsibility is only apparent, as when a practitioner has, for example, had the misfortune to lose in one week a woman from puerperal convulsions, and another in the following week from placental hemorrhage. Singularly enough, not one of the sequences mentioned occurred in the practice of a physician connected with a lying-in hospital. In face of the charge that the physicians holding obstetrical appointments in public institutions are active disseminators of puerperal fever through populous communities, I find that the total loss from all puerperal causes occurring in the private practice of ten physicians intimately associated with such institutions, numbered, during the nine years, but twenty-one cases. Of these, thirteen were the result of ordinary accidents, and only eight cases of metria proper, of which one was developed before the physician was called in attendance; whereas a single physician, holding no hospital appointment, lost during the same time twenty-seven cases, of which twenty-one were cases of metria.

I have been interested in endeavoring to ascertain how far experience corresponds with Semelweiss's theory that puerperal fever owes its origin to poisonous materials obtained from dissecting-rooms, and introduced into

[1] Report before the Académie de Médecine, Sept. 17, 1872.

the genital canal by the hands of physicians attending cases of labor. With this view I have made personal application to a number of gentlemen who have engaged in midwifery practice while performing the functions of demonstrators of anatomy in our medical schools. Dr. H. B. Sands, of the College of Physicians and Surgeons, reports that in the five years during which he held the office of demonstrator, he attended about sixty cases of labor. All did well. He lost his first patient, from child-bed, a short time after he had resigned his position in the dissecting-room. Dr. J. W. Wright, the present Professor of Obstetrics in the Medical Department of the New York University, who held for one year the position of Demonstrator in the Woman's College, writes me that "during the year, I attended one hundred and four cases, including twenty-two forceps cases, two of craniotomy, two of podalic version, and four of breech presentation. Of this number I lost two cases, one from phlegmasia dolens complicating uræmia, from both of which troubles the patient had suffered during her previous labor, and one from double pneumonia, the result of unusual exposure following confinement. Out of these one hundred and four cases, I can recall but three or four cases of metritis, and those of a mild character; I have never thought they had any special connection with my duties in the dissecting-room. I may add that for ten years I have attended a pretty large number of confinements each year, and that during the whole of this time I have been in the habit of making autopsies as occasion has offered, and of handling and examining pathological specimens both in and out of the dissecting-room, notwithstanding which, my death record among this class of cases has been unusually low." Dr. Samuel B. Ward, formerly Demonstrator at the Woman's College, at present Professor of Surgery in the Medical School at Albany, writes: "While I was daily in the dissecting-room, during the winter sessions of the school from 1868 to 1872, I attended thirty-two confinements of which I have notes. All of the patients recovered, nor did any of them suffer from any complication that could be traced to infection." It is familiarly known that after Semelweiss had introduced the practice, among the physicians attending patients at the large lying-in hospital in Vienna, of washing the hands in a solution of chloride of lime, there was a great diminution in the mortality which prevailed, notwithstanding which, G. Braun reports, however, that in 1857, in the month of July, in two hundred and forty-five deliveries there were seventeen deaths. The following month, Prof. Klein gave orders to suspend the use of disinfectants. By chance, in August there were only six deaths out of two hundred and fifty confinements, and in September, of two hundred and seventy-five patients, none died. From 1857 to 1860, the mortality was slight, though disinfectants were not used, while during the three following years, in spite of the systematic and persistent employment of these agents, the death-rate once more assumed formidable proportions.[1]

Of course I do not wish to underrate the importance of Semelweiss's labors. There is no question but that it is a perilous experiment to pass from the dissecting-room to a patient in labor, without employing rigorous measures to disinfect the hands and all parts of the person brought into contact with the dead body. But it is well to call attention to the fact that puerperal fever is not due to any single, simple cause, nor is to

[1] Rückblicke auf die Gesundheits Verhältnisse unter den Wöchnerinnen, u. s. w., S. 32, 33.

be effectually guarded against by a single precaution; and again that cadaveric poison does not of necessity exist in every cadaver examined. Haussmann found that injections into the vagina of gravid rabbits, in the latter half of pregnancy, of serum from the corpse of a person not dying of septicæmia, produced no fatal results, while rapid death resulted from injections, under the same conditions, of pus from the abdomen of a woman dying from puerperal infectious disease.[1]

TABLE II.—*Deaths occurring in New York, between the years* 1867 *and* 1875, *inclusive, from metria, excluding other causes, which proved fatal during the puerperal period.*

Year.	Jan.	Feb.	Mar.	April	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Total deaths from metria.	Total from all causes.
1867 . . .	16	10	14	16	12	5	6	8	5	5	9	14	120	238
1868 . . .	17	14	27	16	18	9	12	8	7	3	8	12	151	267
1869 . . .	25	26	18	10	15	14	12	15	8	12	16	19	190	312
1870 . . .	12	17	37	27	20	10	12	16	10	12	11	17	201	333
1871 . . .	21	30	22	25	26	17	11	13	7	7	12	23	214	399
1872 . . .	26	37	41	40	27	21	14	24	13	14	18	32	307	503
1873 . . .	33	40	32	39	29	22	16	15	13	11	23	12	285	431
1874 . . .	19	32	24	33	34	21	18	15	10	7	7	25	245	439
1875 . . .	28	28	40	30	22	17	10	10	5	11	14	19	234	420
Total . .	197	244	255	236	203	136	111	124	78	72	118	173	1947	3342

Season of Year.—I have in a special table (Table II.) classified all the deaths from metria in New York, during nine years, according to the months in which they occurred, and from this it appears that more than twice as many deaths took place between the six months from December to May inclusive, as between the six months from June to November inclusive. The greatest mortality occurred in February and March, amounting to 499 cases, or rather more than one-fourth of the entire number. The smallest death-rate occurred in September and October, in which months but 150 deaths, or one-thirteenth of the entire number, took place.

[1] Untersuchungen und versuche über die Entstehung der übertragbaren Krankheiten des Wochenbettes; Beiträge zur Geburtshülfe und Gynäkologie, Bd. iii, Heft 3, S. 374, u. ff.

TABLE III.—*Deaths from puerperal causes in the different city wards of New York.*

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.
1867 . . .	4	0	0	10	6	3	7	17	4	15	2	13	2
1868 . . .	4	1	1	12	3	4	9	11	9	8	8	28	7
1869 . . .	7	0	0	5	2	5	20	24	8	19	13	20	16
1870 . . .	4	0	1	5	6	5	6	20	4	19	13	21	16
1871 . . .	6	2	4	4	8	3	17	17	11	21	21	27	10
1872 . . .	6	1	4	5	4	11	13	19	10	26	14	47	11
1873 . . .	8	0	2	5	4	12	12	19	8	20	14	40	14
1874 . . .	7	0	1	7	7	14	14	26	8	23	11	24	13
1875 . . .	9	2	2	3	4	7	17	14	13	17	20	25	18
Total . . .	55	6	15	56	44	64	115	167	75	168	116	245[1]	107
Estimated number of births[4] . . .	4284	387	1089	7053	5049	6237	12,855	10,395	14,137	12,325	18,975	14,107	9898
Ratio of deaths to births . . .	1 : 78	1:64.5	1 : 72	1 : 126	1 : 115	1:97.5	1 : 112	1 : 62	1 : 188	1 : 73	1:163.5	1 : 57	1 : 92

	XIV.	XV.	XVI.	XVII.	XVIII.	XIX.	XX.	XXI.	XXII.	XXIII.	XXIV.	Total.
1867 . . .	9	9	8	38	15	22	18	24	12	....	....	238
1868 . . .	7	5	6	32	18	17	21	35	21	....	....	267
1869 . . .	10	4	9	33	21	37	22	27	10	....	....	312
1870 . . .	2	5	9	41	28	34	19	53	22	....	....	333
1871 . . .	8	6	18	41	21	38	37	54	25	....	....	399
1872 . . .	14	11	23	45	24	85	45	43	42	....	....	503
1873 . . .	5	7	14	40	24	51	40	54	38	....	....	431
1874 . . .	7	6	20	36	8	56	39	66	34	12	....	439
1875 . . .	14	12	18	51	27	53	19	30	39	5	1	420
Total . . .	76	65	125	357	186	393[2]	260	386[3]	243	17	1	3342
Estimated number of births[4] . . .	7850	8197	14,363	28,323	17,701	25,542	22,394	16,840	21,191			
Ratio of deaths to births . . .	1 : 103	1 : 126	1 : 113	1 : 79	1 : 95	1 : 65	1 : 86	1:45.6	1 : 87			

[1] The twelfth ward includes Ward's Island Hospital and the Colored Home, which furnished together 102 deaths; deducting these the ratio would be 1 : 99.
[2] The nineteenth ward contains the Nursery and Child's Hospital and the Charity Hospital, which furnished together 83 deaths; deducting these the ratio would become 1 : 82.
[3] Includes 189 cases from Bellevue Hospital, deducting which the ratio would become 1 : 85.
[4] The number of births in each ward is roughly estimated by assuming the population of 1870 to be the average of the nine years, and then allowing 33 births to the thousand. This method makes no pretence to accuracy, but it is not likely that exact returns would materially alter the relative proportions given. It furnishes a total of 279,000, or 5000 less than that obtained by assuming an annual increase of 15,000 to the entire population of the city.

Influence of Locality.—I have likewise prepared a table showing the number of deaths from puerperal causes occurring in each year in the several wards into which the city is divided, and the ratio of deaths to

the estimated number of births in each ward. The average ratio is, as has been already stated, about 1 to 85. In the 9th Ward, however, it was 1 to 188; in the 11th Ward, 1 to 163.5; and in the 15th Ward, 1 to 126. These wards make the most favorable showing in the city. By reference to the map, they are all found to occupy the same territorial zone. In the 17th Ward, situated between the 11th and 15th Wards, the ratio of deaths to births, it is true, was 1 to 79; but the 17th Ward is the most densely populated section of the city. As neither the 9th nor the 11th Wards are otherwise especially favored, both containing a large and crowded tenement-house population, it is hard to understand their relative immunity from deaths due to puerperal diseases, unless upon the assumption that local causes, probably superior drainage, render them exceptionally free from endemic sources of disease.

TABLE IV.—*Deaths from puerperal causes in the* 18*th*, 19*th*, *and* 21*st wards, with the relative proportion of deaths, not occurring in Hospitals, among the population east and west of Third Avenue.*

Year.	Total.	Died in hospital.	Deaths upon the west side of Third Avenue.	Deaths upon the east side of Third Avenue.	Relative proportion of deaths on west and east side, expressed in percentages.
1867 . . .	61	20	8	33	24 per cent.
1868 . . .	70	28	8	34	23.5 "
1869 . . .	85	23	12	50	24 "
1870 . . .	115	34	9	72	12.5 "
1871 . . .	113	32	15	66	22.7 "
1872 . . .	173	43	10	120	8.3 "
1873 . . .	129	37	15	77	19.5 "
1874 . . .	130	45	14	71	19.7 "
1875 . . .	110	15	8	87	9.2 "
Aggregate . .	986	277	99	610	16.2 "

Social State.—I have, too, sought to ascertain whether the wealthy and well-to-do classes enjoy special immunities from puerperal disease. The district bounded by Fourth and Sixth Avenues, and Twelfth and Fifty-ninth Streets, contains the great proportion of those who make up the privileged classes. In this portion of the city, the proportion of deaths from puerperal causes to the deaths from the same sources throughout the city, is as 1 to 43. The precise population of the district I cannot obtain, but it is safe to say that it contains at least one-thirtieth of the entire population of the city.

The eighteenth, nineteenth, and twenty-first wards, included between Sixth Avenue and the East River, and extending from Fourteenth to Eighty-sixth Streets, contain in a marked degree the two extremes of wealth and poverty. Third Avenue separates the territory into two equal parts, and forms a fair dividing line between the upper and the lower social strata. The population upon the west side of Third Avenue is, it is true, less than that upon the east side,[1] possibly not more than half as great, but the mortality of nine years from puerperal causes was in this favored region less than one-sixth, even after deducting

[1] I am informed by politicians that the vote in the ward mentioned is about 50 per cent. larger on the east side of Third Avenue.

deaths occurring in hospitals, which for the most part lie upon the east side. In 1872, the fatal year, the proportion was one to twelve.

TABLE V.—*Deaths from scarlatina, diphtheria, and erysipelas, and from all causes, by quarters, compared with deaths from metria, for the nine years ending December* 31, 1875.

Causes of death.	1867.					1868.					1869.				
	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.
Scarlatina	264	194	89	103	650	318	351	102	90	861	299	285	159	223	966
Diphtheria	79	56	53	63	251	92	87	43	55	277	87	89	66	86	328
Erysipelas	40	32	20	22	114	31	45	13	13	102	54	55	14	29	152
All causes	5197	5121	7516	5325	23,159	5953	5523	8658	4755	24,889	5919	6065	7390	5793	25,167
Metria	....	....	....	....	120	....	....	....	....	151	....	....	....	....	190

Causes of death.	1870.					1871.					1872.				
	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.
Scarlatina	418	258	106	193	975	274	196	137	184	791	301	367	132	190	990
Diphtheria	94	69	67	78	308	89	49	46	54	238	93	86	87	180	446
Erysipelas	50	42	4	25	121	54	50	18	23	145	64	73	23	25	185
All causes	6517	6293	8416	5949	27,175	6622	6621	7833	5900	26,976	7406	8737	10,025	6479	32,647
Metria	....	....	....	....	201	....	....	....	....	214	....	....	....	....	307

Causes of death.	1873.					1874.					1875.				
	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.	1st quarter.	2 "	3 "	4 "	Total.
Scarlatina	242	275	202	326	1045	322	239	166	152	879	186	161	63	104	514
Diphtheria	169	184	316	482	1151	334	314	320	697	1665	608	549	489	683	2329
Erysipelas	83	60	30	31	204	49	62	25	33	169	66	51	21	29	167
All causes	6815	6583	8983	6703	29,084	6328	6548	8509	7342	28,727	7840	7065	9257	6547	30,709
Metria	....	....	....	....	285	....	....	....	....	245	....	....	....	....	234

Relations to Zymotic Diseases.—I prepared, some time ago, tables to answer the inquiry as to whether there was any relation between the frequency of cases of scarlatina, diphtheria, and erysipelas, and those from metria proper. Previous to their publication, however, I was anticipated in my deductions by a paper upon the same subject by Dr. Matthews Duncan.[1] I therefore simply submit the tables with the comment that

[1] On the alleged Occasional Epidemic Prevalence of Puerperal Pyæmia, or Puerperal Fever, and Erysipelas; Edinburgh Medical Journal, March, 1876, p. 774.

they show no relation to exist between the statistical frequency of puerperal fever and the zymotic diseases mentioned. The tables do not, however, invalidate any direct testimony which goes to show that, in individual instances, a real connection between puerperal fever and the zymotic diseases may exist.

Prevention of Puerperal Fever.

Of the 3342 deaths from puerperal causes, 420 died in hospital, or one-eighth of the entire number. Of the 1947 cases of metria, about 300, or not quite one-sixth, were contributed by the hospitals. Upon such a showing the first impulse would be to cry out loudly for the suppression of maternities. But a wiser policy suggests the inquiry whether the large mortality in hospitals is an evil necessity. To this the answer is "No." It is possible, in the present state of our scientific knowledge, to so control the conditions that favor the generation of puerperal diseases in large hospitals, as to make them safe asylums for the needy.

Thus in the Charity Hospital, Dr. Kitching reports the number of confinements between July 1, 1874, and May 1, 1876, at 1149; of which 20 ended in death, or 1 in 57. But only six were cases of metria, or 1 in 191 deliveries, a better record than that afforded by the average of the city at large. In the Nursery and Child's Hospital, there was but one death from metria in 1874, and one in 1875. In 1872, the number of confinements was 205. I regret that a request for the number of confinements in the succeeding years has been refused me. The Marion Street Lying-in Asylum, an excellent institution for married women, reports two deaths from metria in nine years. Dr. Goodell reports that, at the Preston Retreat, in 756 cases of labor there have been but two deaths from septic disease; Winckel,[1] of the Lying-in Institution in Dresden, reported, in 1873, eighteen deaths from metria, or 1.8 per cent., but from the 10th of January to the 7th of July, in 570 deliveries there was but one case of septic disease; in the year 1872 the death-rate exceeded 5 per cent. The reduction in the mortality was no fortuitous circumstance, but was due to rigid measures for the prevention of disease.[2] Stadfeldt reduced the mortality from puerperal fever, in the Maternity Hospital of Copenhagen, from 1 in 37, the proportion between the years 1865 and 1869, to 1 in 87, between the years 1870 and 1874.[3] Dr. Johnston reports in the Rotunda Hospital of Dublin, during seven years, 7860 deliveries (I have excluded the cases of abortion) with 169 deaths, of which 85, or 1 in 91, were from metria.[4]

Prof. J. J. Bischoff reports that in the Maternity of Basel, between the years 1862 and 1867 inclusive, in 514 confinements there were 33 deaths, or in about the proportion of one to sixteen. In 1868, of 75 patients confined, there died three, or one in twenty-five. In 1869, of 86 patients confined, seven died, or one in twelve. In 1870 effective measures of prevention were introduced, and, that year, in 80 confinements there were no deaths.[5] In 1871, two out of 124 patients died, or one in sixty-two. In 1872, in 153 confinements there were four deaths, a proportion of one

[1] On the Means employed at the Preston Retreat for the Prevention and Treatment of Puerperal Diseases, p. 13.
[2] Berichte und Studien, u. s. w., Leipzig, 1874, S. 183.
[3] Les Maternités, leur Organisation et Administration; Copenhague, 1876.
[4] Series of Clinical Reports, from 1870 to 1876 inclusive.
[5] J. J. Bischoff, Zur Prophylaxis des puerperal Fiebers; Basel, 1876, S. 12, 13.

to nearly forty. The next year, 1873, strict measures were again put in force, and in 184 labors there were two deaths, or one in ninety-two. In 1874, in 214 labors there were three deaths, one to seventy-one, but one of the patients entered the hospital with endocarditis, of which affection she died. In 1875, in 204 confinements there were two deaths, one from hemorrhage, and one from pyelitis which existed at the time the patient entered the hospital.

Surely these figures do not support the idea that it is better for a woman to be confined in a street gutter than to enter the portals of a lying-in asylum. Dr. Goodell's experience shows that a hospital for respectable married women may be so conducted that its inmates may enjoy absolutely a greater degree of safety than do women in their homes, surrounded by all the aids that wealth can command. Equally good results are not to be obtained in hospitals which are open to unfortunates of every class. But there is much misapprehension and confusion of ideas respecting the fate of these women, when no charitable provision is made for them. In Copenhagen, the Maternity Hospital is closed for from six to eight weeks in the summer time. During this period, unmarried, parturient women receive pecuniary assistance from the hospital to enable them to obtain a place in which to be confined. Now Stadfeldt reports a larger mortality among this class than among those delivered in the hospital. Yet they are confined at a favorable season of the year, without any communication with the furniture, the *sage-femmes*, or the physicians of the hospital. As they fortunately receive nothing but money, that can hardly be suspected of communicating contagion. What their fate would be in New York City, perhaps, may be judged from the following facts: Excluding cases confined in hospitals, nearly one-thirtieth of all the deaths and one-twenty-fourth of the cases of metria are reported by four practitioners. Ten practitioners out of 1200 signed the death certificates of one-fifteenth of the women dying from puerperal causes, and one-tenth of the cases of metria. But it is not to be supposed that these deaths were all the result of malpractice and incompetence. The true history of most of them probably was that the doctor was engaged to attend the case of confinement for a small fee, with the understanding that he should make no calls subsequently, unless specially summoned by the friends of the patient. The latter, left to ignorant care, or perhaps without any assistance whatever, and exposed to all the pernicious influences bred by poverty, when illness supervened probably did not call the physician to her aid until the time for help had passed, so that in the end his professional functions were confined to procuring the requisite permit for burial.

Humanity demands that charity should furnish places of refuge in which poor outcasts can receive assistance during the perils of childbearing. If we must then have maternities, we should make them safe, and this can be in great measure accomplished by remembering the twofold source of danger arising from a poisoned atmosphere and direct inoculation. A hospital must be clean, spacious, and well ventilated, or its atmosphere will become charged with decomposing albuminoid substances, and produce nosocomial malaria. But the experience of the Hôpital Cochin,[1] a costly, palace-like structure, with every appliance of art, proves that fresh air alone does not protect patients from the consequences of inoculation. On the other hand, the most rigid sanitary pre-

[1] Billet, Réforme des Maternités, p. 75.

cautions observed by the attendants will not prevent a badly ventilated ward from becoming unhealthy, unless unoccupied wards are kept to which patients can be transferred upon the first admonition of danger. At the Charity Hospital, the service consists of two large wards, occupied for three weeks at a time alternately. Dr. Goodell states that at the Preston Retreat the wards are used invariably in rotation. In connection with the Maternity at Copenhagen, there are a number of small supplementary hospitals scattered through the city, which serve as safety-valves for the central institution. Artificial methods of ventilation render the task of keeping the wards healthy, comparatively easy. They do not need, however, to be complicated and expensive. The good repute of the Rotunda Hospital, it seems to me, is in large measure due to the natural ventilation afforded by open fireplaces.

The testimony is very general to the advantages of frequently washing the wards with carbolic acid. At the Charity Hospital, in addition, the wards are fumigated three times a week by burning a mixture of carbolic acid, chloride of lime, and sulphur. During parturition, Stadfeldt exposes the woman to a fine spray of carbolic acid. If, after confinement, vaginal washes of carbolic acid should be used, every woman should be supplied with her own nozzle, which can be made of glass tubing, and attached in turn to the irrigator. When not in use, these should be kept in solutions of carbolic acid.

A liberal supply of metallic catheters should be always kept on hand and, when not in use, placed in a disinfectant fluid. Sponges, in a hospital, are an abomination. Oakum, lint, or old cloths should be employed to receive the discharges, and, when removed, should be placed at once in a vessel containing disinfectants, and then, instead of going into the washtub, should be burned. As regards all prophylactic measures, cheapness is not to be consulted.

As puerperal patients require much attention, lying-in wards should possess a sufficiency of attendants, best, perhaps, by making them training schools for nurses. Nurses should be intelligent enough not only to comprehend their duties, but their responsibility. Physicians should wash their hands with disinfectants before and after making examinations, and nurses should do the same whenever their hands are brought into contact with vaginal secretions. D'Espine has shown that the lochia of the third day of a healthy patient will poison a rabbit.[1] A nurse employed in the puerperal wards ought not to have access to cases of labor. A patient attacked with fever should be immediately removed to another part of the building, and the nurse in attendance should go with her. Special wards for the recently confined should never be established. As enforcement of details is essential to success, a lying-in hospital should have a resident medical head with plenary power to enforce the most rigorous discipline. The adoption of Lister's principles in maternity hospitals has, when carried out with the requisite discipline, served to dissipate the well-founded objections to their existence. It makes no difference whether they are old hospitals or new, cottages or pavilions. All that is required is space, light, air, intelligent nursing, an active head, and generosity in needed supplies, to keep them as free from infectious diseases as are the homes of the better classes. I have already shown, in another paper, how, through limited room, ignorant nurses,

[1] Contributions à l'Étude de la Septicémie Puerpérale, p. 18.

lax discipline, and niggardly management, an epidemic, costly in life, could be generated.

So far from tearing down the maternity hospitals now in existence, new maternities of small size, for married women, modelled after the Preston Retreat, are urgently called for to meet the present waste of life among the very poor. I cannot but hope that such institutions, intelligently constructed, ordered, and maintained, may yet be the outcome of private charity. A small maternity has the advantage over a large one that, when a dangerous element is introduced, repressive measures can be more rapidly and effectively carried into execution. I have already taken up so much time that I will not dwell upon the duties of the physician toward his private patients: how he should avoid conveying to them zymotic diseases, or septic poisons; how he should understand that the proper management of labor involves a normal puerperal state; how he should guard lest the antecedents of his patients should unfavorably affect the period of parturition and child-bed. I have only to add that, among the wealthy and refined, there is an annual sacrifice of lovely women, due, not to want of skill or care, not to infection, but to the fact that the enervating influences of civilization and the pressure of social life, have apparently unfitted them for offering effective resistance to any form of traumatism. Dr. E. H. Clarke's little work entitled "Sex in Education," strikes the key note of a subject requiring on the part of obstetricians the profoundest consideration.

In closing this paper, I beg to offer the following conclusions:—

I. There is no specific, puerperal fever, and, as long as, when the term is limited, each physician uses it in some sense personal to himself, it is best to employ it as a general term for all the febrile affections peculiar to the puerperal state.

II. Puerperal fever, the term being used as above, may be either a non-infectious or an infectious disease.

III. The non-infectious form is due to traumatic inflammations of a simple character, to old peritoneal adhesions, to moral causes, and to the vulnerability of the patient.

IV. The infectious form, on the other hand, is a septic disease, intimately associated with the existence in the tissues of minute organisms which form the connecting link between puerperal fever and erysipelas and diphtheria.

V. Its causes are certain atmospheric conditions (nosocomial malaria of hospitals), and infection from septic materials. It prevails most in certain winter months, and finds its victims chiefly among the very poor.

VI. Prevention is best accomplished in hospitals by the adoption of Lister's principles.[1] Maternity hospitals are no longer necessarily foci

[1] The following regulations, enforced by Bischoff, in Basel, will serve as an example:—Removal from the genitalia, after confinement, of *débris* capable of undergoing putrefaction. Protection of the wounded surfaces from all air not impregnated with carbolic acid. Hands of attendants, and all instruments used in labor, to be disinfected with carbolic acid. A full bath to be given upon the advent of labor, and the vagina to be washed out with a two per cent. solution of carbolic acid. In cases of premature rupture of membranes, of protracted labor, artificially induced premature labor, and of dead fœtus, a repetition of the injection every two hours. Hands and instruments to be lubricated when necessary with a ten per cent. preparation of carbolized glycerine. In cases of version, of artificial removal of the placenta, of putrid fœtus, of post-partum hemorrhage with relaxation of the uterus, employment of intra-uterine injections of two to three per cent. solutions of carbolic acid. After the end of labor, the external genitalia to be inspected, and a ten per cent.

of disease. Small maternities, if well managed, meet a need among the very poor, and their establishment by the charitable should be encouraged. In private practice the physician should never, and need never, be the carrier of contagion. The antecedents of nurses should be investigated. Auto-infection should be very rare in cases properly managed during the period of labor.

VII. The question of personal responsibility cannot be too strongly impressed upon the medical profession.

Discussion on Dr. Lusk's Paper.

After the reading of the preceding paper, Dr. H. P. Yeomans, of Mount Forest, Ontario, Canada, said:—With regard to Dr. Lusk's statement that bacteria in puerperal fever cause endocarditis, I would say that the presence of lactic acid in the blood may produce precisely the same cardiac conditions, and that when injected into the veins of animals, it has developed endocarditis, and caused deposits of fibrin on the valves. Now it is a significant fact that, in cases of puerperal fever, lactic acid is one of the morbid constituents of the blood. In such cases it is not necessary, therefore, to invoke the presence of bacteria as the cause of the endocarditis.

Dr. William Goodell, of Philadelphia, said:—I have listened with much pleasure to the admirable paper just read. There are points in it on which I should like to dwell; but, as the time for discussion is very limited, I shall confine my remarks to the prevention of puerperal disorders. I contend that no statistics are trustworthy, excepting those of hospitals and of other public institutions, for physicians very naturally shrink from reporting their fatal cases of puerperal fever as such. In evidence of this, I may adduce the fact that at one time I was cognizant of fourteen deaths in two weeks from puerperal fever. Yet the weekly mortuary returns of the Board of Health gave for that time but twelve deaths *in the whole city* from that disease. With regard to the prevention of fever in the inmates of the lying-in institution of which I am in charge, I am governed by four golden rules: *Cleanliness*, *Ventilation*, *Rotation*, and *Isolation*. And I am the more satisfied with them since, among 1021 hospital patients who have passed under my care, I have not had a single case of phlegmasia dolens, and but one of pelvic abscess, while I have lost only three from fever. Each one of these rules is a generic term embracing many specifications, but since the first three explain themselves, I shall dwell on the last only. By isolation I mean the imitation of the home-environment—in other words, the putting a hospital patient in a condition as like as possible to that which she would have at home. While this ideal cannot be wholly reached in a hospital, yet it should be approached as closely as possible. If the same sponge, the same chamber-pot, or the same syringe, be used by several patients, this law of home-environment is violated; for every woman delivered at home has her own sponge, her own syringe, and her own vessel. Hence in place of sponges I use finely picked oakum, with which every woman—and not the nurse—washes her own person. Hence every bed is provided with its own vessel, and hence every woman's person is sweetened,

carbolized oil to be applied to all lesions. Rupture of the perineum to be closed with carbolized silk or catgut sutures. The parts to be washed clean, and picked lint soaked in carbolized oil (ten per cent.) to be laid upon the vulva. Vaginal injections of a two per cent. solution of carbolic acid to be used twice a day—or, in cases of difficult labor, putrid fœtus, or retained portions of ovum, to be repeated every two hours. In cases of retained portions of placenta, hindering the contraction of the uterus, intra-uterine injections of a two or three per cent. solution of carbolic acid in water twice a day. The nurse to wash at each visit the genitalia and inner surface of the thighs, with a lukewarm solution of carbolic acid. I have already given the results of this treatment.

not by detergent vaginal or intra-uterine injections, but by the upright position, by making her get out of bed and slip into a chair several times a day. Since the same ward holds, and the same nurse attends, a number of women at the same time, this is a flagrant violation of the law of home-environment, but one which cannot be avoided in hospital practice. As a make-weight, however, I put but few women in the same ward, and make the nurse use a nail-brush, take frequent baths, and wear such clothing as can be washed. As soon as the women are out of bed for good, the nurse is relieved from duty; she takes a carbolized soap-bath, and goes into a clean ward, which has lain fallow for some two weeks or more, and usually gets a week of rest before a new batch of patients falls to her care. I myself always wash my hands with carbolized soap, before making an examination, and, to make assurance doubly sure, I keep an obstetric and a gynæcological hand. With my left hand I make all my office and dispensary vaginal examinations; my right hand I reserve as much as possible for obstetric work. Since moist heat forms the condition under which poison-germs best germinate, after a ward is once occupied, the floor should not be washed. An accidental stain may be wiped up, but nothing more. I have been led to this practice from observing that the cleanest ward-floor, when drying, emits a bad smell, and from finding, some hours after such a floor-washing, that the body temperature of my patients often shows a sharp curve-peak. From the cloud of epidermic scales, and of other human and consequently septic *débris*, raised by the broom, I am also opposed to sweeping a ward, and especially while any patient is in it who has not yet got her milk—that is to say, whose labor-lesions have not yet become protected by a covering of granulations. But, since such a mode of enforcing cleanliness, although a wide departure from our ideal of the home-environment, cannot be well dispensed with, I would suggest the plan devised by my friend, Dr. Addinell Hewson, viz., to strew the sawdust of some terebinthinate wood, such as cedar or pitch-pine, over the floor of the ward before it is swept. The sawdust of any ordinary wood would do, if sprinkled with turpentine, which, while being antiseptic, would give the needful amount of dampness without moistening the floors. Since fatty degeneration of the heart as well as of the other viscera is one of the most common lesions of child-bed fever, to avert this tendency large doses should be given of such antiseptics and antipyretics as quinia and alcohol, combined with opium enough to allay all pain. And such a heart-tonic as digitalis should be given instead of a pure depressant like veratrum viride.

Dr. W. H. Byford, of Chicago, said:—I believe with Dr. Lusk that puerperal fever is produced by many different causes, and assumes many different forms. Sometimes it originates under isolated conditions, and is autogenetic, resulting then from various intrinsic causes, such as decomposition of blood clots, or of retained membranes, etc. In others it has a zymotic origin from various extrinsic causes, which prove at times morbific, owing to the peculiar state of the patient. Frequently cases of puerperal fever result from overcrowding in hospitals; at least they are always more prevalent in close buildings where there is no good ventilation. The disease ought not to be called epidemic; it is rather endemic. But there is another class of cases which differs from these, especially in miasmatic sections. Some years since I witnessed an epidemic of "black tongue" which occurred in Indiana. During the height of the disease, which lasted two months, every case perished and every lying-in woman in that neighborhood died of puerperal fever; but as the epidemic subsided, the virulence of the fever grew less, and some cases began to recover. When, finally, the "black tongue" disappeared, puerperal fever vanished with it, and the younger doctors then thought that they had discovered the cause of the latter disease. I believe puerperal fever to be contagious under certain circumstances, but think that it cannot be carried by the physician after he has washed himself with carbolic acid.

Dr. John L. Atlee, of Lancaster, said:—I had attended a thousand cases

of labor without seeing puerperal fever, and from this good luck had argued that there was no necessity for more than ordinary cleanliness on the part of the physician. But once I had a case of phlegmonous erysipelas, which I dressed, and then, during the succeeding afternoon and night, attended three cases of easy delivery. The next morning I left them in charge of a friend, and went to Baltimore as a delegate to the American Medical Association. On my return I found my friend waiting for me at the station, to take me to see these women, all of whom had puerperal fever. I found one already dead, and the other two hopelessly ill. Under the teachings of Hodge and Meigs, it would then have been considered heresy to say that puerperal fever was communicable, and yet I carried the disease, having used no special disinfectant, and I believe that I was the innocent cause of the death of those three ladies. I therefore urge that the profession should use extraordinary measures against personal infection and the carrying of this disease.

Dr. J. P. WHITE, of Buffalo, said:—Puerperal fever was said years ago not to be communicable, but most of us have changed our minds upon this point. We may call it infectious or contagious as we will, but it has spread about neighborhoods. I know of cases in which it has been carried, and I know that it is communicable. The opposing opinions of Dr. Hodge, Dr. Meigs, and others, have been overturned, and there is now no doubt that puerperal fever is eminently contagious. No man has a right to go directly from a case of puerperal fever, and attend another patient in labor. I know of no disinfectant that will make it safe to attend a case of labor after one of puerperal fever. In the treatment of this disease, I place much reliance upon opium, for which there is great tolerance in puerperal fever. I have given as much as one grain of morphia every hour for forty-eight consecutive hours, and have been rewarded for my boldness by the recovery of my patient.

Dr. E. H. TRENHOLME, of Montreal, said:—I have given from a grain to a grain and a third of morphia, every hour, with positive benefit; and I have never lost a patient from puerperal fever, although I have met with five cases.

Dr. J. C. HUBBARD, of Ashtabula, Ohio, said:—I should like to test the sense of the Section as to whether, in cases of puerperal fever, the child should be allowed to remain in bed with the mother or not. Most physicians allow the child to remain as long as it can get a drop of milk.

Dr. GEORGE W. MEARS, of Indianapolis, said:—I consider puerperal fever more infectious than contagious, in the strict sense of those terms. I knew a physician once to lose seven cases of labor from this cause within two weeks (every case of the kind which he attended), notwithstanding that he had used all necessary precautions, such as the free application of disinfectants, change of clothes, etc. They were obviously all instances of personal transmission, no other physician in the city having had a case of the kind to treat. A few weeks' absence from home served to free him from the infecting influence, no other instance of the disease having subsequently occurred in his practice. I believe it also possible that the fever arising from other causes than that of erysipelas may be conveyed, but not as readily, by the attendant on labor. According to my observation, when the infection arises from erysipelas the child develops erysipelas and the mother peritonitis.

Dr. ALEXANDER R. SIMPSON, of Edinburgh, said:—The chapters in our text-books, instead of being headed "Puerperal Fever," as they are, should be entitled, "On the Fevers of the Puerperal State," not specially referring to puerperal fever as an entity. The puerperal woman is liable to ordinary diseases, but in her they assume peculiar features, and are so marked as to give rise to the idea of a special disease, called puerperal fever. I believe that often these fevers are merely typhoid fevers in the puerperal woman. In some cases the development is due to the poison of scarlatina; but both typhoid and scarlet fever are diseases which are as natural to the puerperal woman as to the non-puerperal. I do not know whether such tables have been drawn up, but I should like to see statistics of the results of wounds re-

ceived in battle, as to the difference in the ratio of recovery of those wounded early in the day, when they are fresh, and those wounded later. Such data would apply to cases of puerperal women; whether the women are fresh from a short labor, or exhausted by a long and tedious one. With our present knowledge of the subject, it matters little, as far as treatment is concerned, whether the poison be chemical or animal. To meet the difficulty of personal infection, we should be careful not to carry the poison about in our daily rounds. Most of the germ destroyers are acids, as carbolic, sulphurous, and salicylic acids, but, as a disinfectant, I would call attention to one which can be found in every household, and that is common vinegar; I have, on many occasions, found it extremely efficacious. After extraordinary exposure, the clothing should be fumigated with sulphurous acid.

Dr. Lusk, in reply, said:—I beg to thank the Section for the attention which has been given to my paper, and for the discussion which has followed. In reply to Dr. Yeomans, I would say that it is true that the presence of lactic acid in the blood causes endocarditis, but that I confined my remarks to a single form of endocarditis, of which the great characteristic is the presence of masses of bacteria upon the cardiac valves. But the great question is, whether the poison can be destroyed after its inoculation has taken place. I think that it can. I think that the mother ought to be separated, in these cases, from the child, as it is better for both. I use Labarraque's solution to disinfect my hands, and have never carried the disease from hospital to private patients.

ON ELECTROLYSIS, WITH SPECIAL REFERENCE TO THE TREATMENT OF OVARIAN CYSTS.

BY

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Electricity, the essential nature of which we are unable to explain, reveals itself to us under all the different known forms of motion, both as mechanical power and as molecular movement, whether as light, heat, or chemical action. The phenomena of these are essentially identical; they are correlative, and are constantly being transformed one into another. We can command, to a certain extent, by choosing among the various sources of electricity, and by different arrangements of apparatus, the particular manifestation which we wish to produce; but no form of electricity can be generated which does not display, more or less, all its various qualities. Especially is it impossible to conceive of any electric current acting on a living body without causing chemical action, and perhaps the only way in which electricity can act in the living tissues is through its chemical effects.

Electrolysis is the name given to that application of electricity in which chemical action is principally sought for, and, especially in medicine, when, by the use of either the galvanic or the faradaic current, an attempt is made to dissolve or destroy organic productions, infiltrations, exudations, or distinct tumors. Electrolysis is extensively used for industrial purposes (electrotyping, etectroplating, etc.), but its applications to medicine and surgery are as yet very limited; this branch of electro-therapeutics is still in its infancy, its philosophy is very imperfectly understood, the number of cases in which it has been employed is small, and the data for judging of its value are widely scattered; the time for writing a treatise on medical electro-chemistry has not yet arrived.

The effects of electrical currents on simple, inorganic solutions are easily understood; but it is different when we have to deal with organic and highly complex substances, such as are met with in the living body. An electric current passing through common water decomposes it, setting free oxygen at the positive and hydrogen at the negative pole; with a solution of common salt a similar process goes on, the oxygen formed at the positive pole acting on the salt and decomposing it. In the case of liquids like those found in the living body, containing albumen and azotic combinations, we know, as a general rule, that, at the positive (oxygen) pole, albumen, fibrin, fat-acids, etc., are separated, while at the negative (hydrogen) pole there appear the alkaline and earthy bases, iron, and coloring matters.

If an electric current is passed through a watery solution of albumen, in a short time (say five minutes) a clot will be found at each pole; that at the positive pole is the larger and denser, the wire (copper) becomes green by oxidation, and the whole clot has a slight, greenish tinge; the clot at the negative pole is more cloudy, soft, and diffuse, and of a grayish tinge, while the wire becomes blackened from the development of

sulphuretted hydrogen. The same thing is observed (except that the clots are smaller) when the wires are applied, after the shell is removed, to the soft membrane which covers the white of an egg. The above remarks apply to the constant, galvanic current; although the faradaic current also decomposes water and coagulated albumen, yet the electrolytic properties of any faradaic current, which a living subject can endure, are so insignificant, that its value in this respect may be disregarded.

What has been described as taking place in an albuminous solution, is precisely analogous to that which occurs in the living body, the effects varying somewhat, however, according as both poles are introduced or applied on the surface, or as one or the other is introduced while the other is applied externally; and from a knowledge of these differences may be derived varied indications for the therapeutic use of electricity.

With these introductory remarks, I beg to call the attention of the Section to a new application of electricity in surgery; I mean the electrolytic treatment of ovarian cysts. When, a year ago, I published my first successful cases, a claim of priority was made by two gentlemen, one of whom had indeed reported cures of ovarian cysts, by electricity, as long ago as 1859. But these cases excited no attention at the time, and were so entirely forgotten that in Prof. Schrœder's Essay on Gynæcology, in Ziemssen's Cyclopædia, no earlier cases were referred to than those of Dr. Fieber, published in 1874. All that I claim is the credit of having again brought the method before the profession, and I sincerely hope that it will not require fifty years to elapse before the value of this mode of treatment is acknowledged, as was the case with ovariotomy, the great invention of Dr. McDowell, from whose deserved reputation I would certainly be the last to detract.

But science is constantly progressive, and what was yesterday considered a great triumph, may to-morrow be superseded, and just as legitimately abandoned as it was praised the day before. Hence I do not hesitate to repeat, before this assembly, my assertion, that I hope within a short time to see ovariotomy abandoned, or at least restricted to a very few exceptional cases. My grounds for entertaining this opinion I have recently published at length, and shall not therefore repeat at this time what I have already written; but I will merely say that six cases of ovarian dropsy, which I have treated in this way, have been completely cured in from four weeks' to five months' time, according to the size of the tumor; and that in a case of fibro-cystoma of the womb, under electrical treatment, the liquid contents of the cysts have been absorbed, the fibrous, solid masses remaining nearly unaltered. The ovarian cases were not selected but were taken indiscriminately; the cysts varied in size from that of a cocoanut to that of a pregnant uterus at full term. In one case the tumor was undoubtedly monocystic, with thickish contents; and in another polycystic; in two cases the cyst contents were quite thin; while one was probably a case of dermoid cyst. The ages of the patients varied from eighteen to forty-five years; one was a virgin; one married, but sterile; and four had borne children.

The constant current was employed in all the cases; in one, no needles were introduced, but both electrodes applied to the surface, and yet the contents of the tumor were absorbed. The currents employed were mild ones; no inflammation of importance occurred, and consequently no adhesions were formed; the pain was insignificant, anæsthetics never having been required, and no patient having been confined to bed. Thus far, no relapse has occurred. The method is equally applicable to

single and to multiple cysts; to those with albuminous, with saline, and with fatty contents; to those with and those without adhesions; to those which have and those which have not been previously tapped.

In one of my cases, after the liquid contents had been absorbed, hard, cartilaginous bodies in the cyst remained, apparently unaltered; the cyst walls, in all, underwent contraction, and a solid lump, of the size of a small apple, remained when all the liquid had disappeared. One obvious advantage of this method of treatment over ovariotomy, is that it does not deprive the patient of one of her most important organs. The sittings, in my cases, usually lasted only a few minutes, and were repeated daily—in one case, even during the menstrual period.

The *modus operandi* of this method of treatment is not thoroughly understood. In addition to the decomposition and absorption of the liquid, some change must take place even in those parts of the cyst walls which do not come into direct contact with the electrodes, for the further production of liquid is prevented. Dr. Althaus's theory as to the effect of electricity on tumors containing saline liquids, is that, the salt being decomposed, a caustic alkali is formed, and by setting up a mild inflammatory process increases the vitality of the tissues, and thus leads to the absorption of the fluid.

Before resorting to ovariotomy in any case, the surgeon should at least give electricity a trial; for, even if it fail, the patient will be in no worse condition for the operation, while the risks of the latter more than counter-balance the evil of a little delay.

PARACENTESIS, ASPIRATION, AND TRANSFUSION.

BY

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Fifty years ago, paracentesis was seldom employed, except in cases of fluid distension of the abdomen, bladder, or scrotum, after other means of relief had failed, and then it was a clumsy and unvalued procedure. The old trocar, having a triangular point and a big head, with a split canula behind it, went in with a jerk, endangering intestine and testicle in tapping for ascites and hydrocele; then the canula, pushed onwards over the big head expanding the segments of its split end, would often catch some bit of the three-cornered aperture, and fail to enter perfectly, or in entering would tear or split up the edges of the aperture, and allow the fluids to leak injuriously into or upon the neighboring tissues, as in the operation for hydrocele, or in puncturing the bladder; and finally, the instrument would leave a jagged, punctured wound, indisposed to heal.

But at the present time paracentesis is called upon both to detect and treat a marvellous variety of diseases, and some of them of great importance, such as hydrothorax, empyema, hydro-pericardium, ascites and purulent deposits in the abdomen, cysts of the broad ligament and of the ovary, intra-pelvic and lumbar abscesses, retention of urine with impermeable urethra, distressing tympanitis, strangulated hernia, hydatids and abscesses of the liver and kidneys, hurtful effusions within or about the hips and other joints, hydrocephalus, and spina-bifida; and the present duties of paracentesis are more critical, in consequence of the sensitive and vital character of the textures and parts in and upon which many of these diseases are situated, such as the lungs, the heart, the abdominal viscera, the pelvic organs, the great joints, the spinal cord, and the brain.

In dealing with such diseases, in such parts, none of the trocars now in use are equal to what is required of them, either for exploration or for treatment.

Paracentesis now, with or without aspiration, with or without injection, is commonly done either with a single tubular trocar having an unguarded point (which is most dangerous to the interior), or the guard is an open canula, to be pushed over the point after it has entered, and of itself only somewhat less dangerous if it gets in, while it is uncertain to enter; and we will do wrong to employ these uncertain and dangerous instruments if an efficient and safe instrument can be had.

The perfect trocar should be: (1) *Easy of insertion*, the puncturing tube entering by a clean incision, and the protecting canula following without the least stretching of the aperture, or possibility of failure; (2) *Harmless when inserted*, so that it shall not injure the interior of the cavity, or adjacent parts; (3) *Safe as a probe or sound*, or artificial finger, for internal exploration; (4) *Adapted to the aspirator* and every form of exhausting apparatus, both attachment and detachment being instantaneous; (5) *Competent to give the freest exit or entrance* to fluids to be dis-

charged or injected; and (6) *Certain to leave an incised wound*, ready to heal. Let us now glance at the growth and present character of the modern trocar, and try to ascertain what should be done towards perfecting it.

In 1850 the improvements seem to have commenced, when Sir James Y. Simpson reduced the large head of the old trocar to the same size as the shaft, so that it should stretch the orifice less, and, as the canula could then be advanced without spreading, he omitted the split in the end, and published his employment of a long and slender trocar of this kind, with an exhausting syringe attached, as a means of diagnosis in various internal enlargements, especially in pelvic tumors, and so initiated the idea which Dieulafoy afterwards developed into his admirable aspirator. The plan of having the canula itself pointed, so as to penetrate independently of the stylet, was first conceived by Fergusson, of London, who in 1853 devised a pen-like tubular needle, for injection of perchloride of iron in the treatment of nævi and aneurisms. In 1858, Dr. Alexander Wood, of Edinburgh, adapted this instrument to the subcutaneous injection of morphia, and a modification of the same instrument is now in universal use. Shortly afterwards, Mr. Spencer Wells enlarged this little tubular trocar, for ovarian tapping, and had the edges of the pointed end made sharp and cutting for one half the circumference of the tube, whereby a semilunar incision is made; and he added, or restored, a shorter sliding canula on the outside, which, upon the puncture being effected by the point of the inner tube, is pushed forward as a guard against its further cutting action.

This instrument, superior in many respects to the old trocar and canula, especially in that the wound made is incised, rather than punctured, has still this grave fault of the old instrument, that the protecting canula is on the *outside*. If the distal orifice of this outer canula be made thin to pass with less resistance through the aperture made by the pointed smaller tube, the open thin end will itself present a cutting edge dangerous to the interior of the cavity and to the contained or adjacent viscera. To lessen the difficulty of getting the outer canula through the aperture, over the inner tube, the distal end is sometimes made obtusely pointed, but this makes it only the more dangerous to the interior, especially in the smaller or aspirator sizes of the instrument. If on the other hand the terminal margin of the outer canula be blunted or made thicker, it will present a ridge or shoulder outside of and behind the entering point of the inner tube, liable to catch and carry before it the sac, or the immediate investment of the cavity, and so fail to properly enter.

Now, this accident may occasion considerable inconvenience, or even be productive of great harm, in cases of simple tapping; as for instance in hydrocele and in retention of urine, where, if the tunica vaginalis or bladder be not very tense, it is oftentimes difficult to get the outer canula in without pushing so hard as to endanger its going in with a jump, and hurting something, so tightly does the punctured membrane cling to the inner tube in advance of the edge of the outer canula; and in empyema, with a thick and tough pleura, so much force is sometimes required to urge the entrance of the outer canula, as to endanger a separation of the pleura from the ribs. And in tapping for the temporary relief of ovarian dropsy, if the outer canula do not follow the trocar, or inner tube, quite into the sac, or if in entering it catch and tear or split up a fragile sac, more or less of the cystic fluid escapes into the

abdominal cavity, and forms one if not the chief cause of the fatality which sometimes follows this simple operation.

But should *injection* be attempted with an imperfect introduction of the protecting tube, whether that tube be the canula of the old trocar, or the outer tube of Mr. Wells's trocar, the result may be most disastrous. I have seen intense peritonitis occasioned by tincture of iodine thrown upon the peritoneum in such an attempt to inject an ovarian cyst, and Syme, in his "Principles of Surgery," noticing a like faulty injection for cure of hydrocele, remarks: "If the liquid is allowed to remain in the cellular substance, it gives rise to violent inflammation, and soon terminates in sloughing of the scrotum."

I have encountered on many occasions embarrassments similar to the above, and in common with others had tried to obviate the uncertainty of entrance of the *outer* canula, by cutting down to the bladder or tunica vaginalis, or pleura, or peritoneum, or whatever might be the immediate investment of the fluid to be evacuated; until it occurred to me to *reverse the relation* of the tubes to each other, and have the protecting tube on the *inside.* And while in Edinburgh, in 1871, I had a double, tubular trocar made by Gardner upon the following plan: The outer tube, smooth and of uniform size, has the distal end pointed and cutting like a lancet, so that it penetrates easily, and to any depth, without any need of previous incision of superimposed tissues; and the protecting tube, being now inside, may be advanced into the interior of the cavity, with absolute certainty of entrance, and without a possibility of even touching the margin of the aperture, or any of the tissues through which the outer tube has passed. The cutting edges must rise from the point of the trocar by a very acute angle, not more then 30° for the largest size and only 15° for the aspirator sizes; and the cutting-edges must be *very thin* and *sharp*, but of course sharp for only one-half the circumference of the tube, the half next the point, as if sharp all around a circular piece would be cut out, and a round hole left.

Instruments similar to the above were made under my direction by Krohne and Sesemann, in May of that same year, and were exhibited at the next meeting of the British Medical Association, in London, August, 1873. The largest size is noticed in Spencer Wells's recent work on "Diseases of the Ovaries," p. 336. But the smaller sized instruments seem to have been overlooked, for Mr. Alfred Goodrich, in a letter to the British Medical Journal, August 8, 1874, says: "In emptying a cavity with the aspirator the operator is often alarmed by finding the instrument filled with blood, arising from the walls of the collapsing cavity being forcibly sucked against the sharp point of the needle;" and he proposes, as if it were his own idea, that the trocar consist of two tubes, the *outer* one pointed, the *inner* one not so. But in the same journal for August 22, Mr. George Brown, of the Northeastern Hospital for Children, referring to Mr. Goodrich's suggestion of a trocar guarded by an inner tube, remarks: "The idea is not original; we have had one in constant use for more than twelve months, which was supplied by Messrs. Krohne and Sesemann." Now these excellent instrument-makers, in 1871, three years before this correspondence, constructed my instrument, which, in the catalogue of the museum at the forty-first annual meeting of the British Medical Association, was designated "Wells's Trocar improved by Fitch."

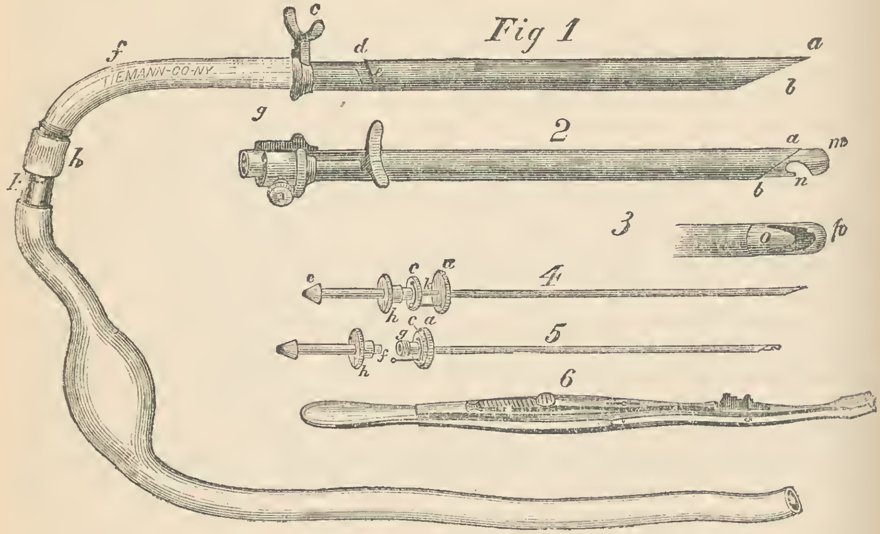
This instrument is not perfect; for, although the entrance of the protecting tube is rendered free from the possibility of failure, yet the *open*

end of this same tube is hazardous, especially in the aspirator-trocar, where it must be thin and sharp, to avoid bulk; thus in tapping the bladder, we dare not draw off the last of the urine, lest we injure the lining of the contracting viscus, and this danger is pretty generally felt by surgeons. In the August numbers of the journal just quoted, a correspondence appeared in reference to a successful paracentesis for hydrops pericardii, in which Mr. Singleton Smith, upon whose patient the operation had been performed, admitted the risk which accrued, not only from the unguarded point of the aspirator-trocar in the cavity of the pericardium, as the fluid was drained off, but also from the sharp edge of a canula rubbing against the pericardium, or coming into contact with the beating heart. About the same date, the Lancet, under the head of "Medical Facts," observed that in "tapping of the chest, when the fluid has been evacuated by the exhausting apparatus, the lung in expanding may strike against the sharp and hard canula. To prevent this, M. Béhier, of Paris, uses a canula of soft metal to be introduced into the ordinary tube, which, when the pleura is emptied, bends down against the parietes of the chest, and the lung does not suffer." Now a tube of soft metal cannot be drawn very thin, and, were it made large enough for the bore to be permeable to fluids, it would be too bulky to enter a small canula, and then coughing or other movement of the patient would be liable to throw the lung against the soft tube, and bending it down, stop the flow prematurely. Perhaps enough has been said and quoted upon this point, and upon the disadvantages of the open canula; I think we all feel the want of a safer trocar than any hitherto used.

I now announce a most important modification of the double tubular trocar, which avoids the danger of the open canula, and by which the instrument, while performing its highest achievements of discovery and cure, may be used as a trustworthy exploring probe and sound; and which will, I believe, in time supersede every other form of the instrument. Retaining my first improvement of making the outer canula the puncturing, or rather the incising, trocar, I have had the distal orifice, or open top, of the *inner* canula closed over by a rounded or dome-shaped roof, so that, when it is projected beyond the cutting point of the outer canula, the two tubes fit closely together, and the end of the combined instrument feels perfectly smooth like the end of a rectal sound, or catheter, or probe, and may be freely moved within the cavity penetrated, whether this be an ovarian cyst, a uterine fibro-cyst, the abdomen, the thorax, the bladder, a joint, or even the pericardium, without danger of wounding any viscus or organ, puncturing any vessel, or even scratching or abrading the lining of the cavity, or of any parts contained therein.

The base of this *dome* being of the same external circumference as the inner tube, of which it is the continuation, and fitting the outer tube accurately, there can be no escape of fluid till the dome is advanced or pushed out so as to occlude and shut out the cutting point of the outer tube; then there is disclosed by this movement a fenestra, or oval aperture on *the under side* of the inner tube, just below the roof, or dome, cut out of the lower wall and one-third of each side-wall, of the full size of the bore of the tube, and by which fluids may be freely evacuated or injected; the distal end of this segment is sloped off towards the dome, so that no obstruction can lie there, while at the proximal boundary a curved lip projects over one-third of the whole fenestra to prevent the possibility of obstruction; and the fenestra thus guarded, and being,

moreover, on the under side, cannot be stopped by the wall of the cavity coming into contact with it, nor by the falling upon it of any natural textures, or layers of false membrane, or flakes of plasma, as often happens with the open end of the old canula. If, while dis-



Figures 1, 2, 3, represent the ovarian trocar; 4, 5, the aspirator trocars; the intermediate sizes for paracentesis abdominis, paracentesis thoracis, etc., and the transfusion trocar, are sufficiently represented by one or all of these figures. Fig. 1 shows the cutting point (a b) of the outer canula advanced, ready for puncture, with the dome of the inner canula retracted, shutting the instrument just behind the point (b) against ingress or egress of fluid; c is the thumb-rest for projection and retraction of the dome by the thumb of the hand holding the instrument; d is a slot with a knob regulating and fixing the dome and point in any desired position; turning the knob one-half revolution into the proximal transverse slot allows the tubes to be separated for cleansing and oiling; f is a continuation of the inner canula, forming a tubular handle, which directs the current downwards—one end of an India-rubber tube is drawn over the lower orifice of this handle; k is a bit of glass-tubing by which the character of the fluid may be observed, or its absence noticed; the bulb is a simple expansion of the India-rubber tube (without valves or joints to interrupt the easy flow of fluid or to whip the blood in transfusion). Fig. 2 has the thumb-rest pushed forwards and turned into the distal branch slot, projecting the inner tube and dome (m), sheathing the cutting edge and point (a b) of the outer canula, and disclosing the fenestra (n) cut out of the under and side walls of the inner canula; n, in Fig. 2, and o, in Fig. 3, show the curved process of the lower wall of the inner canula, underlying the proximal third of the fenestra, to prevent occlusion from contact of cyst-wall, or vein-wall, or any floating substance. Fig. 4 is the aspirator trocar, with the dome retracted; Fig. 5 has the dome projected; e h is the nozzle; e the proximal end, over which the end of the India-rubber bulbous tube or the tube of the aspirator may be drawn; f the conical distal end to be inserted into (g) the funnel-shaped end of the inner canula, and fixed by a turn of the hose-coupling nut (h). Fig. 6. The clamp forceps for holding the sac upon the trocar during evacuation of the contents; also, for closing the orifice in the sac upon withdrawal of the trocar, and preventing leakage of cystic fluid into the peritoneal cavity.

charging the fluid for which it was introduced, there be found an aggregation of cysts, or a multilocular sac, this instrument may be used as a long artificial finger to examine the interior of the original cavity, and to feel for a proper place to enter, where it may be held till the cutting point is advanced to make an aperture for its introduction.

Thus, in ovariotomy, it will be found extremely convenient, the left hand supporting the tumor and the right holding the instrument,

which can be instantly changed, by an easy movement of the same hand, from a trocar to a sound, and *vice versâ*, to define and puncture cyst after cyst, until the bulk of the whole is sufficiently reduced to admit of withdrawal through the abdominal incision, with only one aperture in the cyst-wall first punctured, and this always occupied by the instrument which thus prevents leakage; and the dome-trocar may here be used, where the end of the open canula could not be with safety, to stir up and liquefy the loculose contents, and to break down such obstructions to the flow as imperfect septa and membranous intersections, while it still plugs the original aperture, thus preventing escape of cystic fluid into the cavity of the abdomen; and it oftentimes obviates the necessity of enlarging the aperture in the cyst for the introduction of the hand, which procedure should be avoided, as involving overflow of cyst-fluid upon the peritoneum.

In operating for hydatids of the liver or kidney, the dome-trocar, of aspirator size, may be used to loosen and dissever these little bodies while the aspirator is extracting them through the same instrument. And we may, with one of these smooth-ended instruments, of suitable length, search for and drain off the last drops of urine, during aspirato-puncture of the bladder, which we dare not do with the end of the open canula, much less with the sharp point of the ordinary single-tube aspirator needle; or, while the dome instrument is within the bladder, we may use it to explore the interior both before and after emptying it. In cases of intractably enlarged prostate, I believe that we may properly reach the bladder by perforating this gland with a dome-trocar having a less curve than an ordinary sound, and thus not only relieve the bladder at the time, but give permanent release from the oft-recurring retentions. I have forced a common strong catheter through the prostate in such a case; and the patient, who was previously nearly worn to death with his disability, is now enjoying a new prostatic bit of urethra, and is independent of instruments.

For explorations of bone, we may insert a probe-sized instrument, with the point projecting, till we reach the proper depth; then, advancing the dome, we have a harmless probe with which to search for caries or exfoliation, leaving only a capillary wound, without entrance of air or exit of blood. The dome, in all the sizes, should be solid or accurately floored over, to prevent lodgment of anything there which might impede its easy withdrawal from the cavity.

The life-giving operation of *transfusion* may, I think, be quickly and well done with this instrument. A short dome-trocar, of suitable size, having been attached to each end of an India-rubber tube a foot long, with the middle expanded into a bulb, one of the trocars is inserted into the vein which is to furnish the blood, and, when the apparatus is filled, the other trocar is introduced into the receiving vein, when the operation is completed. The tubes are closed and opened at their distal ends by retraction and projection of their domes, which prevents the possible admission of air; and no valve or stopcock is needed. The receiving vein should be exposed by a short incision, but the supplying vein will generally be sufficiently prominent to be entered without previous dissection. As soon as the lancet-end of the outer tube is inserted, the dome is projected, and the tubes thus guarded may be safely pushed as far as required, downward into the furnishing vein, and upward into the receiving vein, and no ligature will be needed. Thus time, so valuable

in this operation, is saved, disturbance of the vein is avoided, and injury to the interior of the vein need not be feared.

If the *mediate* method be preferred, a common glass, or hard India-rubber, or metal, syringe, with the piston removed, and the nozzle inserted into a flexible tube, armed with one trocar, will be a suitable reservoir into which the blood may be caught, as in ordinary venesection; or the blood may be defibrinated by whipping, and strained into the syringe; the dome with the open fenestra is left projected till the trocar fills, then it is retracted, closing the fenestra, and leaving the point of the outer tube ready for puncture. Upon inserting the trocar, we need not replace the piston, for sufficient and more steady propulsion may be obtained by merely raising the syringe.

The hole in the side of the nozzle, used by Mr. Wagstaffe, is liable to occlusion from the contiguous wall of the vein; in the dome-trocar this is obviated by a curved projection of the tube-wall over the proximal end of the fenestra, open at the sides, as previously described. In this operation it is very important that the dome be solid to prevent lodgment of clot.

The *aspirator attachment* deserves attention, for it can be applied to any syringe or exhausting apparatus; the adjustment is effected instantaneously and without moving either trocar or exhausting apparatus, or twisting the flexible, connecting tube, by merely pushing the end of the aspirator nozzle into the funnel-shaped end of the inner canula, and fixing it by one turn of a loose ring-nut, like a hose-coupling. The India-rubber tube connecting the nozzle with the aspirator has the usual bit of glass-tubing, so that the current may be observed or its absence noticed.

Some peculiarities of the different sized instruments should be mentioned. The *ovarian trocar* has a thumb-nut (for which I have to thank Dr. Thomas Keith), by which either the cutting point or the dome may be advanced or retracted, and fixed in either position by the thumb of the hand holding the instrument. The proximal end of the inner canula is prolonged into a hollow, curved handle, very convenient to hold by, while it also directs the current of the flowing liquid downwards; and one end of an India-rubber tube, three feet long (with a bit of glass tubing in it), may be drawn over the lower orifice of this hollow handle, to conduct the fluid into a receiving vessel; the middle of this tube is expanded into an elastic bulb, by which the flow through the tube may be promoted until the siphon current is established; and we may use it for washing out or for injecting the cavity. Mr. Wells's grapples may be slipped upon this trocar, or long, light clamp-forceps, with ring ends, may be used to seize the sac, upon or even before puncturing, and, held in the hand with the trocar, will accommodate themselves to the varying distances to which the trocar enters.

The *trocar for paracentesis abdominis* has a curved, hollow handle continuous with the inner tube like the ovarian trocar.

All the sizes below that for paracentesis abdominis, have their proximal ends adapted to the aspirator nozzle, and therefore a separate, curved, hollow handle is provided to fit all of them, and may be instantly fixed to either by a ring-nut, similar to that of the aspirator nozzle; and with this handle an India-rubber tube and bulb may be used, as with the ovarian trocar, when we wish to simply empty a cavity without the aspirator.

The instruments may be of any size. Of those which I have had made, the *ovarian trocar* is ten inches long, including the handle which is

four inches, with the internal diameter of the inner tube half an inch. Dr. Washington L. Atlee tells me that he has used one of these in his last forty-one ovariotomies, and expresses unqualified approval of it.

The *abdominal trocar* is six inches long, including two inches for the handle, the calibre equalling that of a No. 11 catheter of the American scale, 17 of the French. The *smallest sizes* correspond with Dieulafoy's aspirator needles; of these, two are each seven inches long, and fine, for the bladder and deep tapping; two are four inches each in length, and stouter, for hydrothorax, transfusion, etc.; one is short and very fine, for hydro-pericardium, spina bifida, etc.

UTERINE HEMORRHAGE DURING GESTATION.

BY

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THE object of the present paper is simply and briefly to point out a form of hemorrhage met with during pregnancy, heretofore unrecognized by writers upon midwifery. It is foreign to my purpose to speak of the subject in general, or even to recall to your minds some excellent papers that have lately appeared from the pens of the well-known and honored Dr. Fordyce Barker, and others.

I will first offer the history of the following case, as illustrating the subject:—

Mrs. N., æt. 39 years, and of English parentage, was a feeble, anæmic woman, of dark complexion, spare in flesh, and of a lymphatic temperament. She had been married some sixteen years, and was the mother of five children, the youngest of whom was eight years of age. She had been deserted by her husband after the birth of her last child, but he had returned to live with her during November and part of December, 1874, after which time he had taken his final departure for parts unknown. The menstrual flow had appeared as usual in January, 1875, but for the first time had been rather profuse, and had never entirely ceased; although the hemorrhage had moderated between the terms, it had been increasingly severe at each period up to the removal of a dead fœtus, of about the size of a third or fourth months' child, on August 7, 1875. For some months the patient had had constant pains in the uterus, of a sharp cutting character, and, as she remarked, "not at all like labor-pains." The condition of the patient, and the persistence of the pains, with the bleeding, led me to suspect malignant disease of the organ.

To make a diagnosis, the os was dilated sufficiently to allow the introduction of the index finger, when I felt a globular body, well up in the cavity of the uterus, and barely accessible to the touch. Dilatation of the os was continued until the greater part of the hand could be introduced. From the outset there were no indications of the presence of the decidua; the mucous membrane around the lower segment of the uterus was smooth and glossy to the touch; the cavity of the womb was found to be occupied by a projecting body somewhat like a large sessile tumor, with its base toward the right side, and occupying about one-quarter of the superficial surface. This body was felt to be a fœtus in its amniotic and epichorial decidual envelopes; the point of coalescence between the epichorial and parietal deciduæ was easily recognized.

The condition of matters having been determined, the membranes were ruptured, and the fœtus with its placenta removed. This, however, was not effected without difficulty, as the epichorial membrane had to be forcibly separated at the point of its uterine attachment; the sensation felt in so doing was very perceptible and characteristic. The parietal decidua was in no way interfered with by the delivery thus accomplished. The uterus contracted well, and the patient made a good recovery without any subsequent return of hemorrhage.

The uterine decidua has been the centre of attraction to many able investigators during the past few years. Much has been done toward elucidating its anatomy and physiological functions. The changes, truly marvellous, that occur during ordinary menstruation are somewhat definitely known. From this basis, so well established, we may reasonably hope for rapid advancement in the successful treatment of uterine disease, and from the same source we may expect to obtain hints for the more correct appreciation of some of the pathological conditions met with in pregnancy.

To understand the irregular development and behavior of the mucous membrane, we must bear in mind its regular and normal course in gestation. This course may be briefly stated as follows: (1) Hypertrophy and folding of the mucous membrane preparatory to the reception of the impregnated ovum; (2) The imbedding of the ovum in the decidua, and the consequent dividing of that membrane into three, viz., the utero-epichorial, the epichorial, and the parietal deciduæ; (3) The subsequent union of the epichorial and parietal deciduæ, forming one homogeneous membrane. As soon as this is accomplished, the uterine cavity is perfectly occluded, and any discharge from the interior is impossible.

A normal conception and gestation must proceed in the above order; when any serious deviation occurs, difficulties must follow; deficient vital action, and excessive vital action, alike work mischief. In the latter case, a fold of the mucous membrane may protrude into the canal of the cervix, or even pass outside the external os; a case of this nature came under my notice a short time ago, in which the protruding mass appeared to be a plug of tenacious mucus; and it was only when this was forcibly removed that the nature of the case became known. I need hardly say that abortion followed. On the other hand, deficient vital action is followed by imperfect development of the membrane, and an absence of that perfect union of the epichorial and parietal deciduæ which takes place in normal pregnancy. This form of abnormal development is met with, as might be expected, in anæmic and exhausted women; it also occurs in those of apparently good, general health, but whose vital powers have been exhausted by too frequent and rapid gestations. Nature tries to meet the requirements of the case, but her powers are too feeble to do so efficiently.

The epichorial decidua, being deficient in vitality, fails to enlarge and allow the embryo fair scope for development. The envelope acts as a compressor, forcing the fœtus toward the placental surface, and at the same time interfering with its nutrient supply and growth. The ovum and its envelopes, in fact, form a sort of sessile tumor, such as was noticed in the case referred to.

The muscular structure of the uterus is apt to be excessive in growth, and not deficient, as at first thought we might be inclined to suspect: we know, however, that hyperplasia or subinvolution are not indications of excessive vital activity, but rather the reverse; most probably it will be found that subinvolution is markedly present. Clinical facts indicate this view as probably correct. In the case mentioned, the uterus was much larger than was necessary for the accommodation of the fœtus. There was no impediment to the escape of any fluid from the left Fallopian tube to the vagina, a condition which would permit of free menstruation if that function could really be performed during gestation, which I very much doubt. That hemorrhage can really occur from the cavity of the uterus, and that, too, from a decidua of pregnancy without

causing abortion, is quite possible, and need not be doubted; perhaps we have in this condition the clue to the mystery of super-fœtation.

The purport of these remarks is to explain the cause of the uterine hemorrhage met with during gestation. The form of gestation and the condition of the uterus itself give an easy solution to what has heretofore been inexplicable; the exact point or points of hemorrhagic escape will undoubtedly be ascertained hereafter. The condition of the enlarged uterus, the character of the mucous membrane, and the unnatural and irregular strain upon the organ, are sufficient factors to account for the monthly and continuous flow of blood referred to. We know that sessile and submucous tumors are frequent causes of uterine hemorrhage, and as this form of pregnancy resembles these cases in their chief outlines, it is naturally followed by similar results.

As to treatment, I have but little to say. When we meet with such pregnancies, whether complicated with metrorrhagia or not, it seems to me that the proper treatment is to induce abortion. Apart from the exhausting hemorrhages, cases are met with in which the uterine distress and constitutional disturbance are so great as to urgently demand our interference; there is little ground for hesitancy in the matter, as we cannot reasonably expect a normal termination to an abnormal gestation.

The following conclusions seem to be justified by what has gone before:—

I. That an abnormal development of the uterine decidua is not apt to occur.

II. That hemorrhages other than those of placenta prævia can take place from the cavity of the gravid uterus, only in cases where there is an abnormal condition of this membrane.

III. That menstrual hemorrhages can take place from the cavity of the uterus without accompanying abortion.

IV. That this condition might permit of superfœtation.

V. That the treatment indicated in such cases of gestation, when the uterine and general distress are great, is to remove the products of conception, whether such distress is or is not accompanied by metrorrhagia.

CHRONIC INVERSION OF THE UTERUS.

BY

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In introducing the subject of Chronic Inversion of the Uterus to the Section on Obstetrics, I propose simply to bring prominently forward facts and conclusions already in a great measure laid before the profession, and to urge the claims of the operation described to the confidence of this representative body.

Never having witnessed the occurrence of the accident in question, I will not attempt to describe its causes or the manner of its occurrence, but shall consider chiefly the question of treatment or restoration. The gravity of the accident and the sad condition of its victims will be conceded by all who have observed the miserable state of those suffering from its effects. Without discussing the possibility of spontaneous reduction, which has been asserted by one or two recent writers to sometimes take place, it certainly does not occur with sufficient frequency to warrant delay in our efforts for artificial reposition, in the expectation that a cure will be effected by the unaided efforts of nature.

Until quite recently, inversion of the uterus, unless restored within a short time after its occurrence, was regarded by all writers as entirely incurable, and the unfortunate female was left without hope of restoration, to drag out a loathsome existence. So miserable has the sufferer from this terrible accident been made by it, that relief by extirpation of the organ, with all its attendant dangers, and consequent deformity if succeeded by recovery, has been acceded to as preferable to living on in this deplorable state. Meigs, I suspect, at the time he wrote gave very briefly the sentiments of the profession in relation to the practicability of restoration, in his "Letters to his Class," on the diseases of females. Speaking of a case of six months' duration, after describing the feeble and exsanguine condition of the woman, he says:[1] "What are you to do for the patient? Will you reposit or reinstate this womb? You can't. You might as well try to invert one of the non-gravid uteri on my lecture-room table as to reposit this one. The time is gone by. You have no art or skill, nor any power equal to the performance of such a miracle of surgery as that." All the authorities of the same date regard the reposition of the uterus, after involution, as highly improbable, and do not direct any efforts to be made for its restoration. Churchill, Ramsbotham, Velpeau, and Bedford content themselves with directing such measures as will contribute to the comfort of the woman, and with suggesting that removal of the organ may, in extreme cases, be resorted to. Notwithstanding the success which has attended the operation for reduction of the inverted uterus during the last few years, especially in America, the more recent writers still seem incredulous. They continue to give the same directions for the alleviation and treatment of this con-

[1] Letters to his Class, by Charles D. Meigs, M.D., page 232.

dition that are to be found in the older authors. I have myself treated by reduction, since March, 1856, twelve cases of uterine inversion, varying in duration from immediately after the occurrence of the accident to no less than twenty-two years. Reduction in all was effected at the first effort, and, as far as could be ascertained, without laceration of tissue. By similar measures other gynæcologists in this country and in Europe have repeatedly succeeded in accomplishing reduction in cases of chronic inversion, and yet the feasibility of the method is by no means uniformly admitted by writers and practitioners at the present day.

Thus, Leishman, whose recent work on Midwifery is justly regarded as high authority, at first hesitatingly admits that "elastic pressure alone, or aided by incisions in the neck, will overcome every case of inversion except when fixed by inflammatory adhesions," but subsequently goes on to describe the method of removing the uterus "when the doom of the patient seems fixed;" and adds, under such circumstances, "we can have no hesitation in resorting to a measure so extreme as the removal of the organ."[1] Schrœder, a German authority whose work has been translated and published in this country, dismisses the whole subject of chronic inversion by saying, "Reposition is easy as long as the os is not yet firmly contracted; otherwise great difficulties may be met with, and repeated trials under chloroform may become necessary."[2] Meadows, in England, in his Manual of Midwifery, says: "When the case has remained unrelieved for many weeks, though attempts should always be made to secure reduction, etc., yet the chances are very greatly against success." Courty says that, if reduction be impossible, it will be necessary to resort to palliative treatment or to extirpation of the uterus for its radical cure. Dillinger, in a work republished in London in 1871, says: "Chronic inversion is held to require palliation; if that is vain, removal by ligature or excision." Barnes, of London, thinks it necessary to invent some new operative procedure, in order to avoid "the various methods of amputating the uterus, or of effecting *forcible* reduction, as by Professor White's method," etc., and accordingly recommends incisions in the neck, and gives a plate of an elastic pessary, to aid in effecting gradual pressure. He very justly admits the failure of elastic bags in the vagina, as they, when distended, stretch the vagina excessively, require a firm *point d'appui* in the perineum, which cannot always be had, and also fail to exert pressure upon the fundus with sufficient force and directness. He might also have added, in relation to the elastic bags, that they produce painful pressure upon the rectum and neck of the bladder, and cannot be borne by the patient for any considerable time.

In the abstract of Society proceedings, given in the Obstetrical Journal of Great Britain and Ireland, republished in Philadelphia, in the August number, 1873, will be found a very full report of a discussion in the Obstetrical Society of Dublin, on the subject of chronic inversion of the uterus. Two cases, of six and seven months respectively, were reported by Drs. Kidd and Johnston, giving rise to a debate on the subject in which several members present participated. Dr. McClintock, in the course of his remarks, said that he was satisfied that cases would arise where all manipulations would entirely fail to replace the organ. The President, Dr. Evory Kennedy, said that, in dealing with these cases of

[1] Leishman's System of Midwifery, American edition, p. 417.
[2] Manual of Midwifery, American edition, page 315.

inversion, they could not classify them merely as acute and chronic. He thought there was an intermediate stage, in which it was *just possible* to reduce by compression the uterus back into its place, but when that stage was passed, the thing became *impossible.* He looked upon it that up to four or five months after the accident the case was not a chronic one, and that facilities still existed for reduction which did not exist at a later period. Again, that he was not aware of reduction having been effected after fifteen years. He thought that the condition of the tissue would be so altered in the course of ten or fifteen years that it would be totally impossible to effect reduction at the end of that time.

In the January number of the Obstetrical Journal, for 1876, a description of the amputation of an inverted uterus is published without one word of editorial comment. It was of only three months' standing, and was excised with the *écraseur* by Professor Martini Barta, as the only means of arresting the hemorrhage incident to this condition. During the present year also, I find published, in the American Journal of Obstetrics, an article "On Ablation of the Body of the Womb in irreducible Uterine inversion, by External Hysterotomy," by M. Donné, of Paris. This paper, which was read before the French Academy of Medicine, arrives at the conclusion that external hysterotomy is an extreme surgical resource, but precious in cases of *irreducible inversion.*[1]

But it is useless to multiply citations illustrative of the hesitation and prejudice which still prevail on this subject, recommending, as several authors do, for the relief of uterine inversion, an operation of much greater danger to the patient than reduction, and one which leaves her, when she escapes with her life, in a sadly mutilated condition.

Whence this almost universal want of confidence? The cases of reduction are now so numerous, and the operation, in my opinion, so demonstrably practicable, as not to admit of doubt. I am utterly unable to account for this unbelief, and feel, therefore, justified in detaining you with a brief history of the steps by which I arrived at a different conclusion. Some remarks upon the method of operating, and a reference to three illustrative cases, will also be added.

There may be found in the Buffalo Medical Journal for March, 1856, an account of a case of inversion of eight days' standing which had been reported, with a description of the successful operation for its reduction, to the Buffalo Medical Society in the preceding February. I concluded that article with the following remarks: "This case is regarded as interesting in many respects. It will encourage the growing belief among accoucheurs that reduction may be undertaken with reasonable hope of success, at a period much later than most writers have heretofore advised." In the same article, alluding to a case of fourteen days' standing which had fallen under my observation long before (in 1842), and in which, in accordance with the teachings of that day, no attempt at restoration had been made, I added: "With my present views upon this subject, I should abandon such a case as hopeless only after a long effort at reposition." Fully convinced, from the result of the efforts made in this instance, eight days after inversion, of the feasibility of restoring the uterus in many cases heretofore considered irreducible, I did not meet with an opportunity of putting my convictions to the test until March 12, 1858, when I visited Mrs. M., near Hornellsville, N. Y., who had been confined on the 22d of September previously, almost six months before, and, after

[1] American Journal of Obstetrics, June, 1876, page 348.

about one hour's continuous effort, succeeded in repositing the uterus, which had been completely inverted. A full account of this case, with illustrative plates and a description of the manner in which reduction was accomplished, was published in the July number of the American Journal of the Medical Sciences for 1858. Permit me to crave indulgence for one word in relation to priority in this operation. Tyler Smith, in London, on April 24, 1858, published in the London Medical Times and Gazette an account of a successful operation for inversion. But I submit that this was forty-two days after my operation in Hornellsville, and more than two years after I had taken the initiative and had published my views and "hopes," in connection with the report of a case of eight days' duration.

Upon this point, I trust that I shall be pardoned for gratefully quoting the following passage from an able article on this subject by the late Dr. Henry Miller, the impartial and distinguished Professor of Obstetrics in the University of Louisville:[1] "Before I proceed to do this [give an account of the method of manipulation pursued by Dr. White], it is meet that I should pay a tribute of honor to our countryman, Professor James P. White, who has not received the credit he so well deserves for his leadership in the revolution of gynæcological practice which he inaugurated." The writer then proceeds to a careful analysis of the facts which led him to the above conclusion, which it is unnecessary here to detail.

On August 24, 1858, in the presence of Professors Austin Flint, Austin Flint, Jr., Thos. F. Rochester, W. H. Mason, and others, I reduced an inverted uterus of over fifteen years' standing. A full account of this case, as reported to the Buffalo Medical Association, with the remarks of the above-named distinguished gentlemen, will be found in the Buffalo Medical Journal for October, 1858. The difficulty encountered in the operation bears no proportion to the duration of the inversion. I believe it to be as difficult to restore the uterus immediately after complete involution has taken place, as at any subsequent period. In No. 9 of my series, a case of only six months' duration (at Port Dover, Canada, reported to the Medical Society of the State of New York, and published in the Transactions of that body for 1872), reduction was far more difficult than in the case of fifteen or in that of twenty-two years. In the case of six months, two protracted efforts, under chloroform, had been made before I saw the patient; each attempt had been succeeded by some peritoneal inflammation, and it was the opinion of all present at the final operation that reduction was protracted in consequence of firm adhesions of the uterus to the surrounding tissues. The same difficulty was encountered in my tenth case, one of nearly seven years' duration, in New Bedford, Mass. At an early period in the history of this case, as will be seen by the excellent report of Prof. Bixby, the *écraseur* had been applied, and such pressure made as resulted in breaking the chain of the instrument, the operation being succeeded by inflammation, which produced adhesions. The remark of Evory Kennedy, that "the organ undergoes such change in process of time as to render reposition impossible," is not well founded. The uterus becomes as small and inflexible immediately upon the conclusion of involution, which, according to the best authorities, occurs within twelve or at most sixteen weeks after delivery, as at any subsequent period

[1] Thoughts on Chronic Inversion of the Uterus, specially with reference to Gastrotomy as a substitute for Amputation of the Uterus. By Henry Miller, M.D. Richmond and Louisville Medical Journal, April, 1870, p. 14.

before the menopause. Reduction, therefore, at the expiration of twenty years, is not more difficult than after the same number of weeks, as far as the manipulation of the organ itself is concerned.

There is, however, an intervening period between the commencement and the conclusion of the process of involution, when the organ must, in my opinion, be handled with more care. Whilst undergoing this change, the uterus does not possess the firmness and elasticity of the unimpregnated organ, nor the muscular flexibility and toughness of that at full period of gestation. Whilst in this transition state, undergoing the process of fatty degeneration, it is, I apprehend, far more brittle and liable to rupture than after this change is fully accomplished. In the cases in which the operation was resorted to at eight days, at three weeks, and even at five weeks after delivery, as in my eighth case,[1] the organ did not possess the firmness of normal uterine tissue in the unimpregnated state. In the report of the last case (eighth) will be found the following remarks: "Indeed, I am induced to suspect that at this period after delivery the uterus cannot be subjected, without danger of laceration, to manipulations which would be perfectly safe at a later period after complete involution has taken place. The sensations occasioned by pressure were such as to excite apprehensions lest the tissues should yield under the fingers." It may be added also that this was the first case in which the repositor was used, and the safety of the structures may be attributed, in part, to its aid during the operation.

It will probably be found safer, in view of this friable condition of the tissues after the commencement of involution, to wait for the completion of this process, notwithstanding the increased difficulties of the operation occasioned by such delay. Priestly, in his lectures on the subject of the involution of the uterine parietes, assures us that "with the advance of the fatty transformations, the uterus becomes in a corresponding degree friable, and continues so until it has completely returned to its normal condition," and that "it is occasionally so soft and friable whilst affected by this fatty degeneration, that a sound passed into the uterine cavity might be readily pushed through the uterine walls." The impression given whilst manipulating the uterus in the course of involution confirms these remarks of Dr. Priestly, and leads me to suspect increased danger in all operative procedures upon the organ during this physiological process.

In relation to the *modus operandi* of the operation of reduction, there is little to be added to the description given in 1858, of the manner in which reposition is accomplished. There seems great and unnecessary ambiguity and confusion on this subject. In all recent cases, the fundus uteri can be pressed into the body and neck, or "dimpled," as it is termed, by pressure upon the most depending part. In this manner I am certain that the two cases which were reduced immediately after delivery, and to some extent that of eight days' duration, were carried up. In the case of Dr. Lockwood's patient, in 1861, the womb had been inverted for about forty minutes when I arrived. Administering some paregoric and brandy to relieve the patient's semi-collapsed condition, I seized the uterus, pulled off the partly adherent placenta, and passed my hand, with the fundus before it, up through the neck and into the

[1] Report of two cases of Chronic Inversion of the Uterus, with a description of the Uterine Repositor. By James P. White, M.D., Transactions of the New York State Medical Society for 1872, page 200.

cavity of the replaced womb, with scarcely more difficulty than would be encountered in inverting a wet bladder. Retaining my hand there for a short time, contraction soon came on under the influence of the ergot and stimulants which had been freely given. The patient made a good recovery, and has since borne children. So with the case of eight days, it is stated in the report that, "having succeeded in 'dimpling' the fundus, pressure was maintained with the thumb at that point until the hand had become nearly powerless. To preserve this depression whilst the muscles of the hand were permitted to rest, a rectal bougie about twelve inches in length and one inch and a fourth in diameter, was carried along in its place, fixed in the dimple, and pressure unintermittingly continued through it, by the left hand outside the vulva. Gradually, the concavity of the fundus was found to be deeper, until it finally became completely restored."

This is doubtless the manner in which reduction takes place in all recent cases, whilst the organ is large and the walls flexible, and I was at first led into the error of supposing that this same method obtained in all cases. This delusion was, however, completely dispelled by my first case of chronic inversion, of six months' standing, in the account of which the following description of the method of reduction is given.

"There can be no doubt that the os first commenced to yield, and pressed down upon the intra-vaginal hand, which, it will be recollected, inclosed the entire body and fundus of the uterus, and the upper extremity of the bougie, and kept them in contact. This part [the os] gradually dilated, and passed down upon and over the neck, which in turn dilated and doubled down upon itself. The fundus did not perceptibly dimple in its centre or at either Fallopian orifice, and was not reflected upon itself during the operation. The organ was too firm, and the cavity too small, for any depression to be made upon the walls of the fundus. In recent cases, on the contrary, reduction may doubtless be effected by doubling-in, or dimpling, the fundus, and using it as a wedge to dilate the neck and os. That reduction takes place in all cases after complete involution by thus dilating the os and reflecting it down over the neck and body consecutively, has been several times verified by examinations made by competent observers, as well as by myself, during the progress of these operations. The uterus and vagina in complete inversions represent a continuous bag or sac doubled or reflected upon itself, with the open extremity of the sac securely fixed. Pressure upon the closed end of the bag (the fundus) will, it is plainly perceived, under such circumstances, result in straightening the bag by completely turning it the other side out. So with the parts concerned in inversion. Pressure upon the fundus, if gentle and well directed, pulls open first the mouth of the womb, then its neck, and finally, if persevered in, doubles the body upon itself also, and carries the fundus through the os, and neck, and body, to its normal position."

I have nothing to change in this description, given twenty years ago, of the *modus operandi* of reposition in chronic inversion. In the cases of fifteen years' and of twenty two years' standing, the attention of the distinguished gentlemen who were present was called to this fact (the reflection of the os upon the neck and body), and they were requested to pass a finger up beside the hand of the operator and verify the observation. Similar careful examinations were made by those present at the two most difficult of all my reductions, that at Port Dover, Canada, and that at New Bedford, in both of which peritoneal inflammation had been excited by previous surgical interference, resulting in adhesions, and in both also the same relation of parts was ascertained to exist during the progress of the operation. In my opinion it would be unwise

to double in the fundus upon the body, after complete involution, even if it were practicable to do so. The size of the tumor to be carried up through the cavity of the neck and os, would be increased by this introflexion, and would, by just so much, augment the difficulty of reduction. Any one who may make the effort to dimple the fundus of the unimpregnated uterus into the cavity, will perceive that it is impossible to effect reduction or inversion in this manner even upon the dead subject.

By this explanation all the apparent discrepancies, as to the manner of reduction, can be reconciled. One practitioner has operated on a recent case, and restored the womb by dimpling in the fundus, whilst his no less observing and truthful fellow has restored the involuted organ by reflecting the os over the neck and body, without at all being able to depress the fundus.

It is incorrect for Dr. Barnes to call the procedure just described, "a forcible reduction." I submit that there is no conceivable method of dilating the os and neck of the uterus less forcible than that of making gentle, elastic pressure upon the fundus, producing thereby equable and moderate traction by the vagina which is attached within the os in complete inversion. The vagina pulls open the os with less liability to laceration than the pressure of the tent, the India-rubber bag, or any other mechanical means can do. Does any gynæcologist of the present day hesitate to dilate this canal for the removal of intra-uterine growths? Does any uterine pathologist believe that it would be impossible safely to dilate the os and bring down the fundus of the uterus—completely inverting the organ—if carefully and perseveringly undertaken? If these questions are affirmatively answered, why may we not *pull open* the neck by means of the vagina in the same gentle manner as we could *press it open* when in a normal position, and thus carry the fundus up through it by means of pressure upon that part when it is in a downward direction? Perhaps I may be too sanguine, but my present belief is that well-directed pressure upon the fundus, if continued long enough, will, in all cases, unless prevented by firm adhesions, result in restoration or reposition, no matter how much time may have elapsed since inversion has occurred. Observation and careful reflection based upon the cases which have been committed to my charge, now numbering twelve in all, and varying in duration, as already stated, through nearly a quarter of a century, confirm me in the opinion first expressed in 1858.

Indeed my convictions are very decided, and I am incredulous as to the necessity of ever resorting to amputation, or to the still more objectionable operation of gastrotomy. True, the womb may not always be reduced at a single sitting, as in all the cases which I have encountered; but, by means of the "repositor," uniform and gentle pressure can be maintained until the os is fully dilated and the fundus pushed up through it. The insurmountable difficulty heretofore has been supposed to consist in our inability to maintain uniform and persistent pressure for a sufficient length of time. The hand would soon become fatigued, and another hand, even of the same individual, could not be substituted without losing a part of what had been gained. This loss is increased when the hand of a fellow practitioner is introduced to continue the operation. No doubt great physical endurance, being able to maintain one position for a great length of time, has been an essential element of success. The various substitutes which have heretofore been resorted to for continuing pressure when the operator has become exhausted, have utterly failed. The elastic bags, so often called in requisition, press more upon the viscera

resting upon the large surfaces anteriorly and posteriorly situated, than upon the fundus, which has no firm ossific base of support as have the rectum and bladder. The uterus ascends very soon, owing to the yielding nature of the vagina and perineum, and escapes from the reach of the distended vaginal bags. The repositor (which will soon be described), when it is deemed better to proceed in a more gradual manner, or when it may be found impossible to effect reduction by a single effort, can be received with the uterus into a large cylindrical speculum, and, by means of a "T" bandage, can be made to press directly upon the fundus until the os is gradually dilated and all resistance overcome. By means of the large spring at the outer extremity, the amount of pressure can be graduated to an ounce. The disk of this instrument will follow up the fundus, without compressing painfully the urethra or rectum, by means of this continuous elastic pressure in the upward direction, until the fundus disappears in the os and neck. Any intelligent assistant can be trusted to increase or diminish the pressure, during the absence of the operator, as the exigencies of the case may demand.

The construction and action of the "uterine repositor" will be readily understood by reference to the accompanying wood-cut, Figs. 1 and 2.

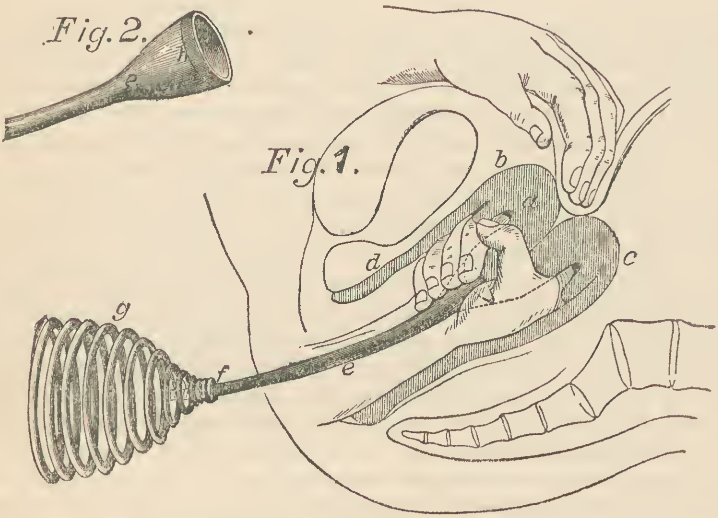

Fig. 1. *a*. Uterus in process of reduction. *b*. Anterior lip or wall of the uterus with the fingers of the left hand pressing upon it and assisting in pulling open the uterine cavity. *c*. Posterior uterine wall semi-reflected. *d*. Anterior vaginal wall. *e*. Wooden or hard rubber stem of Repositor, its enlarged extremity held in contact with the fundus by the intra-vaginal hand of the operator. *f*. Distal extremity of stem made into a screw, so as to be fastened into *g*, a coil of No. 11 steel spring wire, requiring eight or ten pounds pressure by the breast of the operator, against which it is placed, to bring it down.

Fig. 2. *h*. Uterine extremity of stem *e*, which is terminated with a soft India rubber disk $1\frac{3}{8}$ inch diameter, the concavity into which the fundus is received being about one-half inch deep, with its terminal margin thin and soft.

The instrument is composed of a stem of wood or hard rubber, curved to conform to the vaginal curvature, with a coil of steel wire attached to the outer extremity, whilst the other end is expanded and hollowed so as to receive the fundus of the uterus in its concavity or disk. The edge of this disk is tipped with soft rubber, being an inch and three-eighths in diameter, and about half an inch deep. The concave extremity of

this instrument is carried up into the vagina and placed in contact with the fundus, and then firmly held by the hand in the vagina. The outer end of the instrument, or coil of wire, is placed against the breast of the operator on the same level with the uterus. By means of this large circular spring, the instrument readily keeps its place on the clothing of the operator, and leaves the other hand free to be used above the pubes to assist in fixing the uterus and assist also in forcing open the dilating os, which can ordinarily be plainly felt through the abdominal walls.

The spring at the outer end of the instrument enables the operator without danger of lacerating the tissues to keep up a constant gentle pressure upon the fundus, and by leaning forward to increase this pressure intermittingly. The force thus exerted is applied more directly upon the fundus by means of the repositor than would be possible if the thumb and fingers were used, or the round end of the large bougie. I have often been delighted, since I have used the repositor, to find that it gave me a third hand which did not become fatigued, and which permitted me to use the left hand in manipulating over the hypogastrium, while the right easily held the instrument in contact with the fundus, and firmly grasped that part of the uterus which was not yet reflected, and which remained in the vagina. The disk, in which the fundus rests, is less likely to bruise or lacerate the organ than any other mechanical appliance. The intra-vaginal hand compresses the body and fundus, and lessens its vascularity, whilst something is gained by intermitting the pressure, also lessening by its use the exhaustion incident to unintermitting muscular effort on the part of the operator.

It may be well to state that the patient is always placed at the side of the bed with the feet resting in the laps of intelligent assistants, each of whom is also charged with the care of the knee and hand of that side. The hips of the patient are brought quite to the edge of the bed, which is raised so as to bring the parts on a level with the breast and arms of the operator. He should occupy the space between the extremities of the patient, resting upon his knees. This position gives him greater mobility than the sitting posture, and is much less fatiguing than standing. It is best to manipulate the uterus freely, thus rendering it more flexible and lessening its vascular congestion, before commencing the effort of restoration. The back of the hand should rest in the concavity of the sacrum, and should be completely introduced into the vagina, the body and fundus of the uterus with the upper end of the repositor being firmly grasped in the palm. The anæsthetic should be entrusted to a competent practitioner, and should be carried not only to insensibility, but as far as it can be without danger. It is unnecessary to add that the operation should never be undertaken without having had the patient's bowels completely evacuated a few hours previously. It is also unnecessary to say that it should not be undertaken during the menstrual flow. Sometimes, however, the hemorrhage is so continuous that the menstrual epoch is not well marked, when the operation may be proceeded with without fear of dangerous hemorrhage. It is remarkable, in view of the slight causes which usually give rise to hemorrhage in most cases of inversion, how little blood is lost during the manipulations incident to restoration. This is doubtless in a great measure due to the fact that the bleeding is controlled by the compression exerted by the hand around the body and fundus. Nor, if the manipulations are gently and carefully practised, is the danger of metritis by any means as great as might be expected.

I shall now give the histories of three typical cases, one of six months',

one of seven years', and one of twenty-two years' duration. In order to secure confidence in the feasibility of the operation, and avoid the bias which inevitably attaches to the observation of an enthusiast, I have selected two which were kindly reported by distinguished gentlemen who witnessed the operations. The first case is one of six months' duration, and the first in which I effected reduction after complete involution, although I had already successfully treated two cases of much shorter duration.

CASE I.[1]—On the third of March, 1858, Dr. C. D. Robinson, of Hornellsville, wrote to me that he "had been called in consultation with a neighboring physician, and found a patient with an inverted uterus of more than five months' standing." My opinion was desired as to "the possibility of returning the inverted organ and the propriety of extirpation." In my reply, the impropriety of removal, unless as a last resort, was insisted on, and the hope expressed that a prolonged and well-directed effort might succeed in reduction; and it was said that, in my belief, it was due to the poor woman that the attempt should be made before she was abandoned to the evils of chronic inversion. A few days subsequent to this date, I was requested to visit her at my earliest convenience. Engagements in the city prevented my complying with this request until the twelfth of March. On accompanying Dr. Robinson to the residence of the patient, Mrs. A. M., I found her extremely anæmic, confined to her bed, and suffering greatly from the loss of blood.

The history of the case, as furnished by herself, her husband, and the medical gentlemen who had been in attendance, was as follows: At the age of 30 she was taken in labor at the maturity of her second pregnancy, on the 22d day of September last, Dr. Batten in attendance. This labor was natural to the conclusion of the second stage, when she gave birth to a large male child. Placenta adherent, but removed at the expiration of about thirty minutes, and its delivery followed by copious flowing, severe pain, and faintings. The prostration was so great as to require the constant use of stimulants during the succeeding forty-eight hours, and for three weeks she continued extremely weak and faint. At about this time she took an aloetic cathartic, which occasioned violent efforts at stool, accompanied by pains resembling those of labour. Profuse hemorrhage followed these straining efforts, and a large pear-shaped tumor made its appearance through the os externum. The neck or smaller extremity of this body was at the vulva, and the larger extremity down between her thighs. By the assistance of her husband, she was enabled to return this tumor within the vulva, when a messenger was dispatched for Dr. Batten. Dr. B., upon his arrival, introduced his hand into the vagina and carried the uterus high up into the pelvis, and resorted to astringents and cold for the purpose of arresting the flow of blood, which continued profuse and difficult to control. The prostration being at this time very great, the horizontal posture was enjoined, stimulants and tonics were given, and the bowels were moved by enemata.

During the succeeding three months she had occasional hemorrhages, which were severe, with constant discharge of muco-sanguinolent matter. Two or three times, during this period, she so far improved as to walk about her room and partially supervise her domestic affairs, though looking very pale and being very feeble. About the middle of January she had another severe attack of hemorrhage, the tumor again presented itself externally, and was again returned as before; that is, pushed back within the vulva. Dr. B. again visited her, and prescribed such remedies as seemed necessary to control the flowing. Since about the first of February she has been compelled, from the debility consequent upon the exhausting, sanguinolent and leucorrhœal discharges, to preserve the recumbent posture. Lactation, doubtless, has added

[1] Vide Amer. Journn. of Med. Sciences, July, 1858.

to the exhaustion, and being confined to her bed she has little appetite, the stomach is irritable, and the bowels are costive. Ever since the patient took the aloetic cathartic and the tumor made its first appearance between the thighs, she has been aware of the existence of something unnatural in the vagina. This body has occasionally made its appearance externally, requiring the assistance of her husband to replace it, and she has had frequent attacks of a "straining sensation" described as accompanying its first complete descent. She has suffered greatly from all the symptoms arising from exhaustion and from sympathy with the uterine irritation necessarily developed by the malposition of the womb. The pulse now numbers 130; she is blanched or wax-colored, and grows dizzy and faint when raised to the semi-recumbent posture, and cannot be moved without experiencing a sense of prostration. It should have been stated that, on February 25, Dr. Robinson, of Hornellsville, and Dr. Reynale, of Dansville, visited the patient in consultation with Dr. Batten, when inversion of the uterus was diagnosed, and measures calculated to ameliorate her condition were resorted to.

Upon making a careful examination (nearly twenty-five weeks having now elapsed since confinement), the fundus of the uterus is found just within the os externum, and about one inch and three-fourths in its transverse diameter, and scarcely exceeding an inch in its antero-posterior diameter (Fig. 3). The body and neck of the organ occupy the vagina, and the neck is not more than one inch in diameter, and feels like the pedicle of a polypus. The inversion is recognized as complete, and the organ is no larger than when in its natural position six months after delivery. Introducing a large cylindrical speculum into the vagina, the fundus of the uterus passes readily into its cavity, thus demonstrating the complete involution of the uterus, and bringing distinctly into view the rough mucous membrane of its now outer covering, which bleeds upon the slightest touch with the finger or sound. It is seen to be covered with muco-purulent matter also, and is not susceptible of indentation by pressure with the point of the sound.

Fig. 3.

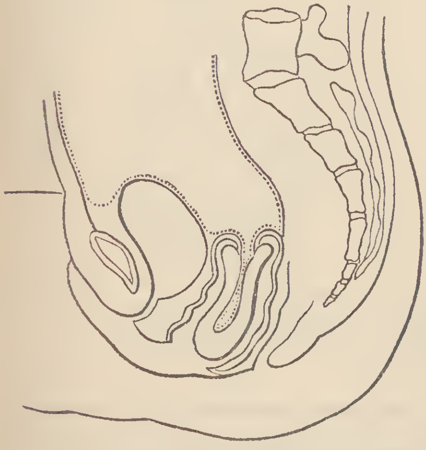

Fig. 4.

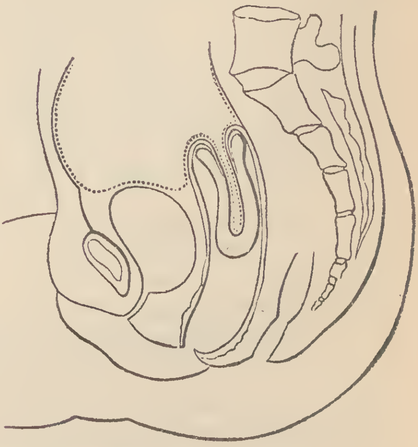

Fig. 3.—Position of the uterus in chronic inversion.
Fig. 4.—Vagina put upon the stretch by pressure upon the fundus.

The bowels having been freely moved by an enema, I proceeded to the operation of reduction in the presence of Drs. Robinson, Reynale, Batten, Dimick, and Mr. J. W. Robinson, medical student. The patient was placed, for the operation, upon an elevated, firm bed, with the hips brought quite to its edge, the knees separated, the feet resting in the laps of Drs. Reynale and Robinson, with directions to each to support a knee and hand of the patient, and prevent

her from moving about. Dr. Batten brought the patient moderately under the influence of chloroform, which was continued throughout the operation, whilst I placed myself upon my knees, between the patient's legs, her pelvis being at a convenient elevation for manipulation. I introduced my right hand completely into the vagina, and firmly grasped the entire body and neck of the uterus. It may here be remarked that the parts were so firmly contracted as to render the introduction of the hand difficult. At the same time that the entire uterine tumor was grasped by the right hand, a large rectal bougie was carried up, and also received into its palm and held firmly in contact with the fundus of the uterus, the hand being sufficiently large to receive both, and keep them in apposition. Continuous, gentle pressure was now made upon the external extremity of the bougie with the left hand, whilst the right compressed the uterine tumor, and kept the upper extremity of the instrument directly upon the fundus, and, with the dorsum of the hand in the concavity of the sacrum, directed the force in the axis of the pelvic cavity, putting the vagina completely upon the stretch (Fig. 4). This pressure was exerted, and this position unintermittingly maintained, although the force was not to such a degree as to endanger laceration of the utero-vaginal connection, until my strength was nearly exhausted from continuity of effort. At length, and when about to relinquish the task, the uterine tumor began to shorten at *its neck*, and the mouth of the organ to push upon the upper surface of the hand. No depression or dimpling of the fundus was at any time perceptible. Ascending more and more rapidly as the neck diminished in length (Fig. 5), the fundus finally passed out of the hand, and was easily pushed by the bougie through the mouth and neck of the organ up to its proper position (Fig. 6).

Fig. 5.

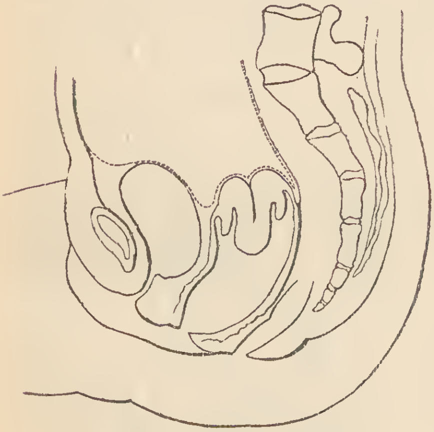

Fig. 6.

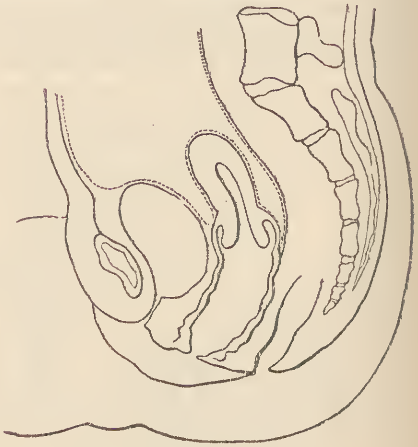

Fig. 5.—Manner in which the os and neck double upon the body.
Fig. 6.—After complete restoration.

In order to verify the restoration, Simpson's sound was introduced alongside of the bougie, and was found to enter a little more than two and one-half inches above the os, which could now be distinctly felt. The large speculum, already referred to, was now slipped up around the bougie, when the os was brought distinctly into view, surrounding also the bougie. The sound was again carried through the os to the fundus, through the speculum, and all the medical gentlemen present saw that it passed easily beyond the mouth, to the shoulder of the sound, and could not, without force, be carried further. Thus was demonstrated not only the reduction of the uterus, but that the organ was accurately measured, and found scarcely, if at all, enlarged. The speculum and sound were now withdrawn, the patient carefully removed to the bed,

and the bougie retained in place by the hand, to prevent reinversion. Meanwhile, stimulants were given to sustain the patient, and ergot in such doses as were deemed likely to excite the tonic contraction of the uterus. The patient soon recovered from the effects of the chloroform, and expressed herself as feeling quite as comfortable as before the operation from which she had suffered but little. The discharge of blood was slight, and, when the effects of the chloroform had passed off, and she had taken a little brandy and water, she expressed herself as feeling comfortable. Pulse not sensibly changed in quality, and numbering the same as before the operation.

Drs. Robinson and Batten remained with the patient during the succeeding night, alternately maintaining the bougie in the uterus, supporting it gently, and rendering such other attention as the patient required. Continuing the pressure upon the fundus of the uterus was perhaps unnecessary, but it was thought safe not to withdraw the bougie until the next day.[1]

March 13, Dr. Robinson wrote: "The patient is quite comfortable; pulse 108. Free from pain. Withdrew the bougie this morning, and found the os uteri embracing it pretty firmly." Tonics with nutritious diet were continued, and quietude in the horizontal position enjoined. On the 15th, he wrote: "The patient is quite comfortable this morning. Made a digital examination, and found the uterus perfectly *in situ*, and mouth well contracted. Has some appetite. Pulse 100." March 23, he again wrote: "She is improving; has been much annoyed by neuralgia and sickness of stomach, but both are giving way, as is the leucorrhœal discharge. The treatment has been sustaining (porter, wine, quinine, iron, etc.), with nutritious diet." In conclusion, Dr. R. adds: "She will get well, and I feel gratified in the success of the effort of restoration, not only on account of the patient being relieved of a loathsome malady, but also that I have been instrumental in contributing to the professional reputation of one for whom I feel a deep friendship, as your success in this case will relieve obstetrical surgery from the opprobrium which has hitherto been attached to it in cases of chronic inversion of the uterus." In reply to a letter of inquiry from me, Dr. Robinson wrote, April 22: "I visited her yesterday, and found her very cheerful and able to sit at the table and take tea with us. Her final recovery is now beyond all doubt. In short, the operation has been as successful as its most sanguine friends could desire. Her convalescence has been protracted; slow, perhaps; but when we take into account the great prostration from the long continuance of the malady, and the exhausting hemorrhages and leucorrhœal discharges to which she had been subjected, the only wonder is that she has recovered at all." This patient, I am informed, nearly twenty years after reduction (September, 1876), is quite well, and able to attend to her household duties.

Case II.[2]—In July, 1872, Dr. George T. Hough, a very intelligent practitioner in New Bedford, Mass., wrote me to know "if I could be induced to come there and operate upon a case of chronic inversion of the uterus which had lately come into his hands." After long efforts to persuade the incredulous sufferer and her friends of the feasibility of making an effort for her relief, I was at length invited by him to visit her on the 15th of May, 1873. In passing through Boston on my way to New Bedford, I called upon Dr. George H. Bixby, the celebrated gynæcologist of that city, and invited him to accompany me. Dr. Bixby was present at the operation, rendered valuable assistance, and furnished the following report to the American Journal of Obstetrics, which I take the liberty of transcribing with many thanks, not only for this report of the case, but for his many courtesies.

Mrs. H., aged 35, native of New Bedford, first menstruated during her thirteenth year. She married at fourteen, and has given birth as follows: the

[1] In all subsequent cases the bougie has been withdrawn immediately after restoration, as there is little or no danger of spontaneous reinversion while the patient is kept quiet and in the horizontal position.

[2] Vide Transactions of the New York State Medical Society, 1874.

first, fourteen years after marriage; the second, eighteen months after the first. She was attended in her last confinement, more than six years ago, by an irregular practitioner. After the delivery of the child, the medical attendant left the patient for two hours, and on his return proceeded to remove the placenta in a very rough and hurried manner. For three weeks convalescence proceeded as usual. She was then taken with profuse hemorrhage. The latter continued in varying quantities and intervals for several weeks, when the family physician was called. Upon examination, he diagnosticated uterine polypus, and a surgeon was summoned to remove it. Accepting and confirming the opinion of the attending physician, the operation was immediately undertaken. The wire of the *écraseur* was with some difficulty applied about the pedicle, but upon making traction it parted. Upon close inspection, after the accident, it was ascertained that the case was one of uterine inversion, originally partial, but rendered complete by the previous manipulation. Nothing further was done at this time.

June, 1872, Dr. George T. Hough, of New Bedford, was called. He found the patient confined to her sofa, where she had been almost continuously since her last confinement. She was thin and chlorotic; menstruation had been regular as to time, but profuse. She suffered, in the intervals, from frequent attacks of hemorrhage, which were provoked by exertions as slight as walking across the room. Frequently slight hemorrhages through the entire interval of menstruation; at other times there was a complete absence of the same. A vaginal examination revealed the following: External genitals normal; within the vagina, a round, regular, conical body, more or less immovable, and constricted at its superior extremity. Upon the application of the usual diagnostic signs, Dr. Hough pronounced the case one of chronic inversion of the uterus. Not feeling quite sure of his diagnosis, Dr. Johnson, of New Bedford, was called in consultation. Dr. Hough's opinion was confirmed by Dr. J. In June, 1872, Dr. Hough consulted Prof. James P. White, of Buffalo, by letter, and subsequently Dr. White was called to see the case.

On the sixteenth of May, 1873, an examination was undertaken by Dr. White, in the presence and with the assistance of the following gentlemen: Dr. Mason, of Norwich, Ct., Professor of Physiology in the Buffalo Medical College, Drs. Hough, Johnson, Hooper, Vermeyne, and Hayes, of New Bedford, and Dr. Bixby, of Boston. Artificial teeth removed and urine drawn; etherization commenced by Prof. Mason. When completely anæsthetized, the patient was placed upon her back across the bed with her pelvis near one side, in a favorable position to allow the lower extremities to be flexed and rest in the laps of two seated assistants. By palpation the uterine tumor in the suprapubic region was found wanting. The excessive emaciation of the patient rendered this point easily demonstrable. By the vagina, the finger came in contact with a smooth, round, cone-shaped body, more or less movable, and constricted at its upper extremity. By the sound, a complete *cul-de-sac* was found encircling the entire constricted portion. By recto-vesical exploration, no interposing tumor was found, and the finger in the rectum was separated merely by the rectal and vesical walls from the sound in the bladder. The case having been clearly recognized as one of chronic inversion of the uterus, the operation of reduction was undertaken as follows: Prof. White assumed the kneeling posture before the patient, and between the two seated assistants. The left hand, well-lubricated, was carefully introduced into the vagina, and the fingers slowly insinuated under and about the mass. While the latter was sustained by the palm of the hand, the constricted portion was embraced by the thumb and finger from opposing sides. With the right hand, Dr. White's "egg-beater, uterine repositor," was introduced into the vagina, its concave extremity placed against the fundus or cone of the tumor, and its spring-end against the chest of the operator. This done, the right hand. now free, was employed for making counter-pressure above the pubes, or for any other desired purpose. Gentle but unyielding

pressure, rendered equable and manageable by the action of the spring, was continued without any variation for thirty minutes, little more force being exerted than that required to compress the spring. Thirty minutes after the commencement of the operation, patient in good condition; hemorrhage insignificant; tumor in vagina still firm; constriction unyielding. One hour from commencement of the operation, hemorrhage insignificant; tumor more soft; a tendency to collapse, manifested by retarded respiration and pulse, was combated by the injection of an ounce of clear brandy into the rectum, to which the heart's action promptly responded. The use of the "egg-beater repositor" was now continued, its action varied by a slow to-and-fro motion of the body upon the spring. At one and a half hours, the patient continued to bear the operation well; hemorrhage insignificant; pulse somewhat accelerated; tumor more soft and compressible; a large-sized, gum-elastic, rectal bougie was substituted for the "egg-beater." At two hours and ten minutes, under the pressure of the bougie and manipulation with the fingers, the constriction suddenly relaxed, and the organ assumed its normal position. The patient rallied from the anæsthesia in less than a quarter of an hour. Reaction took place slowly. Notwithstanding this prolonged manipulation, neither the vagina nor the perineum was in any way injured.

Dr. Hough thus writes in regard to the subsequent condition of the patient: The day after the operation there were symptoms of metritis, which rapidly disappeared under appropriate treatment. There were no bad symptoms following the operation, but two weeks after she suffered from a slight attack of pneumonia from exposure to a draft of cold air at night. One month after the operation: She has sat up, has a good appetite, sleeps well, and gains strength daily; vaginal examination reveals the uterus in a normal position. Some months afterwards, desiring to know the condition of the patient, I addressed a note of inquiry to Dr. Hough, to which the following reply was received:—

New Bedford, Jan. 16, 1874.

My Dear Dr. White:—At your request I visited Mrs. H. yesterday. She says, "Tell the Doctor that I am perfectly well in every respect," and she seems so. Upon inquiry, I find all the functions normal; menstruation perfectly regular, lasting two days; scanty, if anything. She has lost her chloro-anæmic look, and has a good color. Eats and sleeps well, and in fact is well. Very respectfully yours. Geo. T. Hough.

Case III.—In the winter of 1871–2, I received a letter from Dr. O. C. Strong, of Colden, informing me that there was in his neighborhood a case of inversion of more than twenty years' duration. The Doctor had been called in to prescribe for the patient during one of the attacks of flowing from which she had suffered ever since the birth of her child, now twenty years old. Not content with the assertion that she was "bleeding to death," and seeing her exsanguine and feeble condition, the Doctor resolved, if possible, to ascertain the cause of the hemorrhage, before undertaking to prescribe a remedy. Upon instituting a careful digital examination, he recognized what no one who had preceded him in the treatment had ever suspected—complete inversion of the uterus. He soon after wrote me on the subject, asking whether I would be willing to undertake its reduction, etc. Considerable time elapsed before the patient could be made to believe that there was any hope of her relief from a malady which had prostrated her for more than twenty years. At length, through the persuasion of her physician, Dr. Strong, she was induced to invite me to undertake its reduction. The case was reported by A. T. Livingston, M.D., assistant physician in the New York State Lunatic Asylum, in Utica, and from his notes I make the following extracts:—[1]

The history of the patient, prior to the time of the operation, has been obtained principally from a thesis presented to the faculty of the Buffalo Medical College, in February, 1871, by Dr. O. C. Strong, of Colden, N. Y. Mrs.

[1] Buffalo Medical Journal, August, 1872.

A. D., then aged twenty-four, and residing in Buffalo, gave birth to a female child, July 15, 1850. The midwife who attended her in her confinement, experienced some difficulty in the removal of the placenta, but finally succeeded in *pulling it away*. This act was followed by so much hemorrhage as nearly to destroy the woman's life. From this time until the day of the operation she suffered a continual loss of blood, which twice a month amounted to flooding. She consulted various physicians in relation to her condition, but received no benefit from any of them. This is not surprising when we are told that, until Dr. Strong was called to see her in 1870, no physician had ever proposed to make an examination per vaginam with the view of determining the cause of the hemorrhage.

March 2, 1870, Dr. Strong was called to attend Mrs. D. He found her lying upon a bed, about which were evidences in abundance of the terrible flooding which had just occurred. Upon making a digital examination, he discovered a tumor about the size and shape of a small pear, occupying the vagina, which, after a careful specular examination, he diagnosed as an inverted uterus. Dr. S. informed his patient that he considered that her condition permitted the use only of palliative measures, and advised the use of tonics and local astringents. Afterwards, learning of Dr. White's previous, successful operations for chronic inversion of the uterus, a correspondence arose between that gentleman and himself, with reference to the case of Mrs. D., which resulted in an invitation to Dr. White to visit the patient, and, if upon examination he should think it advisable, to attempt the reduction of her uterus.

June 23, 1872, Prof. White, with Profs. Julius F. Miner and M. G. Potter, Drs. Geo. N. Burwell, W. C. Phelps, and the writer, whom he kindly invited to accompany him, proceeded to the residence of the patient in the town of Colden, N. Y. She was found to be feeble and very anæmic, and slight hemorrhage from the tumor was then occurring. Prof. Potter, who was requested to take charge of the anæsthetic, administered some chloroform to the patient, and the tumor was then examined by several of the gentlemen present. It resembled in size and shape an ordinary hen's egg, and was suspended in the vagina by a long, narrow pedicle continuous with its upper and smaller extremity. This appearance, particularly the small and elongated cervix, led some who examined it to doubt its being a uterus at all, and to consider it rather a polypoid growth. But a probe could not be passed along the pedicle into the os, as might have been done had the tumor been a polypus, nor could the uterus be detected by palpation over the abdomen. A uterine sound passed into the bladder could be felt by the finger in the rectum above the tumor, and by the hand placed over the hypogastrium; also the finger, passed up the rectum, came in contact with the anterior abdominal wall as felt by the other hand. All these diagnostic means proved the absence of the uterus from the situation which it ordinarily occupies. By these negative proofs, Dr. White was entirely convinced that the tumor was the inverted uterus, and he therefore proceeded to attempt the reposition of the same in the presence of the above-named gentlemen, and of Drs. Strong, of Colden, G. H. Lapham, of Aurora, and Davis, of Boston. Dr. Potter had produced anæsthesia by chloroform, which he now exchanged for ether, with which he kept the patient anæsthetized during the operation. Dr. White assumed a kneeling posture in front and between the legs of the patient, who had been placed upon the side of the bed with her hips projecting a little beyond its edge, and her feet resting in the laps of Drs. Miner and Phelps, who sat on either side of Dr. White, each supporting a knee and holding a hand of the patient. Dr. White then introduced his right hand into the vagina, and began manipulating the tumor. This manipulation consisted in compressing the uterus, which relieved its congestion and rendered it more pliable, and in making gentle pressure in the line of the axis of the pelvis by the use of the uterine repositor. After continuing this a short time the doctor brought the tumor down to view, when a glance sufficed to assure the doubting of its true nature. By the pressure which had been exerted the neck had been

shortened and dilated, the body and fundus reduced in size, the superior angles (now inferior) were distinctly seen, and altogether the tumor then presented the normal outline of a uterus of small size. The repositor was again introduced, and a pressure of eight or ten pounds exerted by it, and, at the same time, compression was made by the hand within the vagina upon the portion of the uterus protruding beyond the os, the same hand also retaining the fundus uteri and the repositor in coaptation, whilst the left hand was employed in pressing above the pubes or through the rectum. When, in this manner, the cervix had been made to embrace the fundus, the uterine repositor was substituted by a large rectal bougie, with which pressure was continued until the close of the operation.

At the end of an hour and fourteen minutes, Dr. White was obliged to discontinue his efforts on account of the benumbed condition of his intra-vaginal hand, caused by the pressure upon it of the narrow and unyielding vagina. The fundus was at this time within the cervix and above the os, and the doctor considered the reduction substantially accomplished. He requested Dr. Miner to continue the manipulation, which he did, and in sixteen minutes, or just one hour and a half from the beginning of the operation, Dr. Miner enjoyed the satisfaction of announcing that the uterus, which for twenty-two years had been completely inverted, was now as completely reposited. Two hours after the operation, when we left her, the patient was quite as comfortable as could have been anticipated. An opiate was administered, and directions were given the attendants to keep her perfectly quiet for at least a fortnight. By letter dated June 27, Dr. Strong reports: "Mrs. D. doing better than we had dared to expect." July 2, Dr. Strong writes: "Our patient is doing well, and nothing has yet interfered with her progress toward recovery." July 19. "Since the operation upon Mrs. D., nothing has transpired to mar the beauty of the result. The pulse has at no time exceeded ninety per minute." At this time a careful examination "shows the uterus occupying its normal position, the intra-vaginal portion of the neck presenting the ordinary appearances of mild cervicitis. The patient is now about upon her feet some portion of her time, and everything points to a rapid, and I think, perfect cure. She has had no sanguineous discharge since the operation."

On the 23d of August, two months after the operation, the following letter was received :—

My Dear Doctor :—Allow me to congratulate you on the perfect success of your operation upon Mrs. D. I have delayed writing thus long that I might not be required to retract any statements made in regard to the success of the operation. The most sanguine could not have expected so good a result. She now does her own work, goes on foot to the neighbors', rides ten miles and returns over the hills, and in fine now seems competent to perform her part in life. At the expiration of four weeks from the operation, she had a regular menstrual discharge, which seemed perfectly normal both in regard to amount and character. Your obedient servant,

O. C. Strong.

In order to ascertain her subsequent condition, I addressed a note of inquiry to the doctor, and received the following reply :—

Colden, Jan. 20, 1874.

My Dear Doctor :—Your note of inquiry is received. In reply permit me to say that Mrs. D. is in the enjoyment of good health; that her menses are regular; that in her present appearance one can see nothing to tell the tale of those twenty-two long years of suffering; nothing that speaks of terrible floodings; and, in fine, nothing to show that she ever possessed an inverted uterus. Very truly yours,

O. C. Strong.

It would seem an unnecessary expenditure of the time of this learned body to detail the remaining nine cases. Those related fairly illustrate the state of the organ at the different periods of the accident, and the mode of operating for its restoration. The result has been, in all the cases encountered, restoration by manipulation on the first trial, and as is believed without serious injury to the tissues, thus confirming the conviction that all cases are curable, irrespective of their duration.

CASE OF RETROVERSION OF THE GRAVID UTERUS; TREATMENT BY PUNCTURE; RECOVERY.

BY

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On December 17, 1874, three children, the eldest ten years of age, were attacked with a severe form of scarlet fever. Their mother, with characteristic maternal devotion, was almost their only nurse, although at the time of their seizure she had commenced the second month of her fifth pregnancy. She was on her feet a great deal, and very frequently lifted and bathed the children, although herself of slight and delicate frame. To this she ascribed the unusual pressure and bearing down experienced in the recto-vaginal region, such as she had never felt in her previous pregnancies.

A little after midnight on March 17, 1875, her husband informed me that his wife feared a miscarriage, as she was in great pain, and flowing profusely. Visiting her immediately, and making a digital examination, I found the vagina filled with an elastic, globular mass, as large as the fœtal head at full term. Nothing like an os uteri could be found. The sub-pubic space was tightly filled, as was also the perineal; it was impossible, by rectal exploration, to pass the finger above the lower segment of the tumor. The hemorrhage was free, and was found to emanate entirely from the urethra. A catheter was introduced with considerable difficulty, the instrument taking a backward and downward direction, and serving for the evacuation of about six ounces of very bloody urine. So impacted was the vagina, and so strong was the pressure upon the rectum, that it was impossible to make the vesico-rectal exploration by the catheter and finger.

The case was presumptively one of retroversion, or rather of complete eversion of the pregnant uterus, at or exceeding four months. The condition was so grave that the attendance of my colleague, Prof. James P. White, was obtained in consultation as soon as possible. He made a most critical examination, and was, like myself, unable to find the os uteri. We concurred in the opinion that it was either complete eversion or an immense hæmatocele, and that, in the former event, any attempt at restoration was out of the question on account of the strong pressure and great size of the tumor, and that the attempt would probably be followed by metritis or rupture, or both.

It was resolved to puncture the mass with a small exploring trocar, and the tumor was accordingly pierced at its most depending portion, when about four ounces of clear, straw-colored fluid passed through the canula. There was no blood, the fluid being evidently liquor amnii. Half a grain of morphia was administered, and the patient was left to obtain a few hours of much needed rest. At 5 P. M. the patient was very comfortable. She was now placed in the knee-face position, and an endeavor was made to lift the fundus; but this could not be done. The

bloody urine was still passed, but not as frequently or as painfully as before. The morphia was repeated.

On the morning of the 19th, slight labor-pains set in, and increased in force and frequency very gradually. With each pain the mass sensibly contracted, and the everted fundus was lifted a little toward the rectum. Shortly after midnight, on the 19th, the pains became forcing, and, at 3 A.M., a flattened sphere, the fœtal head, emerged under the pubic arch. At 4 A. M. a male fœtus, nine inches long, was expelled, and very soon after was followed by the placenta and membranes. The intra-vaginal tumor was now diminished about one-half in size, but still retained its completely capsized position, and it was still impossible to feel the os uteri. It was not thought proper to make any immediate effort towards restoration. At 5 P. M. eight ounces of urine, slightly tinged with blood, were removed by the catheter, and during the two following days the urine was drawn, morning and evening, constantly improving in appearance.

On the 22d, copious alvine evacuations followed a soap-suds enema, and on the evening of the same day the os uteri was distinctly felt under the pubic arch, and the fundus had receded to the sacral hollow. From this time forward the catheter was not used. On the 22d, there was an abundant lacteal secretion, which disappeared spontaneously in about ten days. The patient was instructed to lie upon her face and sides, and to avoid straining at stool. By April 5, she was apparently perfectly well, the lochia had ceased, and there was no vesical or rectal irritation. The uterus became gradually reduced in size, but still retained its retroverted position, and it was still thought safest to defer attempts to replace it. The patient was in fact too well to be disturbed, and had manifested no serious symptoms, except local ones, during the severe ordeal through which she had passed. She was always bright and hopeful, suffered little pain, and had at no time any febrile movement; her pulse was only accelerated to 100 for the two days preceding the expulsion of the fœtus. She was instructed to use warm vaginal and rectal enemata daily, to maintain the recumbent or semi-recumbent position, and to await the proper time for interference, which she very patiently did.

On May 1, her menstrual period came on, and lasted five days. On May 13, Simpson's sound was passed in, to the depth of three inches, and the uterus was by its aid placed in the proper position, from which, however, it fell back within a day or two. This operation was repeated, once a week, until the second period returned, which was on May 31. On June 10, the uterus was placed in position; on the 19th, it had receded again, was restored with the sound, and was now retained in position with a cradle-shaped, hard-rubber pessary. This was worn with entire comfort for six months, and was then removed. The uterus has since retained its proper position. The patient is apparently perfectly well, but has not been again pregnant.[1]

Remarks.—This case has been presented, certainly not for its novelty, but to illustrate the successful result of a method of treatment. When

[1] June 11, 1877. The patient is again three months advanced in pregnancy. She consulted me at the end of the second month, when the uterus was again found retroverted. It was replaced by the repositor, but it was found necessary to retain it *in situ* by a pessary, and the three-winged instrument of Thomas was employed successfully, after failure with the cradle-shaped one previously worn. At the present date, gestation is progressing favorably.

we consider the number of fatal cases on record, and when we further consider the serious injury that has frequently resulted to both bladder and womb, either from entire neglect or from repeated and protracted attempts at restoration, and when we remember that in the greater proportion of cases of retroversion of the gravid uterus, even if the organ is replaced by posture and manipulation, the operation is almost always followed by abortion and metritis, may not the procedure which has been described be more safe than the treatment usually adopted? I do not wish to be understood as advocating puncture or tapping of the womb, through its walls, in preference to separation or rupture of the membranes through the os uteri, when this is accessible; but only when, as in the case reported, the os cannot be reached.

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ON THE COMPARATIVE VALUE OF CAUSTICS AND ASTRINGENTS IN THE TREATMENT OF DISEASES OF THE CONJUNCTIVA, AND ON THE BEST MODE OF APPLYING THESE REMEDIES.

BY
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The conjunctiva, one of the least in extent, but one of the most important, of the mucous membranes, is the seat of affections singularly various in form and degree—from the simplest transient congestion to the most violent inflammatory processes and most persistent structural alterations. On the treatment of these depends, often, the preservation or the loss of the most important of our special senses, and the fairest prospects in life may be blighted by either mismanagement or neglect.

In preparing the paper which I now offer to the Section on Ophthalmology, I have avoided a recapitulation of every theory as to pathology and treatment, and offer simply a few practical suggestions, with no wish to unduly depreciate the value of other means and methods than those for which I express a preference. Skilful men will know how to use various instrumentalities to attain good results. Nevertheless, it is desirable, and important, to discriminate among the agencies at our command, and to select such as practically prove best adapted to the cure, *tuto, cito, et jucunde*, of the diseases which we are considering.

A considerable number of conjunctival affections require little or no treatment, and are injured rather than benefited by the use of other than the mildest remedies. The hyperæmia caused by the presence of a foreign body subsides upon its removal; the injection depending on astigmatism, or other defects of refraction, disappears after the selection of suitable eye-glasses; the sub-conjunctival ecchymosis following a blow, or the rupture of a small bloodvessel, is re-absorbed, perhaps quite as rapidly, without our interference. If, in these cases, we prescribe a lotion

or a collyrium, it should be quite unirritating. So also in traumatic injuries of a simple character, and after operations for strabismus or pterygium, the healing processes go on without assistance from applied remedies. Chemical injuries, because of their graver nature and the degree in which the cornea may be involved, oftener require our aid; but even here, emollients and sedatives rather than active stimulants are most useful.

With the above-named exceptions, and apart from certain complications hereafter to be considered, diseases of the conjunctiva may be divided into two classes requiring essentially different management. The first includes a comparatively simple, limited, and benign, though sometimes chronic, affection; the second embraces several conditions of general and often severe conjunctival inflammation. The prominent feature of the first class is the development of small elevations or papulæ, one or several in number, usually situated at or near the border of the cornea, though they may form elsewhere on the ocular conjunctiva. The summits of these may be whitish, as if containing a minute quantity of pus; more frequently they are red; and they may vary in size, but are commonly about that of a hemp-seed. Usually a fasciculus of vessels runs to each of these papulæ; but there is no general injection of the conjunctiva, except as far as may be the result of friction of the papulæ against the inner surface of the lid. Children are the most frequent subjects of this disease.

The tendency of this affection is towards a disappearance of the symptoms at the end of a few days, even when no treatment is employed. Therefore, no active remedies of caustic or astringent nature are needed, and they tend to delay rather than hasten the recovery. Calomel or other insufflations are similarly useless. All annoying remedies are moreover objectionable on account of the age of the majority of the patients. Simple lotions, cold or tepid, with water, or milk and water, may be used. Two or three times a day, a few drops of a solution of ten grains of borax in an ounce of water, or of camphor water, may be dropped into the eye from a teaspoon or a drop-tube. This usually proves a soothing and refreshing collyrium, grateful rather than painful to the patient, and appears to have a sufficiently astringent and stimulating effect to promote absorption of the papulæ. Although the ordinary duration of these small pimples rarely exceeds ten days, it is not uncommon, whatever treatment has been employed, to find successive crops of them, at intervals of a week or two. This disposition seems to be best obviated by continuing the use of the borax collyrium, once or twice a day, for some time after recovery, so as to modify the condition of the conjunctiva. Where the papulæ are slow in disappearing, the application of a crayon of alum, once in a day or two, to the inner surface of the lower lid, is often useful and is nearly painless. The astringent reaches the affected spot as the lid returns to its place, and seems sometimes to excite a more rapid resolution. The papulæ themselves should never be directly touched with caustic or astringent crayons or solutions.

Very rarely, the disease takes a more chronic form, the papulæ being as large or larger than a split pea, and sometimes umbilicated at the centre. Though persisting longer, and for a time intractable to treatment, the prognosis is always favorable. Tonic general remedies, and sometimes change of air, are useful adjuvants to the mild local applications. Solutions of atropia have of late been too much in vogue in the treatment of this form of disease. Though far less dangerous than the strong caustic remedies which were formerly indiscriminately employed, yet an

unnecessary resort to them has grave disadvantages. In cases of corneal ulceration they may be of much service, in lessening the irritability and photophobia; whilst in these less serious conjunctival affections their action upon the pupil does but increase the intolerance of light, and, by exciting strong contractions of the lids, creates a disposition to corneal ulceration as an effect of friction.

The second class of conjunctival affections comprises catarrhal conjunctivitis in its acute and chronic forms, trachoma, purulent and gonorrhœal conjunctivitis, ophthalmia of new-born children, and diphtheritic conjunctivitis; each of which conditions has an importance claiming distinct consideration.

When the conjunctiva becomes affected from exposure to cold, dust, or other simple causes of irritation, the first symptom is injection with an increased flow of tears—soon followed by a velvety thickening of the palpebral conjunctiva and a slight mucous secretion. These phenomena may occur so suddenly that the patient feels sure that something has been blown into his eye; but the scarlet, velvety appearance of the lining of the lids, as distinguished from the mere vascular hyperæmia excited by the presence of a foreign body, renders the differential diagnosis sufficiently easy. At this stage, caustics and strong astringents are alike to be avoided, or used with great reserve. Frequent bathing with cold or tepid water according to the sensations of the patient, with applications, four or five times a day, of a few drops of a solution of ten grains of borax in an ounce of camphor-water, or of five grains of alum or one grain of sulphate of zinc in an ounce of water or rose-water, will generally cut short even a sharp attack and remove the symptoms within three or four days. Occasionally this form of conjunctivitis is epidemic, large numbers of persons being attacked within a short time. Similar, with perhaps somewhat more active, means, are here called for; one or two touches inside the upper lid with a crayon of alum, or, very rarely, a single light touch with a crayon of sulphate of copper, acting as useful auxiliaries in arresting the symptoms in an early stage. If neglected or if too actively treated, the thickened conjunctiva assumes a more or less granulated aspect, and a chronic phase of disease is established which requires more active measures. The lining of the lids, especially the upper, from hypertrophy of the conjunctival follicles, now resembles in appearance the surface of a raspberry, and the sub-conjunctival tissues become so infiltrated that the lid-movements are difficult. The secretions are greatly augmented, causing abundant flow in the daytime, and agglutination of the lids at night. Here, as in another affection next to be described, the mild remedies, sufficient for the lesser degrees of inflammation, are of little avail. Caustics or astringents, of considerable potency, seem to be our only resource.

Trachoma differs widely, pathologically, from catarrhal conjunctivitis, though often coexistent with and complicating it. It consists essentially in a neo-plastic formation; at first in small, oval-shaped, semi-transparent masses, resembling grains of tapioca; and sometimes increasing until the conjunctiva, with its subjacent textures, is largely transformed into a dense, nearly structureless mass, which, as recovery takes place, is not replaced by normal tissue, but is followed by contraction of the conjunctiva and incurvation of the tarsal cartilage. Here, the prognosis is less favorable, the recovery slower, and the danger of secondary lesions of the cornea much greater than in the catarrhal form of conjunctivitis.

But the local treatment of the two affections has much similarity, and may be either caustic or astringent, or both.

Nitrate of silver is the caustic most relied on in the treatment of conjunctival affections, though other means have from time to time been tested and enjoyed ephemeral reputation. The methods of using it, and the means employed to limit its effects, vary with different ophthalmologists. The crayon of pure nitrate and the nearly saturated solution, formerly much used, are now rarely employed, but have given place to the lapis mitigatus, as it is termed, in which one part of nitrate of silver and two parts of nitrate of potassium—or two parts of silver and one of potassium—are fused together; or to solutions of nitrate of silver of from ten to forty or more grains to an ounce of water. The caustic is applied, in most cases, to the surface of the everted, upper lid, more or less lightly or extensively, the stick of lapis mitigatus being preferred by some, the solution of nitrate of silver by other authorities. When thought advisable, the action of these substances is limited by the immediate application of a neutralizing solution of salt and water, or pure water, or both of these, to the cauterized surface. The frequency of the repetition also varies greatly, in the practice of different individuals: once, twice, or thrice a week, daily, or oftener, according to the intensity of the symptoms or the preferences of the practitioner. Moreover, some rely wholly on this means; others employ auxiliary treatment. Theories as to therapeutic effect also vary with different observers, some seeking to destroy the apices of the granulations, while others, deprecating such an effect, use the caustic with more reserve, and, virtually, as a strong stimulant only.

In the class of astringents employed as topical applications by the physician himself, may be named, crayons of sulphate of copper; the lapis divinus, composed of equal parts of sulphate of copper, alum, and nitrate of potassium moulded into sticks; crayons of alum; and strong solutions of tannin in glycerine. Of these, the crayon of sulphate of copper forms the best type; as combining the several qualities of convenience, efficiency, and safety. The alum crayon has an excellent effect in mild forms of conjunctivitis, but is inefficient in the severer and chronic affections. The lapis divinus seems less reliable than the crayon of pure sulphate of copper. Tannin and other vegetable astringents, from which theoretically we should expect so much, have always proved, in my hands, in whatever proportion or combination, inferior to the mineral astringents; whether as a means of energetic action upon the conjunctiva, or as used in collyria to produce milder continuous effects.

The sulphate of copper cannot be fused in moulds, like nitrate of silver and lapis divinus, but the crayons have to be shaped from crystals of the substance. Unfortunately, these are generally much broken at the laboratories in packing for the market, but if good crystals can be found it is easy to cut from them crayons of convenient size. The harder parts of the crystal, free from pores or water of crystallization, are to be chosen. Nothing would be needed to obtain ample supplies of the crystals, or of crayons already shaped from them, but the knowledge that there was a demand for them, as they are easily made from crystals obtainable at the laboratories, and they require no care in keeping, being little changed by exposure to air or light. A single crayon lasts a long time, and may be used, like a silver probe or a bistoury, for successive cases, care being of course taken to wipe the crayon after every application. The crayon of sulphate of copper should be applied, as a rule, rather lightly to the

conjunctiva of the everted upper lid; this lid, in which the circulation is more readily congested, being usually more diseased than the lower lid. It is rarely necessary to touch both lids, as a portion of the remedy, dissolved by the moist surface over which it is passed, is carried by the movements of the lids to every part of the conjunctiva. In sluggish cases the crayon may be applied more heavily, or may be more frequently repeated.

Used as above described, the crayon of sulphate of copper does not act on the conjunctiva as a caustic, but only as an energetic astringent and stimulant. In the more acute cases, the pain, after the first or even after several applications, may continue for some time; though it is much less severe and enduring than when caustic has been used. Bathing the eye with water shortens and mitigates the smarting. While the pain is much prolonged, the crayon should be used lightly and perhaps less frequently than after the eye has become more tolerant of the remedy. The touch may then be harder, and may be repeated daily or less often, according to the judgment of the physician. When the crayon is well borne, cases often improve the more rapidly according to the greater frequency of its application; this being especially observed in many chronic cases, trachomatous, and other.

That many cases of muco-purulent or trachomatous forms of conjunctivitis may be successfully treated with nitrate of silver, pure or combined, does not admit of a doubt. Its action requires to be carefully watched, and good judgment must be exercised in determining the frequency and severity of its application, and in neutralizing at once any excess of the caustic. More benefit is obtained from its moderate than from its too free use. Care must also be taken not to continue it too long, as not infrequent instances have occurred in which the conjunctiva has acquired an indelible, olive stain, or even become quite black, from the prolonged instillation of a moderately strong solution. If employed, caustics should not be solely relied on, but should be supplemented by other milder remedies of stimulating or astringent nature, which may be more or less frequently applied by the patient or his friends.

The use of caustic as an application to the eye is not without its dangers. Its active properties render it a destructive agent in unskilful hands; and it sometimes disappoints even the most experienced oculist. The application of the crayon of sulphate of copper involves no such consequences as are inseparable from the use of nitrate of silver. If it happens to be injudiciously employed, no special harm is done and no immediately destructive effects ensue from its use. It is, therefore, a far safer agent in inexperienced hands.

It remains to consider whether the general results of treatment will be more successful under the one or the other plan.

My own experience, which has included careful personal observation of the practice of nearly every European oculist who has had celebrity within the last thirty years, has satisfied me that better and quicker results, with far less danger and suffering, are obtainable from the use of astringents than from the use of caustics. Many years since, I made comparative tests of the value of these two classes of remedies in severe and chronic forms of conjunctivitis, on a large number of patients. Selecting cases where both eyes were equally diseased, I treated all the right eyes with nitrate of silver, and all the left eyes with sulphate of copper. Of course, no differences of constitution could influence the effect produced, as might, perhaps, have been alleged had merely a certain number of patients

been chosen for each mode of treatment and the same remedy used in both their eyes. The uniform result was a more rapid gain in the eyes treated with the crayon of sulphate of copper. Some of the eyes treated with nitrate of silver went on tolerably well, though slowly; but others did so badly that, after vain attempts to succeed by varying the strength of the caustic applications, I was at last compelled, in order to avert threatened loss of the eyes, to substitute the use of the sulphate of copper, under which they recovered. In no instance was it necessary to abandon the sulphate of copper and resort to another remedy.

A very important adjuvant to treatment, either by caustics or astringents, as applied by the physician himself, consists in combining with these the more or less frequent use, at home, of a milder astringent or stimulating collyrium. Of these, the mineral seem to be superior to the vegetable astringents, as also to the purely stimulating class of collyria, such, for instance, as those containing wine of opium or corrosive sublimate. Many substances have been employed with more or less advantage. Among those most in use may be mentioned solutions of nitrate of silver, sulphate of zinc, acetate of zinc, sulphate of copper, sulphate of cadmium, alum, and borax. Of these, the sulphate of zinc seems by far the best for the forms of conjunctivitis which we are considering, as it appears to have a more astringent with less irritant action than either of the other remedies. The silver, copper, and cadmium solutions, are too actively stimulating, with less of astringent effect; the alum and borax are too mild. The sulphate of zinc solutions may vary from half a grain to four grains to the ounce of water or rose-water; and a few drops may be poured into the eye from a teaspoon, a drop tube, or otherwise, two, three, or four times a day. In cold weather the spoon may first be dipped into hot water for a moment, so as to warm the collyrium, if the eye is sensitive as to the temperature of the drops. I have found the sensations of the patient a useful guide in determining the strength of collyrium likely to be of most benefit to him. If a solution of a given strength, say of two or of three grains to the ounce, causes smarting for more than five minutes, it should be diluted one-half, or until it no longer gives more than a few minutes' pain. Thus graduated, the collyrium should be used often enough to keep up a moderate remedial influence.

Acetate of lead, in substance or solution, formerly much in favor even with the profession, and still in common popular use, should *never* be put into the eye. It has no special remedial value to warrant its employment; and whenever the conjunctivitis is, or becomes, complicated with corneal ulceration, great harm is done; the acetate being decomposed, and an indelible deposit formed on the ulcerated surface, causing permanent opacity of the cornea. It is therefore better to erase it from the list of ophthalmic remedies.

More actively virulent forms of conjunctivitis are found under the designations of purulent conjunctivitis, gonorrhœal conjunctivitis, and the conjunctivitis of new-born children. Purulent conjunctivitis, sometimes termed Egyptian Ophthalmia or Ophthalmia of Armies, is marked by far more rapid sequence and violence of symptoms than the catarrhal or trachomatous affections. In pauper asylums, schools, and barracks, it may speedily extend to a large number of persons by direct infection of healthy eyes with the purulent discharge from those which are diseased. This is largely conveyed through the promiscuous use, in such institutions, of articles of toilet, wash-basins, towels, etc.; and is some-

times purposely introduced; the fearful risk of blindness being incurred for the sake of avoiding military or other duty, or of obtaining some hospital privilege. Enormous tumefaction of the conjunctiva and of the entire lid, and copious muco-purulent discharge, characterize this disease. Serous or even phlegmonous chemosis of the subconjunctival cellular tissue often forms a dangerous complication, interfering with the proper nutrition of the cornea; and ulceration of that structure may result from the direct pressure of the swollen lid, or from the continued maceration in the copious purulent secretion.

Gonorrhœal conjunctivitis is even more intense in its symptoms and rapid in its course. Caused by infection with gonorrhœal matter or with the discharge from an already diseased eye, it is more common in men than in females, the fingers being often the medium for conveying the urethral discharge to the eye. One eye only is usually affected at the outset. The frequency of phlegmonous chemosis, and consequent danger to the cornea, is greater than in the simple purulent affection. So rapid is the course of the disease, that an eye may be lost by sloughing of the cornea within twenty-four hours from the moment of infection.

Active cauterization of these forms of conjunctivitis, especially the gonorrhœal, was formerly practised as an abortive treatment, in the hope of substituting a less dangerous traumatic inflammation for the specific infection. This idea has been generally abandoned, and the modern treatment is in all respects milder. As general means, tonics, instead of copious depletion, are in favor; and, as regards local treatment, great importance is to be attached to the frequent use of mild detergents, injected beneath the swollen lids, to remove the abundant purulent secretion. If caustics are used, the applications should be neither too strong nor too frequent, and should be at once neutralized—as otherwise they tend to increase the phlegmonous chemosis and perhaps destroy the cornea. They should not be continued if corneal ulceration has begun. If the crayon of sulphate of copper is applied at all during the active stages of the disease, it should be lightly used; but it is most serviceable in removing the granulations which remain after the active symptoms are abated. The application of cold compresses, constantly renewed, to the lids; frequent removal of the fast accumulating secretions; and the keeping up an influence on the diseased conjunctiva by means of mild astringent collyria, appear to form the most reliable treatment. Solutions of five grains of alum or ten grains of borax to an ounce of water may be injected several times a day, and a solution of half a grain to two grains of sulphate of zinc may be used three or four times in twenty-four hours. The application of a crayon of alum to the inside of the lid, once or twice a day, is sometimes of great apparent benefit, and is safely and easily applied as an auxiliary to the astringent solutions. It is nearly painless, and has little stimulating action as compared with its astringent qualities. It should be used by introducing it beneath the lid without everting it. If the case is seen early, these means are generally effectual; but unfortunately the vitality of the cornea is in many instances already destroyed when the patient first applies for advice, and the prognosis is then unfavorable.

No disease of the eye imposes greater responsibilities upon the general practitioner than ophthalmia neonatorum, the conjunctivitis of new-born children. Occurring within a few days after birth, under circumstances which, in a large proportion of cases, preclude a consultation with any practitioner of special experience, the preservation or loss of sight must

depend on the skill of the family physician. A slight conjunctival injection and tumefaction, with some mucous discharge, is not uncommon in infants, and yields to simple means of cleanliness. But true ophthalmia neonatorum, preceded perhaps only by a slight reddish hue along the skin of the upper lid, becomes within a few hours an intense inflammation. The lids are tumefied, livid, sometimes projecting much beyond the supra-orbital ridge, or even completely everted from the enormous swelling of the conjunctiva. The chemotic infiltration of the cellular tissue beneath the ocular conjunctiva often causes this to overlap the cornea or even to hide it. The secretion is exceedingly abundant. This disease is one of the most destructive, but happily also one of the most manageable, of the affections of the eye. If neglected, or if treated with inert domestic remedies such as injection of breast milk, or with more harmful means such as the application of poultices, the result is too frequently the loss of one or both eyes. Equally baneful consequences follow the use of harsh measures. The great danger attending the use of caustics, in any form, or of very strong astringents, and the *needlessness of resorting to them*, cannot be too strongly insisted on. The symptoms invariably yield to a mild treatment, if reasonably begun and faithfully carried out. If, as is sometimes the case, the cornea has already become more or less cloudy before the attention of the physician is called to the eyes, this furnishes an additional reason for refraining from strong caustic or astringent remedies, which are never well borne when the cornea has become involved.

The first indication is to insure the utmost attention to cleanliness. The copious, muco-purulent secretion should be washed from the eyes every two hours, every hour, or even every half hour if necessary. Should the great tumefaction of the lids prevent their being readily opened so as to permit free discharge of the pus, the nozzle of a small syringe should be passed beneath the swollen upper lid, and an injection thrown in to completely wash out the conjunctival cavity. For this purpose tepid water may be used in the milder cases; in those of more severity a solution of alum, five grains to the ounce, may be alternated with the injections of water. These injections should be repeated hourly or oftener, according to the severity of the case, during the day, and two or three times during the night. They are nearly painless, and the nurse or mother should be taught how to make them. In a few days the discharge lessens and the swelling of the lids diminishes; but the treatment must be kept up, though with less frequency. The physician should not fail to assure himself, by daily inspection, as to whether the cornea retains its normal appearances; and where the swollen condition of the lid prevents a view of the cornea, an elevator should be carefully used to draw up the lid. If the cornea is seen to be even slightly cloudy at its centre, a drop of a solution of sulphate of atropia, two grains to an ounce of water, should be put into the eye twice daily, to lessen the danger of hernia of the iris in case corneal ulceration and perforation should occur. This haziness and ulceration may come on very early in the disease, as a result of the continuous pressure of the lid upon the cornea, or as the effect of its constant immersion in the purulent secretion; but this threatening condition may also supervene at a later stage, and after marked amelioration of the active symptoms; so that the physician, unless on his guard, may lose the battle in the moment of apparent victory.

The use of the nitrate of silver or of lapis mitigatus, is still recom-

mended by some of the continental European oculists; but the milder plan above described has been, for many years, almost exclusively in use at the ophthalmic hospitals of London, and especially at the Moorfields Royal London Ophthalmic Hospital, probably the largest institution in the world for the treatment of eye-diseases, and certainly the most distinguished for the celebrity of its medical staff. In my own experience, success has always attended the mild plan of treatment; and I have seen so many examples of the destructive consequences of harsher methods, that I cannot regard them otherwise than as unnecessary and injudicious.

Two classes of complications of conjunctivitis are worthy of our attention, in their relations to the questions of remedial action which we are considering. In one of these classes the conjunctivitis results from other morbid processes; in the other class the morbid conditions are the consequence of pre-existing conjunctivitis. In the first class, including cases where conjunctivitis appears as a symptom in keratitis, iritis, or in other inflammations of the interior structures of the eye, the conjunctival injection is not to be regarded as a disease, and its treatment, if any, must be subordinate to or modified by that of its primary exciting cause, the removal of which often suffices for the cure of the secondary conjunctival symptoms. In most of these conditions the use of caustics and astringents is contra-indicated. Instead of relieving, they excite yet further congestion of the external membrane, and increase rather than lessen the deeper seated inflammation. Tepid, unirritating lotions, and collyria containing a few grains of borax, may be used as sedatives, if the patient finds them agreeable. Active use of either caustics or astringents is also to be avoided in conjunctivitis accompanying corneal ulceration, or the presence of papules on the epithelial layer of the cornea, in children; as also, especially, in ulcerations of the cornea in adults, occasioned by debility, in old age, or as a sequela of smallpox or other depressing disease.

The second class of complications embraces those conditions resulting from conjunctivitis, usually from its chronic duration, as seen for instance in pannus and secondary keratitis. Here, the attention must primarily be directed to the removal of the original disease, by such remedies as have been already indicated; but we must be careful to make such modifications of treatment as may be required by the secondary symptoms or structural changes, while we avoid the error of regarding these as the essential morbid conditions and directing our treatment solely to them. For example, in the affection styled pannus, where the epithelial layer of the cornea has become so thickened and vascular as to resemble a piece of red flannel, it is useless to attempt to remove this vascularity by division or scarification of the vessels. The abnormal condition of the external layer of the cornea is the result of constant friction by trachomatous or other granulation of the lids, and is to be cured through the removal of these by the patient use of astringents. After the friction has ceased to irritate the corneal surface, the injection and cloudiness gradually fade away. If ulcerative or exudative processes have begun in the cornea itself, caustics are not admissible, but the use of atropia should be combined with the astringent remedies, the continuance of which in moderation is necessary for the removal of the granulations. A certain degree of vascularity is essential to the absorption of the cloudy transformations, and disappears as these fade away.

Some other more exceptional complications should be briefly referred

to. When chronic conjunctivitis accompanies obstruction of the lachrymal passages, mild astringent collyria may be combined with the appropriate means for the removal of this obstruction. When it is coincident with disease of the Meibomian glands or ciliary bulbs, these collyria may be used in connection with diluted citrine ointment, or other suitable, topical applications, along the edges of the lids. When it appears during an attack of herpes zoster ophthalmicus, or ensues after paralysis of the orbicularis, caustics are scrupulously to be avoided as being likely to promote ulceration of the cornea, to which, in these circumstances, there is always a predisposition; only the mildest astringents should be used. In chronic thickening of the conjunctiva and eversion of the lids in old people, commonly called "blear eyes," attention should be given to the lachrymal passages, and the everted lid should be brought to its place by a frequent but mild use of astringents, patiently followed up. These should not be applied, nor should caustics be used, upon the everted and dry portion of the conjunctiva, as they only serve to harden, and, as it were, tan the surface; but the crayon of sulphate of copper or of alum should be applied, very lightly, to the non-everted portion of the conjunctiva. As this part becomes more healthy, the everted lid is drawn more and more to its proper place, the remedy can be applied more largely, and the improvement finally extends to the whole of the affected tissue.

The conclusions which seem to be warranted by the facts presented in the foregoing paper, may be summed up as follows:—

I. In a considerable number of essentially transient affections of the conjunctiva, and in pterygium or other growths, no active treatment by caustics or astringents is required.

II. When disease affects only a limited portion of the conjunctiva, as in phlyctenular inflammation, the mildest stimulating or astringent remedies are usually sufficient.

III. In the acute and chronic forms of general conjunctivitis, astringents are, as a rule, safer as well as more efficacious than caustics, and are, therefore, better adapted to the requirements of the general practitioner.

Discussion on Dr. H. W. Williams's Paper.

After the reading of the preceding paper, Dr. C. R. Agnew, of New York, said:—I would like Professor Williams to say with more minuteness what he would do in a case of ophthalmia neonatorum in which the swelling was so great as to produce complete eversion. Suppose he were called to see a child ten days old, in whom the inflammation was such as to have caused complete eversion of both eyelids; let him tell us what applications he would use, and how, and how frequently, he would use them. I agree mostly with Prof. Williams's views; I recognize the force of his arguments in favor of mild treatment, and the propriety of his protest against the too heroic measures often employed; but I want him to go further and tell us what he would do in such a case as I have described.

Dr. H. W. Williams said:—I can hardly imagine a case more unpleasant to meet with than such an one as that supposed by Dr. Agnew; nevertheless, those cases in which the excessively swollen, upper lid covers the lower, and confines the discharge, are even more dangerous. I would, however, prescribe treatment similar to that which I have advised in my paper, but should give very close personal attention to the case. I have never known the most critical stage in such cases to last longer than forty-eight hours, and I would make three or four visits every day, and be sure at each visit that the lids if everted were

placed in position, and the child not excited by the use of painful caustic applications. There is no difficulty in replacing the lid, but each time the child cries the lid becomes again displaced. The case should be faithfully followed up with mild local treatment. I would use the alum crayon, three or four times a day, as it causes little or no pain and does not distress the child, as would any strong or caustic solution.

Dr. E. WILLIAMS, of Cincinnati, said:—While I agree with the doctrines of the paper just read in many particulars, I disagree with them in regard to some essential points of treatment. It has been my custom to treat these cases much more energetically than is advocated in the paper, and I have never had a single case of ulceration occur during treatment in an infant. I evert the lids, thoroughly cleanse them, and brush them with a solution of nitrate of silver of the strength of from five to ten grains to the ounce, washing it off with salt water. Scarcely without exception the patient returns the next morning with eyes greatly improved. I continue this for three or four days, once a day, and when I have the disease under control continue the treatment by the use of astringents. I believe that in some cases the sulphate of atropia is very useful, but in infants half a grain to the ounce is as strong a solution as it is prudent to use.

Dr. H. W. WILLIAMS said:—I agree that in some cases it is well to use the stronger astringents, and perhaps an application or two of sulphate of copper. Sometimes I find it necessary to use active astringents, but even then, when danger is past, I return to the milder applications. I have known cases in which, lunar caustic having been used by the family physician, the eyes have been spoiled.

Dr. C. R. AGNEW said:—I would ask Prof. Williams how he would treat the worst typical case of gonorrhœal conjunctivitis, if called in within twenty-four hours, there being as yet no corneal lesion?

Dr. H. W. WILLIAMS said:—I have seen cases of that kind, with serous blood-stained discharges, in which I have thought it well to apply a strong astringent. I would say that I do not mean to limit myself or any other gentleman too strictly to the rules laid down. Every case must be treated by itself; but I have aimed to lay down a rule which is generally applicable.

Dr. JOHN GREEN, of St. Louis, said:—Prof. Williams's paper conveys the impression that in the purulent ophthalmia of new-born children success is uniformly to be expected if the case is treated early enough; nevertheless cases sometimes occur which do not turn out so well. I have seen two such cases in which the cornea was lost although carefully treated from the beginning of the attack.

Dr. GEORGE STRAWBRIDGE, of Philadelphia, said:—The modes of treatment which have been advocated are totally different, and yet have both been pursued by gentlemen of large experience. I have faithfully tried both methods. That which Prof. Williams, of Boston, proposes, is a good method when it can be carried out, but it is impossible to do that except in a hospital, or in a rich family, whereas these cases mostly occur among the poorer classes. My experience has been that after the acute stages have passed by, much benefit may be obtained from the use of the mitigated nitrate of silver.

Dr. S. D. RISLEY, of Philadelphia, said:—I have been struck with the extreme mildness of the treatment recommended by Prof. Williams. In the ophthalmia of new-born children, it has been my custom to use the mitigated stick, neutralizing any excess, as I can thus confine its action to the conjunctiva and spare the cornea. One other application which I have used with equally satisfactory results, is a twenty per cent. solution of carbolic acid in glycerine. It has the advantage of causing less pain than the nitrate of silver. Although painful at first, the pain subsides in a moment, and the child is subsequently more comfortable than after the use of the nitrate.

Dr. GEORGE C. HARLAN, of Philadelphia, said:—I see many of these cases at the Children's Hospital. Until four years ago, I never used nitrate of silver

of a strength greater than four grains to the fluidounce, but since then I have used as strong a solution as ten grains to the fluidounce, and so far from giving excessive pain it seems to give relief, and the children are more at ease after the immediate effects have passed off. In addition, I give directions for the use of an alum solution of the strength of five grains to the fluidounce as often as may be necessary to prevent the accumulation of pus.

Dr. J. H. Pooley, of Columbus, Ohio, said:—I employ the sulphate of copper, and it seems to me the better remedy for chronic conjunctivitis. I am not in the habit of using as strong solutions of nitrate of silver as Dr. Williams, of Cincinnati; using but five grains to the ounce. Stronger solutions I think very objectionable, and the mitigated stick cannot always be used without leaving a scar on the conjunctiva. I prefer to cleanse the eyes with a sponge rather than with a syringe, and I order the use of cold water.

Dr. E. Williams said:—I wish to say by way of explanation that, when I spoke of treating all infants uniformly, I did not mean to imply that I always employed solutions of the same strength. I have never seen more than two cases of diphtheritic conjunctivitis, and they were in children, not infants; each lost one eye. When we invert the lids, we see on the surface a somewhat grayish and consistent film, but this is easily removed and is not at all connected with diphtheria. I make it a point always to get this off. In speaking of my success I do not mean to disparage that of any one else, but I have found my plan of treatment successful, short, and certain.

Dr. William Thomson, of Philadelphia, said:—It seems to me that there is no difference of opinion with regard to the successful use of nitrate of silver, but only as to the mode of its application and the strength of the solutions to be employed. Applications made to the whole conjunctival sac, and so coming in contact also with the cornea, should not be stronger than two grains to the fluidounce; and I believe that it is generally better thus to bring the solution in contact with the whole conjunctival surface, than to make stronger applications to a part only of the diseased membrane.

Dr. C. R. Agnew said:—I feel somewhat embarrassed in telling what my views are, because I find that when we come to the subject of the report, we find cases behind the paper in which mild applications are confessedly not sufficient. We should stand ready to treat particular cases according to particular phases. If I find the eyelids sticky, and without much purulent discharge, my common direction is to bathe with a solution of common salt. If I find inflammation increased, and the discharge more watery, and wine-colored, I take the head of the child between my knees, bathe the eyes with water applied by a sponge, apply a solution of nitrate of silver, and then give some simple ointment to prevent the adhesion of the eyelids and the collection of pus. If the case goes on to a still worse condition, and it is evident that the cornea is to give way, I anticipate the evacuation of the aqueous humor by paracentesis. With regard to gonorrhœal ophthalmia, it is oftenest the case that it destroys the cornea within thirty-six hours, and that when it is not destroyed in this time, the case gets well. What we need to know is what to do when the lids begin to stiffen, and the fluid turns reddish or wine-colored. In a case of that kind I would give an injection of morphia in the corresponding temple, and run a pair of scissors into the external canthus, and cut down to the orbit, hoping to get a good spouting of blood. I would then pour water on the corresponding side of the head to encourage the bleeding, and finally repeat the application of morphia to produce sleep. Unless there is a decided improvement, I evert the eyelids and make an application of nitrate of silver, and afterwards apply iced cloths, which should be damp and cold, but not wet.

The President, Mr. R. Brudenell Carter, of London, said:—The differences in the treatment recommended are far more apparent than real, and would melt away in the presence of individual cases. Prof. Williams without doubt desires to show the danger of very strong astringents and caustics, and I believe that the Section will generally agree in his conclusions.

ON TUMORS OF THE OPTIC NERVE.

BY

H. KNAPP, M.D.,

OF NEW YORK.

A VERY limited number of tumors of the optic nerve has hitherto been recorded. W. Goldzieher, in a very valuable paper on the subject, published in Graefe's Archiv für Ophthalmologie, 1873, states that in modern medical literature he has found only six cases of the kind, to which he himself adds three others. I have found records of a few additional cases, and have seen three myself, two having been under my own care and one under that of Dr. E. Gruening at the New York Ophthalmic and Aural Institute. I shall give a brief sketch of some of these cases, in order to obtain the data necessary for a general description of the affection.

CASE I.—(Described by A. von Graefe, in his Archives, vol. x. part 1, p. 193 etc., 1864.) A farmer, æt. 23, had first noticed, two years previously, diplopia and protrusion of his right eye. The exophthalmos increased steadily, but the diplopia subsequently disappeared from the sight of that eye becoming lost. No pain was felt until a few weeks before the patient was first seen, when secondary infiltration of the cornea set in. On examination, there was found exophthalmos of 9‴, in the direction of the orbital axis; the lids could not be closed; the mobility of the globe was restricted inward, but in other directions was free; the centre of motion coincided with that of the globe. Behind the globe, a soft, elastic tumor was felt; S. = 0. The retinal veins were tortuous and dilated, the arteries attenuated. The inner half of the optic disk was swollen, with abrupt, slightly overlapping borders, reddish, and concealing the bloodvessels; the outer half was level.

Graefe diagnosticated a relatively benign, orbital tumor, probably of a fibro-sarcomatous nature. From the fact that the centre of rotation corresponded with the centre of the globe, he concluded that a stratum of loose connective tissue separated the tumor from the globe. The freedom of the outer half of the optic disk led him to the supposition that the blindness was not caused by compression, but by the immediate transition of the tumor to the nerve, or possibly by its origin in the nerve itself. The tumor being situated within the muscular funnel, and surrounding the optic nerve, its extirpation with preservation of the eyeball, he said, could not be thought of. He enucleated the eyeball, slit the stratum of connective tissue in front of the morbid growth, laid the bluish-red looking tumor bare, and removed it without difficulty. The operation was followed by alarming cerebral symptoms, intense headache and nausea; the pulse fell from 80 to 52 beats, and the temperature rose to 40° C. (104° F.). These symptoms subsided upon the appearance of an extensive suppuration in the wound. The patient recovered.

The tumor, examined by Recklinghausen, was pear-shaped, a little larger than a pigeon's egg, partially soft and gelatinous, and partially fibrous. The bulk of the optic nerve lay as a compact cord on one side of the tumor, and was covered, together with the whole tumor, by a tough, fibrous capsule. The limit of the nerve which looked toward the tumor, was ill-defined. The fibres of the cord-like portion of the optic nerve were preserved in their whole course, the others entered the tumor and were lost in it. This condition was found

in both the cut ends of the nerve, that is at its entranee into and its emergence from the tumor. The growth therefore was a *myxoma* of the optic nerve, originating in the inner sheath and perineurium internum. The swelling of the inner half of the intra-ocular end of the optic nerve was caused (according to the examination of Schweigger) "by a proliferation of indifferent (lymphoid) cells, as might be expeeted in so young a formation."

Case II.—(Deseribed by A. Rothmund, Zehender's Klin. Mon., 1863, S. 261.) The patient was a healthy looking girl of fifteen years. At the beginning of her second year, her left eye began to be pushed forward and downward, and was excessively painful. She could see with that eye. In the course of four or five months the eye became blind, but kept free from pain; yet the lids were swollen every now and then. From that time to her sixteenth year, she felt no annoyance from the eye, though it slowly but constantly grew larger. When seen by Dr. R., the orbit was occupied by a roundish, faintly fluctuating tumor, the size of a large hen's egg. The tumor was totally covered by congested conjunctiva, but incompletely by the lids, and moved harmoniously with the healthy eye in every direction. Remnants of the cornea were recognizable at its apex, which was very tender to the touch. The palpebral fissure was two and a half inches in length, and the orbit was greatly distended.

Dr. Rothmund removed the tumor in the manner of an ordinary enucleation. Its pedicle was severed near the optic foramen. In some weeks the orbital wound was closed by granulations. The patient recovered.

At the posterior end of the tumor a piece of the optic nerve, 3‴ in length, was preserved. The growth was a total degeneration of the optic nerve, consisting of fibrous tissue which inclosed smaller and larger cysts, the cavities of which were pervaded by a network of white, delicate fibres containing a vascular, gelatinous substance. The microscopic examination, made by Prof. Buhl, showed the characteristics of a *myxoma*, which is frequently met with in other nerves. The eyeball had been flattened by the tumor in such a way that the optic disk touched the cornea.

Case III.—(A. von Graefe, Arch. f. Ophth., x. 1, S. 201, 1864.) The patient was a young lady, æt. 24, who for two years had noticed a slight exophthalmos with amblyopia and contraction of the visual field of her left eye. With the ophthalmoscope Graefe discovered choked disk and spontaneous arterial pulsation. During the next five years the exophthalmos slowly increased, while sight was reduced to quantitative perception of light. The exophthalmos measured 8‴, and showed a marked deviation outward. Mobility preserved, but reduced in every direction. The centre of rotation corresponded approximately to the centre of the globe. A soft tumor was located immediately behind the eye, more on the nasal than on the temporal side. Between the upper and inner recti muscles it could be seen through the conjunctiva as a smooth, reddish-yellow intumescence. In the outer part of the field of vision, there was no perception of light. The optic disk was atrophic.

The patient was operated on by Langenbeck, who first enucleated the eyeball, and then the tumor, which did not extend to the apex of the orbit. It was as large as a pigeon's egg. The optie nerve passed as a flattened cord along its nasal side, but a considerable part of it entered the tumor and was lost in it. The posterior part of the eyeball was flattened by the pressure from the tumor.

The tumor, examined by Recklinghausen, was found to be a *myxo-sarcoma* of the optic nerve. Its anterior part contained a cyst, around and behind which there was a soft, partly gelatinous substance, eonsisting of very delicate, interlacing fibres which inclosed round and oval cells. The gelatinous liquid which could be pressed out of the tumor grew very opaque on addition of acetic acid. Larger portions of the tumor showed a substanee almost entirely composed of cells, between which some larger bloodvessels ramified.

Case IV.—(Described by E. Neumann, in Archiv für Heilkunde, xiii. S.

310.) The patient, æt. 20, had suffered from headache for six years, and had noticed a protrusion of her eye for three years. The mobility of the eye was preserved, and was only a little restricted in an upward direction. The vision was almost normal. On palpation a hard immovable tumor was felt surrounding the posterior part of the globe on all sides. Dr. Jacobson, of Königsberg, removed the eye together with the tumor.

The tumor was three-quarters of an inch in length, the size of a walnut, and immediately behind the eye surrounded the optic nerve, which passed quite loosely across it. The neoplasm was connected with the outer sheath, which on section showed itself as a distinct, white border-line. The inner sheath was smooth and shining. Between the two sheaths, delicate cords were stretched when the nerve was drawn away from the tumor. The microscopic examination discovered that the structure of the tumor was partly compact, partly alveolar. The alveolar part had a stroma which resembled that of a cancer, inclosing accumulations of fusiform cells in concentric layers. The centres of many cells were incrusted. Teased preparations showed that these fusiform elements were flat endothelioid cells. The compact part of the tumor consisted of coarse fibrils, here and there inclosing numerous accumulations of sarcomatous cells. The periphery of the tumor consisted of adipose tissue. The tumor is styled by Neumann a *psammoma*, on account of the prevalence of the incrusted, arenoid bodies in the alveolar part of the tumor.

Case V.—(Described by H. Knapp, in Archives of Ophthalmology and Otology, vol. iv. pp. 323–354, 1875, with four plates.) Mrs. J. K., æt. 40, was first examined in August, 1871. Her left eye was normal in function and structure; the right protruded about 5‴ in a forward and slightly downward direction. The protrusion had begun six months previously, and the sight of that eye had gradually become impaired. She suffered from occasional headaches, which always increased the protrusion of the eye. The movements of the globe were somewhat restricted, chiefly in an upward direction. There was no pulsation, nor bruit. S. with +10 Sn. $\frac{20}{100}$; F. complete. The optic disk showed a steep elevation, arteries small, veins dilated and tortuous. The ophthalmoscopic condition did not materially change for three years. In June, 1874, a hardish tumor was felt, on palpation, on the inner and upper side of the posterior part of the globe. It moved with the eye. A free space was felt between the tumor and the orbital wall. The globe was dislocated slightly outward, and considerably downward (4‴–5‴), but mostly forward (6‴). Inversion was perfect, whereas the strongest eversion brought the corneal margin no nearer than 2‴ from the outer commissure; downward rotation was free, but upward rotation moved the eye no higher than the horizontal median plane; S. $\frac{10}{200}$. F. complete. Media clear, pupil responsive. Papilla raised, with abrupt borders, like a "jockey-cap." Venous hyperæmia marked. General health good. For the last years, great pain in eye and head. The diagnosis of orbital tumor could now be more specifically revised into that of a tumor of the optic nerve, and acting upon this supposition I attempted, on June 10, 1874, to enucleate the tumor while preserving the eyeball. I succeeded in the following manner:—

I opened the conjunctiva by a circular section between the inner and superior recti muscles, and laid the anterior portion of the tumor bare. Then I introduced my left forefinger into the wound, and under its guidance freed the hardish tumor from the soft, surrounding orbital tissue. After that, I dissected the mass from the posterior part of the globe, with which it was connected by a thin layer of connective tissue. Further, I severed the optic nerve, and then introduced my finger as deeply into the orbit as possible. Not being able to reach the apex of the tumor, which was close to the optic foramen, I cut the tumor across as near the summit of the orbit as I could reach. Now I rotated the growth on its antero-posterior axis, and severed such connections with its surroundings as had before escaped. All this was done with a pair of strabismus scissors. Then I introduced the closed blades of a stronger pair of

scissors, curved on the flat, behind the tumor, and lifted it out of the wound. This produced sufficient free space in the orbit to enable me to remove the little part of the tumor which had been left. I seized it with a pair of strong scissors, had it drawn forward by an assistant, and excised it, always using my left forefinger as a guide. The eyeball, which had been turned aside, was replaced, and the conjunctival wound closed by sutures. Though the tumor was located within the recti muscles, none of them had been cut. The tension of the globe was not changed. The inner half of the eye was anæsthetic, the outer half sensitive, though a little dull. The fundus gave a reddish reflex, but its details could not be seen on account of irregular detachment of the epithelium from the cornea, giving the latter the appearance of frosted glass.

The reaction from the operation was very moderate. The patient now totally lost her headache. On the third day the fundus oculi was visible, and appeared uniformly milky-white. A small whitish-gray infiltration was noticed on the lower part of the cornea. On the fourth day this infiltration had increased, but the fundus could be seen through the clear part of the cornea above it. In the milky surface two dark-red streaks were visible, which had increased the next day, and proved to be the main branches of the retinal veins. Every day new retinal vessels were filled with blood, and in some weeks there was a picture of very marked retinal congestion. (See Fig. 2, Tab. IX., Arch. Ophth. and Otol., 1875.) The fundus had so far cleared up that only the region around the papilla and yellow spot appeared milky, the remainder having its natural color. Gradually this milky appearance disappeared, and then extensive retinal hemorrhage took place, which disappeared slowly, and left the retina and inner part of the choroid atrophic and degenerated. (See Plate X., loc. cit.) The infiltration on the lower part of the cornea, which I have mentioned above, disappeared after the eye had been protected by carefully closing the palpebral fissure. The cornea has never since lost its lustre, and but little change took place in the interior of the eye for two years. Lately, some membranous opacities in the vitreous have appeared. The eye has its natural size, is a little more deeply set in the orbit than its fellow, squints somewhat inward and downward, but has not been the cause of any annoyance. The cornea is bright throughout, though its inner half is still as anæsthetic as immediately after the operation. At this time, two years and three months after the operation, the tension of the eye is diminished, and there is some blood in the vitreous. The patient looks healthy, and there are no symptoms of a relapse.

The tumor was a little above the size of a walnut (30 by 27 mm.), and uniformly hardish. Its base was slightly concave, covered with a thin layer of connective tissue which had united it to the sclerotic. The optic nerve passed through it. The outer sheath of the nerve was well recognizable, though its fibres were more or less separated by the elements of the pseudoplasm. The intervaginal space was a little enlarged, and filled with the tissue of the growth. This, when well hardened in Müller's fluid, was uniformly finely granular. It had no capsule proper, but was loosely covered with the adipose tissue of the orbit. Under the microscope, the growth showed a typical specimen of *scirrhous cancer.* A stroma of fasciculated connective tissue inclosed alveoli filled with epithelial cells, which had no more intercellular substance between them than was necessary to cement them. In the neighborhood of the nerve, a limited number of arenoid bodies (sand-globules) were seen embedded in connective tissue, yet not numerous enough to give even a part of the tumor the character of a psammoma. The whole growth was very vascular, and a good part of its stroma had undergone colloid degeneration. The inner sheath was thickened, and infiltrated with colloid and lymphoid bodies, nowhere with cells of an epithelial character. The interfascicular tissue of the nerve was somewhat thickened. In it, and between the nerve fibrils, there were very numerous lymphoid cells, either single or arranged in rows and clusters; *inflammation of the nerve.* The same change was seen in the intra-ocular end

of the optic nerve—a well characterized specimen of *neuritis optica descendens*. For a detailed description of the specimen, and remarks on many interesting points connected with this case, see Arch. of Ophth. and Otol., vol. iv., 1875, pp. 323 et seq., and Plates IX.–XII.

Case VI.—(Described by L. Krohn in Zeh. Klin. Mon., 1872, S. 103–108.) A married woman, thirty years of age, had a tumor the size of a fist in the right side of her abdomen. It had begun six months previously with local pain, which was followed by general debility, emaciation, headache, vomiting, and gradual loss of sight in both eyes. A tumor was also developed in the left side of the abdomen. The eyes, when examined six months after the beginning of the disease, were totally blind, and, ophthalmoscopically, exhibited marked pictures of choked disk, with some retinal hemorrhages. The patient died eight months after the beginning of the disease.

The autopsy showed *carcinomatous tumors* of both ovaries; all the other organs were normal; the cranial cavity in particular contained no morbid formation. There was a small intumescence in each optic nerve near the sclerotic. The intumescence was caused by a new formation in the intervaginal space, consisting of a tissue which was rich in large round and polygonal cells, and had the arrangement of a cancer. The same kind of cells pervaded the inner sheath and the interfascicular connective tissue, and were very numerous around the central bloodvessels. They extended in the nerve and intervaginal space as far as the lamina cribrosa, but did not enter the eye. On the other end, they could be followed a little beyond the optic foramen. The intra-ocular end of the optic nerve was infiltrated with lymphoid bodies.

Case VII.—(Described by V. Szokalski, in Annales d'Oculistique, t. xlv. p. 43, 1861.) A boy of four years, healthy and well nourished, had a considerable forward protrusion of his left eye, which was perfectly movable. His parents attributed this condition to a severe contusion of the temple, received four or five months previously. The sight was good; by ophthalmoscopic examination, the retinal veins were found enlarged. The exophthalmos increased, the eye became very painful and sensitive to light, the lids could not be closed, the cornea grew opaque, the eye blind, and the other eye became the seat of sympathetic irritation. There was no tumor felt between the globe and the orbital walls; but the orbital tissues were a little more prominent and elastic than usual. None of the symptoms of a vascular tumor were present. Szokalski made the diagnosis of *cystic tumor* directly behind the eye, near the optic nerve, within the recti muscles. He extirpated the contents of the orbit. After having enlarged the palpebral fissure and made a circular incision around the globe, close to the orbital wall, he felt with the finger a hard, nodular, and movable tumor behind the globe, which he isolated from its surroundings by incisions along the orbital walls (*rasant les parois de l'orbite*); then he drew it forward with a strong hook, and, with scissors, severed it near the summit of the orbit. A profuse hemorrhage was arrested by placing a piece of ice in the orbit for two minutes.

The eyeball proved to be quite healthy. A centimetre behind the globe, the optic nerve entered into a nodular, transversely oblong tumor, the size of a filbert, through which the nerve passed in the direction of its smaller diameter, like a string through a bead. Its outer sheath passed directly over the tumor, where it became thicker and more adherent. The substance of the tumor was hard, grayish, fibrous, grating under the knife; it presented under the microscope, reticular areolæ, in the midst of which, caudate, uni-nucleated or multinucleated cells were accumulated. The nervous fascicles, after their entrance into the tumor, spread like a fan and were imperceptibly lost in its centre; on its other side they reappeared, and entered convergingly into the other end of the nerve. The growth "undoubtedly took its origin in the neurilemmatic partition-walls of the optic nerve, and unquestionably was of the nature of a scirrhous cancer." Though the nerve was divided as near the optic foramen

as possible, its transverse, cut surface showed, by its gray color, that the pseudoplasm had already penetrated into the cranial end of the nerve.

The wound healed so rapidly that the patient was able to leave the hospital at the expiration of three weeks. For almost five years the boy was in good health, but then a local relapse took place. When he presented himself again, the orbit was filled with a hard, somewhat elastic tumor, which projected in the shape of a small apple beyond the distended eyelids. The child was pale, but otherwise healthy, and complained of some lancinating, periorbital pain. The tumor was nowhere adherent to the orbital wall, and, therefore, easily removed. The patient did well enough during the next two days, but, on the third, convulsions and vomiting set in, and he died of meningitis on the sixth day after the operation.

An autopsy revealed congestion of the pia mater at the base of the anterior lobes, and a greenish, sero-purulent exudation at the anterior part of the base of the skull cavity. Under the left anterior lobe, in front of the Sylvian fossa, was a tumor, as large as a nut, growing from the degenerated optic nerve without implicating the chiasm. The adjacent brain substance was softened. The tumor was dense and consisted of a scirrhous substance.

The optic foramen was dilated and filled with a granulated, reddish substance, which connected the cranial with the orbital tumor. The orbit was enlarged to about twice its natural size, mainly at the expense of the ethmoid and maxillary sinuses. The tumor removed from the orbit contained on its posterior side a large cyst, which had been opened during the operation and had yielded about two ounces of a yellow serum. Sections in different directions through the tumor displayed a scirrho-cancerous, lardaceous substance, grating under the scalpel. Some softened portions of it contained a brain-like substance.

I have made a detailed extract of this case on account of its singular clinical and pathological interest. The original and secondary tumors are pronounced scirrhous cancer by the author, yet his description does not fully convince me. The original tumor may have been a fibroma, and the relapsing tumor a myxo-fibroma. According to the author, both tumors were scirrhous cancers, the secondary one having undergone partial softening in its interior.

These seven cases, together with eight others, form the material from which I shall endeavor to frame a general description of the affection under consideration.

Pathologically, the following species of tumor have been observed :—

(1) *Myxoma*, in five cases.
(2) *Fibroma myxomatodes*, in two cases.
(3) *Sarcoma myxomatodes*, in two cases.
(4) *Glioma myxomatodes*, in one case.
(5) *Fibro-sarcoma*, in one case.
(6) *Psammoma* (perhaps alveolar sarcoma or carcinoma), in one case (E. Neumann).
(7) *Scirrhous cancer*, in two cases.
(8) *Secondary cancer*, from primary cancer of ovaries, in one case.

The majority of these tumors consisted either of pure mucoid tissue (five cases, 33 per cent.), or contained a considerable proportion of mucoid substance in addition to their prevalent tissue. In most of these cases the pseudoplasm originated in the inner sheath and the interfascicular connective tissue of the nerve. To these should probably be added the case of scirrhous cancer of Szokalski, the recurrent tumor in which case contained also a large cyst. In three cases (glioma myxomatodes, fibro-

sarcoma, and secondary cancer) the tumors may have sprung from the tissue of the subvaginal space and the inner sheath. Two tumors of a carcinomatous structure (Neumann and Knapp) originated in the outer sheath.

Symptomatology.—The age of the patients varied from one and a quarter to forty years. There is no decided preference for either eye. The prevalent symptom is exophthalmos, either directly forward, in the direction of the orbital axis, or showing besides a secondary deviation in accordance with a greater development of the tumor on one side. The motions of the globe are preserved, but more or less restricted. On palpation, directly behind the globe a softish tumor is felt, the movements of which correspond more or less to those of the eyeball. A free space can be recognized between the tumor and the orbital wall. Neuritis descendens is a constant symptom in the early stages, and atrophy of the nerve in the later stages of the affection. The sight is more or less impaired, the field of vision being restricted when the pseudoplasm originates in the inner sheath and perineurium internum. Sight is less impaired, and the visual field remains complete, when the tumor springs from the outer sheath. Intense and persistent pain has been noticed in the cases of cancer (Neumann, Knapp, Krohn); in the other cases there was pain only when the eyeball was inflamed, with the exception of one case of myxoma (Gruening) in which intense pain was felt after the protrusion of the eye had existed for four years without pain.

The *course of the disease* shows in all cases a slow but steady increase of the tumor. The longest duration observed was fifteen years (Heymann and Rothmund). First, the posterior part of the eyeball is flattened, and the eye becomes hyperopic; then the eye protrudes, and neuro-retinitis sets in; then opacity, ulceration, and perforation of the cornea occur; after which the coats of the eye are so much pressed together that the optic disk touches the cornea. The ultimate, spontaneous issue cannot be known from the published cases, since the tumor in all was extirpated, and recovery took place, except in one case in which a relapse implicating the brain occurred, and in which the patient died from meningitis in consequence of a second operation.

The observations on record are not sufficient to enable us to form a reliable *prognosis* as to the natural course of tumors of the optic nerve. All the cases in which the tumor was removed ended in recovery, which, with the one exception of a fatal recurrence, seems to have been permanent. Sight is, of course, destroyed in all cases.

The *treatment*, until lately, consisted in the removal of the tumor, together with the eyeball, which operation offers no difficulty. Two years ago, I succeeded in removing such a tumor, and preserved the eye. About one year since, Dr. E. Gruening did the same in another case. Both of these cases have continued to do well. In one case (Graefe), the operation was followed by suppurative inflammation in the wound; in another (Szokalski), the operation caused death by meningitis. In none of the other cases was the operation followed by any unpleasant symptom.

In conclusion, I may say that tumors of the optic nerve form, at present, a sufficiently understood chapter of surgery, and that their differential diagnosis is quite possible. Extirpation relieves the patient from disfigurement and pain, and prolongs life. By improved modes of operative procedure, the tumor may be removed while the eye, which, though blind, is better than a glass eye, is preserved.

ORBITAL ANEURISMAL DISEASE AND PULSATING EXOPHTHALMIA; THEIR DIAGNOSIS AND TREATMENT.

BY

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The differential diagnosis between genuine intra-orbital aneurism and pulsating exophthalmia from other causes, is by no means easy. Till recently, all cases of protrusion of the eyeball, with pulsation, were set down as orbital aneurisms as soon as encephaloid, meningocele, and one or two other rare affections, had been excluded. A careful study of the literature of the subject, in the light of pathological anatomy, has unsettled this long established opinion. The case of so-called aneurism by anastomosis, in the orbit, published by Travers in 1808, presented symptoms, in common with many that have since been published, that are now known to be frequently present in affections of the vascular system entirely outside of the orbit. A warm difference of opinion prevailed as to whether these were cases of aneurism by anastomosis, or of circumscribed or diffused aneurism of the ophthalmic artery. But all located their starting point in the orbit.

Such were the views generally entertained till the publication of Nélaton's celebrated case in 1856. True, Baron, in 1835, had presented to the Paris Anatomical Society a specimen of aneurism of the carotid in the cavernous sinus, in which the symptoms, during life, had been pulsating exophthalmos and intense murmur heard by the ear over the protruded eye. These were the typical symptoms that passed current for orbital aneurism, both before and after that time. The strong significance of this observation was not then noticed. In 1853, Aubry had the good fortune to secure an autopsy in a patient in whose case all the symptoms of orbital aneurism had been present, and that diagnosis had been made. No arterial lesion whatever was discovered. The ophthalmic vein was greatly dilated and its branches enlarged, forming the "bossy, vascular, pulsating tumors observed during life." The vein was as large as the little finger, and its coats very thin, and it freely communicated with the greatly dilated cavernous sinus. The sinus terminated behind in a *cul-de-sac*, the communication with the inferior petrosal sinus being cut off. Though observed in 1853, this case does not seem to have been published till 1864. In 1841, M. Gendrin made public a case with symptoms of orbital aneurism, in which a post-mortem examination showed this interpretation to have been incorrect. "The ophthalmic vein was varicose and distended with coagulated blood. The internal carotid and origin of the ophthalmic arteries were surrounded by an adherent clot, continuous with a clot infiltrated into the arterial tunics." Mr. Walter Rivington very plausibly explains this condition as indicating an aneurism of the internal carotid in the cavernous sinus, which had ruptured.

Nélaton's first case is so important in the pathological history of this disease, that I will transcribe it entire from Mr. Rivington's *resumé* of

cases in connection with his excellent paper on Pulsating Tumors of the Orbit, in the Medico-Chirurgical Transactions for 1875. In that paper he gives the most complete collection of cases, and the most systematic and thorough criticism of them, that I have met with. The case of Nélaton is as follows:—

"Male, 21. Right eye. Thrust with the ferule of an umbrella in the left lower eyelid. *Symptoms and Progress.*—Abundant bleeding from the nose, and ptosis of the right upper eyelid. Wound of the left eyelid healed rapidly, but at the end of some days the right eye became more prominent, with diplopia and mydriasis. Two months later, left eye was healthy; right eye prominent; ptosis; dilated veins on the surface of the upper lid; pupil largely dilated; external strabismus; absence of movements of the eyeball; diplopia. . . . . Patient blew blood habitually from the right nostril; paralysis of the right third pair; pulsation of the eyeball; *bruit de souffle* continuous, with re-enforcements during arterial pulse; absence of thrill. A little later, *bruit de miaulement*, audible also to the patient. He continued to blow blood from the right nostril, and at times had epistaxis." Nélaton diagnosed wound of the right carotid in the cavernous sinus. The patient died not long after from bleeding at the nose, and the autopsy fully confirmed the diagnosis. "The body of the sphenoid bone was fractured, and the right internal carotid was found torn across in the cavernous sinus. The sphenoidal sinus communicated with the cavernous sinus on the one hand, and with the nasal fossæ on the other, and through this communication the hemorrhage had taken place."

Hirschfeld, in 1858, had a case presenting similar symptoms, as the result of injury, in which the diagnosis of effusion of blood compressing the motor nerves before their entry into the orbit was made. Death occurred, and a blood clot was found in the cavernous sinus, enveloping the third and compressing the ophthalmic nerve. The clot covered a small circular hole in the carotid, which looked as if it had been punched out, and which was occupied by a string of decolorized clot about two inches long, and passing into the mass of coagulum. The bones were sound. This case is of great value, as it proves that the usually accepted symptoms of intra-orbital aneurism may be produced by a lesion of the carotid artery; and also that a sharp blow on the face or head, without fracture of the base of the skull, may cause a rupture of the internal carotid. Nunneley had occasion to make a post-mortem examination of a patient whose carotid he had ligated five years before for orbital aneurism. He found a circumscribed aneurism of the ophthalmic artery at its origin *in the cranium.* In another similar case the post-mortem appearances seemed to indicate the existence of an aneurism of the internal carotid, which had ruptured into the cavernous sinus. These cases and others induced Nunneley to look for the seat of so-called orbital aneurism rather in the cranium than in the orbit. In a paper, in 1864, he says: "In the great majority of such cases of protrusion of the eyeball, there is no disease whatever in the orbit. The seat of it is most commonly intra-cranial. The protrusion of the eyeball is passive, and the other distressing symptoms are secondary, depending upon obstruction to the return of blood through the ophthalmic vein. That this pressure in the great majority of acute, spontaneous cases is caused by an aneurism of the carotid as it emerges into the cranium, or of the ophthalmic artery in its intra-cranial portion, near its origin, is I think now certainly proved." The cases of traumatic origin he explains in a different way, but by injuries and changes in the cavity of the cranium and not in the orbit.

In Bowman's case, in which "all the capital signs of orbital aneurism

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were present in a high degree," there was no aneurism found after death. The cavernous sinus, and the circular, transverse, and petrosal sinuses, were filled with clot softening in the centre, and the enlarged ophthalmic vein was plugged at its opening into the cavernous sinus. The great Nélaton had a second case, with the current orbital-aneurismal symptoms, following a fall from a carriage, where death occurred after ligation. He had diagnosed an arterio-venous communication in the cavernous sinus. A fracture through the body of the sphenoid bone and pars petrosa of the temporal, was found on both sides. On the left side a sharp point of the temporal bone was detached, and had probably penetrated the carotid and the wall of the sinus. At all events there was found a small hole in the carotid, below and externally, connecting it with the sinus, which was dilated; and the ophthalmic vein was dilated and tortuous. In a case of Dr. Oettingen, the symptoms were much the same, excepting that the veins of the eyelids were not particularly affected, and that the bruit was audible only over the eye; the patient died two years afterwards. No arterial lesions were found, but there was partial obliteration of the orbital veins. Dr. Oettingen attributed the symptoms during life to thrombosis of the orbital veins, and alleges the case as proof that blocking of the orbital veins alone, with no trouble behind, may produce pulsating exophthalmos and bruit. In 1868, Wecker lost a patient fifty-two days after ligating the carotid. The symptoms were as usual. The post-mortem examination showed great dilatation of the ophthalmic vein, and extensive atheromatous degeneration of the inner coat of the carotid. As no description is given of the state of the cavernous and other sinuses, Rivington thinks it not improbable that the extreme dilatation of the ophthalmic vein and its frontal branches, forming "the pulsatile thrilling" tumors observed during life, may have resulted from the existence of a fissure in the internal carotid, in the cavernous sinus, which was overlooked at the post-mortem examination.

To aid in the differential diagnosis between orbital aneurism and other lesions which produce many of the same phenomena, I will give the particulars of three cases, in two of which I think that this rare affection really existed.

Case of Spontaneous Cure of Orbital Aneurism by Rupture.—Mrs. K., æt. 55, in good health, of medium size, rather spare in form, dark hair and eyes, sallow complexion. In June, 1875, her friends noticed an unusual redness of the right eye. She had had no injury, pain, sudden noise, or anything whatever to attract her attention to the eye, but remembers feeling some transient, neuralgic pains in the temple of that side. The injection of the eye increased slowly till September, when undue prominence was noticed. The congestion of the eye was increased by hard work, but was not influenced by straining or stooping. The exophthalmos had probably existed and increased for some time before it was observed.

The patient consulted me first in September. There was then a striking exophthalmos of perhaps a fourth of an inch, the ball presenting directly forwards and the movements being perfect, except for a slight limitation in all directions. No ptosis, no sluggishness of pupil, no diplopia, and V. = 1. By the most careful examination with the finger pressed between the globe and the orbital margin, no pulsating tumor was felt. But to the touch and to the eye there was a marked pulsation of the globe and the surrounding tissues. The subconjunctival veins were enormously injected and tortuous, forming a wreath around the cornea by their large and numerous inosculations. The ophthalmoscope showed the veins of the retina equally dilated and tortuous, with the usual appearances of choked disk, but I could see no pulsations. The arteries

were not as straight as usual, but otherwise not altered. For want of time to examine the case further, the patient was told to abstain from all hard work, stooping, or straining, and to return for further examination in a few days. When she returned I found her condition essentially the same. For want of time, I again deferred further examination till another day, which was fixed upon. I explained to her the serious nature of the disease, and what might eventually become necessary. To my great regret, I saw her no more for five months, so that I did not have a chance to watch the progress of the case.

On the 12th of February the patient returned with the following history. Up to three weeks before, her condition had continued the same as when I last saw her. There had been no increased pulsation; no enlargement of the vessels of the forehead or pulsating prominences around the eye; she had never heard any bruit with the pulsations, and was not conscious of the latter except as she felt the eye with the finger. She stated also that on January 20 the eye had become somewhat painful and more prominent, had throbbed more strongly, and by the next day had become extremely painful. Then the globe was suddenly pushed almost out of her head with excruciating pain and great redness. In her own language she thought that the eye would "burst out of" her head. She applied to her family physician, Dr. Fishburn, who reported to me in writing as follows:—

Mrs. K. first called my attention to her eye, in the evening of January 20, 1876. I made a hasty examination at the moment, regarded it as a mild case of conjunctivitis, and requested her to call next day. On the 21st I was summoned to see her at her home. I found her face so much disfigured by a swelling over the right eye, that I hardly recognized her as the patient I had seen the day previous. The eye had the appearance of having received a terrible contusion from direct violence. The swollen and discolored lids were so firmly closed that it was impossible to separate them, and the effort caused exquisite pain. I directed the constant use of cold water and acetate of lead dressings, and a saline purgative, to be followed by quarter of a grain doses of morphia every three hours till the pain should be relieved.

22d. Patient worse; could not bear the cold water dressings; removed them and applied dry heat, which gave some relief. I ordered four leeches, to be followed by fomentations of poppy capsules.

23d. Found her very much improved; pain had moderated and swelling subsided considerably. I was now able to separate the lids; found the conjunctiva in a highly inflamed condition, and the tumefaction so great as to cause the ocular and palpebral portion to protrude between the lids, while a chemotic swelling surrounded the cornea. There was also great protrusion of the eyeball. Ordered a three-grain solution of atropia to be applied every three hours, and continued the anodyne fomentations, covering them with oiled silk.

24th. Patient improving; treatment continued.

25th. Improvement, and same treatment. In addition, I applied to the conjunctiva, with a brush, a few drops of a solution of nitrate of silver, gr. vj–f℥j.

26th. Swelling of the lids, and vascular congestion subsiding. The same treatment was continued till Feb. 12, when the patient was again referred to Dr. Williams.

At this time the exophthalmos was much greater than when I had seen the patient in September. The cornea stood half an inch in advance of its natural position, and was hazy, with extensive abrasion of the epithelium, and an intense zone of circumcorneal redness. I attributed the keratitis to the great prominence and exposure of the cornea. The patient said that her eye was much less prominent now than three weeks before, after the severe attack of pain and protrusion. Vision was very imperfect, explained in part at least by the state of the cornea. Movements of the eye much limited, but equally so in all directions, and no diplopia; pupil dilated by atropia, which had been constantly used. *There was now absolutely no pulsation felt anywhere* over the globe, or between it and the orbit. The eye could be pressed into the socket a line or more, but did not pulsate in the least, neither was any bruit to be heard on auscultation. Fundus of the eye dimly seen by the ophthalmoscope, and great swelling of the disk recognized. Ordered free use of atropia, and eye to be kept closed by wet bandage and compress.

Feb. 14. Improving rapidly; exophthalmos much less; cornea clearing; V. = $\frac{15}{80}$, with + S. 40. Choked disk, and remarkable enlargement and serpentine appearance of the veins, extending to the extreme periphery of the retina, with isolated points of ecchymosis. The arteries are small, partially obscured, especially on the papilla, but pulsate distinctly when pressure is made on the globe.

Feb. 21. Ball receded decidedly; cornea clear; V. = $\frac{15}{30}$, and patient reads No. 8 at 8″ with + $7\frac{1}{2}$; field of vision perfect; external vessels smaller, and eye less red; movements perfect.

Feb. 26. Eye receding; sclerotic less vascular, and showing white; V. = $\frac{15}{30}$; spots of effused blood still seen, especially at the sudden flexures of the retinal veins; outlines of the disk not yet sharp. Through March all the symptoms continued to improve.

April 8. The external vessels have disappeared above and below; the sclerotic looks more natural, and the exophthalmos is reduced to barely one-sixth of an inch; vessels of retina much smaller and straighter, and the hemorrhages becoming absorbed. Patient feels no inconvenience from the eye, and is gaining sight with the general improvement. On the 10th of June the patient is still better. Only two vessels forming a loop near the outer margin of the cornea attract attention. With this exception the eye appears as well as the other. There is a very slight degree of exophthalmos, not noticeable to the patient or to her friends; movements perfect, and pupil of medium size, but sluggish like the other; V. = $\frac{20}{30}$ with + 36. Fundus greatly improved; no traces of blood; disk nearly natural; and veins reduced in size and but little winding. On September 29, the patient called, at my request, for examination. The eye was perhaps one-twelfth of an inch more prominent than the other, but this could not be noticed without a critical examination. No trace of the external, enlarged vessels was seen except two small ones with a slight loop on the outer side of the cornea. V. = $\frac{20}{20}$ with + 42, almost equal to the other eye. On ophthalmoscopic examination, with dilated pupils, I could detect no material difference in the fundus of the two eyes, both appearing normal, and the arteries pulsating under same external pressure. By pressing the eye backwards with the finger, it seemed to be checked by a firm substance at the apex of the orbit, which did not exist on the other side. The patient was perfectly well.

I wish now to consider the question of diagnosis. Am I justified, in the present state of our knowledge on this subject, in calling this a case of intra-orbital aneurism? The symptoms of aneurism were, it seems to me, unmistakable. The pulsations of the protruded ball were synchronous with the systole of the heart, partly controlled by pressing the globe backwards against a yielding, elastic resistance, and completely stopped by pressure on the common carotid. I unfortunately lost the opportunity to investigate the *bruit de souffle* with the aid of the stethescope, but there is every reason to believe that it was there, and that it could have been heard. An important point in favor of the theory of orbital aneurism is that the patient herself never heard any bruit. In the great majority of intra-cranial aneurisms, the sound to the patient is audible, and often very distressing. In orbital aneurism, further away from the auditory nerve and labyrinth, with little or no direct bony conduction, I apprehend that the pulsations are not heard by the patient himself. There was no paralysis of any of the nerve trunks which pass near the cavernous sinus, and which are so often affected by direct pressure in cases of arterio-venous aneurism of the internal carotid. In short, all the symptoms were limited to the orbit, and all of an aneurismal character. The aneurismal sac was not very large, else it would have been felt by deep pressure of the finger; and it was not sufficiently firm to produce paralysis of the muscles and optic nerve by direct compression.

There was no dilatation of the branches of the ophthalmic vein, forming pulsating tumors, as in arterio-venous aneurism in the cavernous sinus.

The only serious obstruction to the return of the venous blood from the orbit, was confined to the veins of the eyeball, external and internal. The trunk of the ophthalmic vein could not have been seriously compressed without manifestations in its frontal and other branches. The symptoms were therefore all arterial in their origin, and confined to the orbit and globe of the eye. No well-authenticated case of arterio-venous aneurism in the orbit has ever been demonstrated by post-mortem examination. Besides, the symptoms were not of a character to make that supposition possible, much less probable. Of course encephaloid, meningocele, and venous tumors are excluded. It was not an erectile tumor, nor an aneurism by anastomosis in its restricted sense. The history, symptoms, and termination, all point to a circumscribed aneurism of the ophthalmic artery, in the orbit, which eventually ruptured, became diffuse, and was cured. I do not think that anything but an arterial hemorrhage could have pressed the globe so far forward and produced such extreme tension. That the aneurism must have been sacculated, with thin elastic walls and a small communication with the ophthalmic artery, is rendered probable by the occurrence of rupture and recovery without obliteration of the artery. That the artery is not obliterated, is sustained by the fact that arterial pulsations are observed in the optic papilla when external pressure is made on the ball. It could only be such a cause that would fail to produce more serious pressure on the ophthalmic vein, the rotatory muscles, and the optic nerve. That circumscribed aneurism of the ophthalmic artery does occur, though perhaps very rarely, is proved by the post-mortem examination in Guthrie's case, in 1825, where an aneurismal pouch was found on the artery in each orbit.

Some years ago also I treated a case in which I feel confident that there was an intra-orbital aneurism. The case was that of a young man who was shot in the eye by a small pistol. The ball passed through the globe and lodged deep in the socket, causing the eye to collapse and atrophy. The patient was treated by the late Dr. Blackman, and some months afterwards came to have an artificial eye inserted. On examination I found a round, pulsating, elastic, compressible tumor in the back and inner part of the orbit, about the size of a pigeon's egg. The man was not aware that anything was the matter till I called his attention to it. There was an aneurismal bruit heard with the stethoscope placed over the tumor, but not heard over the head or by the patient. It was exactly synchronous with the action of the heart and was completely stopped by pressure on the carotid. There were no venous enlargements in the socket or on the forehead. The globe was reduced to the size of the end of my little finger, so that the tumor could readily be seen and fully examined by palpation. I put the patient in the recumbent position, with the head raised; gave careful doses of digitalis, and had digital compression of the carotid tried for two days. It became irksome and painful to the patient, and my assistants gave out. So I called in Dr. Blackman, and we agreed to try instrumental compression of the carotid, which succeeded in completely curing the disease; and I afterwards fitted the patient with an artificial eye. He lived till recently, and died of phthisis.

These two cases are, I think, shown with reasonable certainty, by the symptoms and result of treatment, to have been genuine examples of orbital aneurism. In my case, published in the Medical Record, 1868, diagnosed as orbital aneurism and cured by ligation of both carotids, thirty days apart, I am now satisfied that there was an arterio-venous aneurism of the carotid in the cavernous sinus. I have since seen a case

with essentially the same symptoms produced by the passage of a cart wheel over the man's head. I never saw the man but once, and know not what became of him.

Case of Suspected Orbital Aneurism.—R. W., æt. 24, of stout figure, robust health, and sanguine temperament, was injured by the horn of a calf on January 18, 1876. The point of the horn struck the lower lid, glancing inwards and backwards and entering the orbit just above the tendo-oculi, causing an ugly contused wound and a severe concussion. The wound bled profusely, but healed in the course of a week, without surgical treatment. There was no bleeding from the nose, nor ocular hemorrhage, nor was the patient rendered unconscious by the shock. Sight was not affected, and there was no diplopia, headache, giddiness, nor other symptom of injury of the brain. There was no protrusion noticed. About five weeks after the accident, while stooping and driving a cross-cut saw, a sharp pain was felt above the right eye, running back over the ear to the temple. This sharp, peculiar pain came on at intervals, several times during the day, but was always provoked by stooping and straining, and soon passed off when the patient straightened himself up, and rested from sawing.

There was no giddiness, throbbing, or bruit. Late in April it was first noticed that the sight of the eye was misty at times, but without pain or other symptom of trouble. For this failure in sight a physician was consulted, who first detected an undue prominence of the eye. From that period, about May 1, till within the past four weeks, the exophthalmos increased slowly, but varied very perceptibly between morning and evening, being always greater in the morning. Since then it has remained stationary. When I first saw this patient, three weeks ago, there was a striking exophthalmos of over one-fourth of an inch, with slight injection of the sclerotic conjunctiva, and some serous, chemotic swelling within the external commissure. Corresponding to this were seen some large, deep-seated, inosculating vessels on the sclerotic. The pupil was larger than the other, and somewhat sluggish. The movements of the globe were limited by the prominence and the stretching of the muscles, but there was no paralysis. There was a divergence of $1\frac{1}{2}'''$ when fixing with the other eye, and the motion upwards and inwards was less excursive, but there was no diplopia, which I explained by the imperfect sight. Strongly marked features of swollen disk, and some neuritis; boundaries of disk obscured, and veins very large and tortuous to their extreme branches; slight alteration of macula lutea, blurring its distinctive features. Vision very defective in consequence of a large central scotoma, which patient expresses as well defined but not complete.

Tension of the globe natural. *No pulsation whatever of the globe nor bruit on auscultation.* When the eyeball is pressed directly backwards, it is arrested by a solid resistance. Diagnosis: Tumor of some kind at apex of orbit, probably of an aneurismal nature. In the complete absence of the three most characteristic symptoms of aneurism, pulsation, bruit, and some degree of elasticity on pressure, I rely upon the evident traumatic origin of the difficulty; its peculiar history; the absence of inflammatory symptoms, and the exclusion of the symptoms of either benignant or malignant tumor at the apex of the orbit. The symptoms followed so soon after the injury, and in the beginning were so characteristic, as to make this opinion more probable than any other. The sudden, sharp, severe pain running backwards over the temple, and caused by stooping and straining, coming on about four weeks after the blow, might be explained by the sudden rupture of the ophthalmic artery, directly as it enters the orbit through the optic foramen. Its coats may have been weakened by the original concussion, or the very slight rupture occurring there might have been plugged by the formation of a coagulum. The history of several published cases of supposed orbital aneurism, makes this latter supposition at least tenable. If the vessel then split or the plug was then suddenly dislodged

by the increased congestion from stooping and straining, why did not immediate protrusion of the eye, pulsation, bruit, etc., occur? It may be that the opening was very small, and close in the narrow apex where rapid expansion and consequent protrusion were prevented. It will be remembered that not till this occurrence was the vision impaired; and that the troubled sight took the patient to a physician who first detected the exophthalmos. The immediate effect of a slow oozing of blood from a small arterial rent in the supposed situation, would, of itself, be pressure upon the optic nerve and very slow protrusion of the ball without pulsation. As the sac, fed by so small a source, enlarging almost imperceptibly, may be filled as it forms with coagulated blood, and make a solid mass without pulsation either from its own expansion or from impulse conveyed to it by the throbbing, small artery, the complete absence, as yet, of pulsation and bruit may be accounted for. The same explanation would account for the absence of any serious symptoms of pressure on the ophthalmic vein or other structures. Of course this theory of the case must as yet be considered as open to question. The characteristic symptoms may yet be developed and make the case clear. My opinion, however, is so strongly in favor of an arterial lesion, that I believe that the symptoms can be relieved by systematic pressure on the carotid. As the patient is present, it gives me great pleasure to submit him to the Section for examination. I can only say that with one exception the symptoms are to-day what they were three weeks ago. The tension of the globe is now *minus*. It was not so then.[1]

[The patient was then presented.]

In conclusion, I will sum up the symptoms which seem to justify the diagnosis of true intra-orbital aneurism, as follows:—(1) If traumatic in origin, the nature of the injury. If it is by direct penetration of the orbit by a long, slender body, or a small shot or similar foreign substance, the ophthalmic artery may have been wounded, as was probable in one of my cases. (2) If the injury to the head or face, though severe, is not attended by symptoms of fracture of the base of the skull, or, in the absence of the positive symptoms of fracture, if the severity and character of the trauma are not such as to make such a lesion probable. (3) The strictly rhythmical character of the pulsations, if such exist. (4) The limitation of the region over which the bruit is heard, to the eye and orbit. (5) The strictly intermittent character of such bruit. (6) The fact that the patient himself does not hear the *souffle*, or, if so, very slightly. (7) The less frequent occurrence of paralysis of motion or sensation, or of any symptoms indicating a disturbing cause in the cavity of the cranium. (8) The less frequent and less serious impairment of vision. (9) The absence, except at an advanced period, and even then the less intensity, of the symptoms of obstruction in the ophthalmic vein, such as the existence of soft, thrilling, pulsating tumors around the eye, and of enlarged and pulsating frontal veins. (10) The complete curability of the disease by direct compression through the eye; or by compression of the common carotid; or by ligature of *one* (the corresponding) carotid.

The symptoms, till recently, accepted as characteristic of intra-orbital aneurism, are much graver and more extensive in their distribution. When we submit them to the test of pathological anatomy, as far as such cases have been examined, it is seen that they depend on lesions

[1] This patient was seen again on April 15, 1877, when there was great protrusion of the eyeball with aggravation of all the other symptoms; a tumor could now be felt behind the globe. Enucleation of the eyeball was resorted to, when the tumor, which was firm and fibrous in character and the size of a small pullet's egg, was readily removed. It was situated between the optic nerve and the inner wall of the orbit, but was not firmly connected with either. (July 5, 1877.)

beyond the orbit. Of the 73 cases reported in Mr. Rivington's paper, in 12 only were autopsies made. Some of Mr. R.'s conclusions I will quote: "In no single instance has aneurism by anastomosis, or cirsoid aneurism within the orbit, been verified by post-mortem discovery." "In no single instance has an arterio-venous aneurism been found within the orbit at a post-mortem examination." "In no case have the symptoms of intra-orbital aneurism been proved to be due to a tearing across of the ophthalmic artery at or near the optic foramen." In the case of Guthrie, an aneurism was actually found in each orbit. In one case, no arterial lesion was found, but partial obliteration of the orbital veins, with evidences of previous inflammation in the orbital tissues. In the other ten, in which autopsies were made, the symptoms were caused by disease of the vessels just back of the orbit, in the cavity of the cranium. Of these, some were dependent upon obstruction to the return of the venous blood from the orbit to the brain. In one a small aneurism existed on the ophthalmic artery at its origin from the internal carotid. In three there was rupture of an aneurism of the internal carotid, within the cavernous sinus. In one, "simple dilatation and atheromatous degeneration of the internal carotid artery in the cavernous sinus." "In three, traumatic cases, a direct communication between the carotid artery and the cavernous sinus was found."

Finally, a word as to treatment. Compression, either direct, or of the common carotid, or both, is advisable in all cases, and succeeds in some. Coagulating injections may sometimes be successfully practised, but require great caution. And finally, ligation of one or both carotids will be nearly always necessary for the extra-orbital cases.

SPONTANEOUS CURE OF CONGENITAL PULSATING EXOPHTHALMOS.

BY

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THIS case was reported at the last meeting of the American Ophthalmological Society, but since then the aneurismal tumor has spontaneously undergone complete consolidation. The following abstract of the history, to date of last report, is taken from the transactions of the society referred to:—

L. L., æt. 25. General health good. Left eye always prominent, and left side of head larger than right. Has always had a "rushing sound in his head and eye." Left eye enormously protruded, forced downwards and outwards, and immovable. No useful vision. Whole left side of face hypertrophied. Mucous membrane of left side of mouth thickened, and left side of tongue twice as large as right. A localized tumor, probably a distended vein, above and to inner side of ball, and a doughy swelling of the soft parts extending about an inch above the orbit. There was strong pulsation, and an aneurismal bruit could be heard several feet from the patient's head. Five years after the first examination, the protrusion had slightly increased, but the bruit had decidedly diminished. The supra-orbital and frontal vessels were much distended, and pulsated strongly, and this aneurismal condition extended down the angular branch of the facial, on the right side.

On December 27, 1875, the patient came to me, complaining of pain in the eye, and over the bridge of the nose, and along the course of the angular artery on the right side. There was slight erysipelatous inflammation at the seat of pain, but not much change in the appearance of the parts. The bruit had entirely disappeared, and there was no thrill in any of the vessels, and no pulsation anywhere except in the localized tumor above the ball. The supra-orbital vessels were scarcely perceptible. He had not been under any treatment, as he was not able to keep up even occasional, intermittent compression of the carotid, on account of the giddiness and faintness which it produced. The continuous application of iced water was directed.

December 31. There was violent pain in the tumor and head. The tumor was increased to at least twice its former size. Pulsation was just perceptible. The exophthalmos was enormous, and the eye could no longer be covered by the distended lids. The patient was admitted to Wills Hospital, and anodynes were given in large doses, and dry cold substituted for the iced-water applications.

January 2, 1876. The pain was somewhat relieved, and all trace of pulsation in any part of the tumor had disappeared. The cornea was completely anæsthetic, and was beginning to slough. The pulse was feeble, and the pulsation of the left carotid was weaker than that of the right. From this time the pain and swelling gradually subsided, and

when the patient left the hospital, on February 2, the tumor was completely consolidated, and had considerably diminished in size.

No satisfactory way of accounting for this sudden and unlooked-for termination of the disease suggests itself to my mind. It is interesting as illustrating the possibility of spontaneous cure under circumstances that would seem to be about as discouraging as possible, and suggests the inquiry, what percentage of cases, if let alone, might end in the same way? This question cannot be answered, as nearly all reported cases have been subjected to some kind of surgical treatment.

I find records of five other cases of spontaneous cure, all but one having been of traumatic origin.

Case I. Collard; quoted by Erichsen (Science and Art of Surgery, American Edition, 1869, p. 643). A man, 41 years of age. Injured by fall on back of head. Bruit, pulsation, exophthalmos. Symptoms entirely disappeared in three years and a half.

Case II Erichsen (Ibid.). Man, 44 years of age. Fall on head. Attacked by symptoms of aneurism of left orbit in most marked manner. Bruit, pulsation, exophthalmos. Ligature of carotid urged by Mr. Erichsen but declined by patient. At the end of fourteen months the symptoms had, to a very great extent, disappeared.

Case III. Holmes (American Journal of the Medical Sciences, July, 1864). Man, 23 years of age. Gunshot injury. Exophthalmos, chemosis conjunctivæ, bruit, pulsation. In about three months all the symptoms had entirely disappeared, except slight congestion of the conjunctiva.

Case IV. France (Guy's Hospital Reports, 1853). Woman, 38 years of age. Point of umbrella thrust into orbit. Exophthalmos, pulsation of eyeball, and defined, pulsating tumor above and to inner side of globe; complete blindness. All the symptoms but blindness disappeared in eight months.

Case V. Julliard, of Geneva ("Note sur un Anévrisme Intra-orbitaire, etc.," quoted by Rivington, Medico-Chirurgical Transactions, London, vol. lviii. p. 272). Woman, 69 years of age. Spontaneous. Enormous exophthalmos, pulsation, bruit, chemosed conjunctiva. In two months all the symptoms but exophthalmos and blindness had disappeared. A few months later, the exophthalmos was also gone.

In none of these cases was any local treatment resorted to, except that in two or three instances cold applications were employed in the early stages. In connection with the case reported to the Section by Dr. Williams, and that now submitted, they make a total of seven instances of spontaneous cure of this affection.

Though statistics prove beyond doubt that ligation of the carotid affords the largest percentage of cures, it is well, before deciding upon its performance, to remember not only the possibility of cure by milder means, or even without resort to any surgical procedure at all, but to bear in mind also that, though one of the most brilliant operations in surgery, it is not without its chances of failure and its dangers of a fatal result. All the operations that I have been able to collect, including two of Dr. T. G. Morton's, not yet reported, furnish sixteen per cent. of deaths and about the same proportion of failures, leaving only sixty-eight per cent. of cures and partial cures.

ARE PROGRESSIVE MYOPIA AND CONUS (POSTERIOR STAPHYLOMA) DUE TO HEREDITARY PREDISPOSITION, OR CAN THEY BE INDUCED BY DEFECT OF REFRACTION ACTING THROUGH THE INFLUENCE OF THE CILIARY MUSCLE?

BY

EDWARD G. LORING, Jr., M.D.,

OF NEW YORK.

After a careful consideration of the above question, I have come to the conclusion: (1) That hereditary predisposition, though undoubtedly a potent cause, is not only not the sole cause, but not even the predominating cause, of progressive myopia; and (2) That the action of the ciliary muscle, taken by itself, exerts but little influence on the production of myopia, and still less on the formation of the cone.

The arguments on which these conclusions are based will be found in the following remarks:—

The belief that "like begets like" has been formulated in the speech and crystallized in the proverbs of every people since the world began, and although this uniformity of opinion exists as to hereditary predisposition, and its influence on the organism in general, a great deal of doubt has been expressed as to the degree of effect which it produces on particular and individual organs, and especially on those which fulfil the functions of special sense. Nor can it be doubted that grave objections have from time to time been raised, and many weighty arguments advanced, which would go to show that the effect of direct transmission could not be so readily detected, nor so clearly demonstrated, in regard to the special senses as to the organism as a whole. Still, in later years, as proofs have gradually multiplied and apparent discrepancies have been reconciled, the opinion has gradually become more general that, beneath a mass of apparent contradictions and almost endless exceptions, there lies a general tendency of inheritance which authorizes us in assuming that even with our special faculties there is a uniform, though perhaps, from want of knowledge, a rather flexible, law, which controls, to a degree at least, their form and character.

In none of these is it claimed that this law of transmission is made more manifest than in sight, the noblest and most intellectual of all our faculties. Thus it has been assumed by the preponderance of authorities, that the specific type of the organ of vision has its varieties of form and perception, every one of which may owe its origin to the force of heredity. Thus it is held that color blindness, strabismus, hypermetropia, and myopia, are transmissible by generation, so that according to Lucas(1)[1] there are families among which many of their members owe

[1] The numbers attached to the names of the authors refer to the bibliography at the end of the paper.

to hereditary influences alone the conformation of their optical apparatus, and the fact of their range of vision being short or long. It is further claimed that the statistical results of Funari(2), supported by the statements of Piorry(3) and Portal(4), have shown that most myopes are the sons or grandsons of myopes. It is certainly not necessary for me to remind the members of this Section that views, similar to, if not identical with, those just quoted, as to the hereditary tendency of myopia, have held sway successively in England, France, Germany, and in this country, and indeed in every country. Nor would the time allotted to this paper allow me, even if your patience would, to cite the long list of distinguished names through whose authority this wide-spread opinion has had and still has its existence; still, for the sake of the arguments which are to follow, I should like to be allowed to refresh your minds in regard to the opinions of a very few of those who in our own time have done so much towards moulding the prevailing opinion as to the hereditary nature of myopia.

Thus Stellwag von Carion(5), in discussing the causes of myopia and the elongation of the eyeball, says "the predisposition to this is of course congenital, and is, as a rule, hereditary," and adds, in speaking of the staphyloma and its relation to the scleral protuberance, that the "exquisitely hereditary character of the affection can also be brought forward as a proof of this." Jaeger(6), while emphasizing the fact that myopia is not the "prerogative of industry," declares that "posterior staphyloma is hereditary to a predominating degree." And Donders(7), in speaking of the causes of myopia, affirms that "the predisposition is almost always congenital, and in that case it is, moreover, nearly always hereditary. Beer, Jüngken, Bohn, Von Hasner, and many others, have referred to its hereditary nature, and I believe even that from time immemorial the conviction thereof has been general among the people." Many more examples quite as explicit as these might be cited here, were it necessary, since, as you are well aware, every standard work on ophthalmology is replete with them.

It would be supposed, from the force of the expressions and the weight of authority from which they come, that these opinions in regard to the predominating influence of heredity in myopia would be based on exact and extensive statistical information which would embrace, certainly, as wide a scope as from grandfather to father and from father to son. Yet, as far as I am aware, no such statistics exist, at least of such a nature as to satisfy the exacting demands of modern science. It is very true that both Donders and Jaeger have—as indeed we all have—been struck with the frequency with which myopic parents bring to us their myopic children; but, as you are well aware, the mind, in these matters, is more prone to be struck with resemblance than with dissimilarity, and no account is taken or recorded anywhere, as far as I know, of how often the reverse holds good, that is, how often children with myopia are brought by parents who, on actual examination, are shown to have normal eyes, and whose ancestors on both sides, as far as known, were never near-sighted. Moreover, I found that among 715 well-educated and intelligent persons, whom I examined for this special purpose, the percentage of those who had normal eyes themselves, but whose parents were myopic, was nearly identical with the percentage of those who were not only myopic themselves, but who had myopic parents. That is to say, that the percentage of emmetropic children from myopic parents was as high as that of myopic children from myopic parents. No one more fully

recognizes or more freely admits than I the liability to error in statistics gathered thus as it were at second-hand. Nothing but the tabulated observations of three generations at least can be of much value as regards heredity, but as such do not exist, I offer the above for what they may be worth.

These observations would, however, seem to be in accordance with those of others. Thus Cohn(8), in his investigations as to the refraction of the eyes of school children, found that out of all the myopes there were only 2.7 per cent. whose father or mother were myopic. I found that only 6.11 per cent. had either father or mother near-sighted. With both parents myopic, the percentage falls with Cohn(9) to 1.04 per cent. and with me to 1.11 per cent. Erismann(10) found, however, from his statistics that 30.6 per cent. of the myopes had one or both parents myopic. Taking, then, Erismann's percentage, which is an enormous increase over Cohn's and mine, as a basis, we find, even then, that two-thirds, or seventy per cent., of the myopic children had parents who were not myopic. This certainly does not show, as far as statistics are concerned, a marked hereditary tendency, or warrant, it would appear, the expression of so decided an opinion as that which we have quoted from the leading authorities.

There are, however, in the absence of exact statistical knowledge, several factors of less importance, it is true, but still of sufficient force to aid us materially in the solution of the problem, and amongst these the most important are necessarily those of an anatomical nature. For if in any case of disease it can be shown that an anatomical formation is peculiar to a certain disease, the presumption is that it is congenital as a rule; and the earlier it shows itself the more likely is it to be hereditary. That both of these conditions have been fulfilled in regard to myopia by the change known as posterior staphyloma, is almost universally admitted. Thus Stellwag(11) declares that posterior staphyloma is due to congenital malformation, while Jaeger(12) says that "the increase in the axis is usually accompanied with a cone which is mostly congenital, and frequently hereditary," and adds that he has seen "typical cases of it in the eyes of new-born children both in life and after death;"(13) and Donders(14) only strengthens the almost universal opinion when he declares that from the frequency with which changes in the bottom of the eye have been observed with the ophthalmoscope, myopia and staphyloma have become nearly synonymous terms. This statement he supports by a reference to Graefe(15), who had previously declared that, in myopia of $\frac{1}{6}$ to $\frac{1}{2}$, ninety per cent. showed that peculiar change in the fundus, and adds that he himself(16) believes the proportion is much nearer one hundred per cent. Still there can be but little doubt that the general and rather sweeping assertion that posterior staphyloma is synonymous with myopia, is somewhat exaggerated. For out of 500 myopic eyes I found only 20.5 per cent. affected with the crescent. Cohn(17) in 1004 cases of myopia, between the same ages, that is, from 6 to 21 years, found the cone present in only 20 per cent.; while Max. Conrad's(18) examinations in 1001 myopes showed 28.1 per cent. The average of these results would then show that only 22.3 per cent. of myopic eyes have the cone. Of my own cases there were only one-fourth which showed any change whatever, while there were three-fourths of the eyes which showed no deviation from the normal standard. This is certainly at variance with the accepted views, though it is corroborated as seen above by the extensive statistics of Cohn and Conrad, the unanimity between the results being

very remarkable. When, however, we come to the proportionate frequency of the cone in regard to the three classes of refraction, we find that it predominates to a marked degree in myopia. Thus I found of 2265 eyes examined, that of the emmetropic eyes 3.33 per cent. had cones more or less developed; of the hypermetropic 3.49 per cent.; while in the myopic the percentage rose to 20.56 per cent., or six times as much as in either of the other classes. We are forced to conclude, therefore, that there is an anatomical variation predominating in myopic over other eyes, although the frequency with which it occurs has been, it would appear, much overrated—certainly for this country. Still, too much weight should not be put upon this fact as an argument as to the congenital and hereditary character of the cone, since, in the first place, an anomaly which makes its appearance in only about one-fourth of the cases, and under the most favorable auspices for its development, that is, in school children between the ages of six and twenty-one years, cannot be said to be strongly congenital.

Moreover, it is a fact that, although, as stated by Jaeger(19) and Von Hasner, well-marked cones are found in new-born children, they are, as known to all, and admitted by them, comparatively rare; while they increase in frequency in after life, and with close application; and oftentimes in eyes which have been shown by repeated ophthalmoscopic examinations to have been previously free from the slightest trace. From these facts, and from the fact that myopia consists as a rule simply of an elongation of the globe, which would, from purely mechanical reasons, produce a crescent, a doubt has arisen, in my own mind at least, whether after all the cone was an expression of congenital malformation of the scleral protuberance, or indeed of any part of the scleral split.

For, if it had any connection with the split, we should suppose it would be in reality what both its name and alleged anatomical origin imply, that is, an actual protrusion of the sclera, or at least an excavation or limited thinning of this membrane, as is invariably the case with a true coloboma, as demonstrated as well by the ophthalmoscope as by dissections. But I hardly need remind those of you who use the upright image that there is almost never the slightest difference in level between that part of the sclera which represents the cone and the adjoining portions, and this, too, no matter how large the crescent is. The cone is, in fact, not a true staphyloma; and it is only when applied to the entire posterior portion of the eye that the name is at all applicable, and we should be forced to assume that the whole posterior part of the eye was congenitally defective, or at least that parts were so which were widely separated from the scleral protuberance.

The second anatomical variation which is supposed to be characteristic of the myopic eye, and one which has had great importance attached to it, is the peculiar conformation of the ciliary muscle, which is supposed to be congenital. From the labors of Iwanoff(20) it was shown that the ciliary muscle in highly myopic eyes varied from that in other eyes, not only in general outline, but also in its composite structure. In the drawing (Fig. 1), which is taken from Iwanoff, the shape and outline of the muscle in the three classes of refraction are contrasted with each other. The solid line represents the muscle of an emmetropic, the dotted that of a myopic, and the broken line that of a hypermetropic, eye. It will be seen that the muscle in the myopic eye extends further back along the sclera than in the normal eye, and is, moreover, broader. It will be observed also that the short side, or leg, of the triangle, which repre-

sents the muscle as a whole, forms in the normal eye nearly a right angle with the sclera, while that in the myopic eye forms an acute angle, with the apex towards the inner portion of the eye. As to its component parts, it was shown that the circular fibres found in the normal eye were

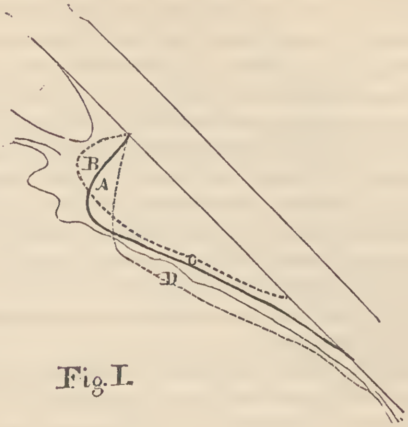


Fig. I.

either very much reduced or entirely wanting in the myopic eye, while they were increased in the hypermetropic eye. It was, therefore, assumed that, inasmuch as the myopic eye from its structure had no demands for active accommodative efforts, the reason why the circular fibres were not present was because they had become atrophied from disuse, so that the entire triangle marked A had disappeared. While, on the other hand, the circular fibres had been so increased by active accommodative demands in the hypermetropic eye as to occupy the superadded triangle B. It was further shown by Iwanoff that not only had the circular fibres become atrophic, but that the longitudinal had become hypertrophied. From this it was assumed that the action of the muscle had been changed, and that, instead of being compound, the circular fibres drawing toward the axis of the eye while the longitudinal drew directly forward, the action had become solely the latter, so that the muscle had become what the author calls a pure "tensor choroideæ." If we should accept this theory, we would have the apparent *reductio ad absurdum*, as the author himself says, in the development of a force which he supposes to produce active accommodation, in an eye which does not require or use it; for traction on the choroid and consequent relaxation of the zonula is supposed by Helmholtz and other physiologists to be the important element in the production of accommodation.

It would certainly lead me too far to enter in a discussion of this subject, but it seems as if a very important factor, and one which might produce a marked effect on the form and shape of the muscle in the myopic eye, had been neglected in the consideration of this question. This is the effect of distension of the investing membranes of the eye, which in some cases is enormous, as, for example, where progressive myopia is developed very rapidly and to a very high degree. It would, therefore, certainly seem more natural to attribute the change in form and structure to this than to any difference in function of its component parts. Thus if the posterior pole of the eye should yield, the insertion of the muscle at its choroidal end would be drawn back as seen in the drawing, and the angle

which, as we have seen, was a right angle in the normal eye would then become acute, since the insertion around the canal of Schlemm and adjacent parts would remain stationary. The few so-called circular fibres which form an anastomosing network, whose meshes run more or less perpendicular to the direction of the longitudinal fibres, would on being stretched assume precisely the opposite direction, and would then have on section the same general direction as the longitudinal, as would of course also the so-called radiating, fibres. Moreover, as the horizontal and vertical diameters also increase somewhat in these highly myopic eyes, the zonula would be stretched, and would then draw with a gentle but sustained traction on the muscle, the general direction being perpendicular to the axis of the eye. Thus the fibres of the muscle would have a tendency to draw apart, and the muscle to increase in bulk. This tension on the zonula would also account for the fact that the lens is flatter in high degrees of myopia than in the normal eye. Thus appearances hitherto supposed to be congenital might occur from a simple mechanical cause, acting during life in an eye which was not congenitally myopic, and with no hereditary tendency.

Thus while not denying the hereditary tendency towards a too yielding sclera, I have become more and more convinced that there are many cases in which the cone is simply the expression of a purely mechanical effect, distension, from which all the peculiar appearances of the crescent might result without there being the slightest connection between the cone and any congenital or hereditary tendency whatever. Does the inguinal ring never give way, or the fibres of the bladder become relaxed, or the pleura distended, or ascites occur, or even the bony cavity of the head enlarge, except through hereditary tendency? and if these can owe their origin, as they often do, to exciting causes which are purely spontaneous and fortuitous, why may not that distension of the sclera, which is myopia, take place, especially when the growing and elastic membrane is subjected to over-use, without our being compelled to attribute it "almost invariably to congenital and hereditary influences?" Clinical experience would point to such a conclusion, for it is a fact that cases occur where myopia with all its characteristic signs is produced in eyes which were previously normal; that is, in the eyes of adults, who have passed therefore the period of development, and whose family history shows no hereditary tendency. This, too, notwithstanding Donders's(21) assertion that he has never seen "myopia arise after the twentieth year in eyes which were previously normal." I have seen several such cases, and I doubt not that most practising ophthalmologists have had the same experience.

If this distension of the sclera did take place, there would be, necessarily, a change in the refraction of the eye, and this would have a decided weight in determining the question of hereditary influence. For if it could be shown that normal eyes which, according to the theory of heredity, would presuppose normal, *i. e.*, emmetropic, ancestry, or better still if it could be shown that hypermetropia, the direct opposite of myopia, with the opposite hereditary tendency, could and did frequently pass into myopia, then it would be an almost convincing proof that myopia could be and often was produced in spite of hereditary influence against it.

That an emmetropic eye can and does pass into a myopic eye, and that it has been proved to do so by direct observation, I think will be admitted by the great majority of observers. But that a hypermetropic

eye can pass into an emmetropic or normal eye, and thence into a myopic eye, is still a matter of great doubt in the minds of some of the best authorities. Thus Donders(22) says: "I have never seen a hypermetropically constructed eye become near-sighted." An opinion which he corroborates and strengthens in many other passages in his world-renowned book. Jaeger(23), however, makes a diametrically opposite statement, and declares that an interchange of refraction does take place, and that "thus an hypermetropic as well as a normally-constructed eye becomes a myopic eye through posterior staphyloma at the posterior pole of the eye." Stellwag(24) proclaims the same thing in very nearly identical terms. Various authorities too numerous to mention have ranged themselves on one side or the other of these opposite opinions, though I think that the majority of the more modern observers are tending towards the view expressed by Jaeger, that an interchange of refraction from a hypermetropic to a myopic eye can and does take place. Still it must be admitted, when it comes to actual demonstration, that the few cases which have thus far been cited are not sufficient in number or accuracy of detail to be of the slightest weight. One would certainly suppose, *à priori*, that such cases would have been observed in great numbers, and the fact that they have not, in the past decade when refraction has been so minutely studied, would seem to point very strongly to the fact that they did not exist.

In the dearth, therefore, of sufficient direct proof by observation of the passage in the same person of emmetropia into myopia, and the almost total want of such evidence of change in hypermetropia, we are forced to the consideration of collateral evidence which, it appears to me, if correctly taken and properly appreciated, is nearly as convincing as that of actual observation. I allude to the proof furnished by statistics taken from large masses of individuals, which show the proportionate rate in which the different classes of refraction occur at different times of life, and with different degrees and kind of application of the eyes. From the statistics of Ware(25), in 1813, and in later years from those of Szokalski(26), Schürmayer(27), Jaeger(28), Ruete(29), and others, very important facts were obtained. Thus it was shown by Ware that myopia was more common in cities than in the country, and that it increased in frequency and in degree with the age and amount of close application, while Ruete also called attention to the deleterious effect of insufficient and faulty illumination. These facts were afterwards corroborated by Cohn, but unfortunately Cohn's statistics, extensive as they are, are not suitable for our purpose, since, from the manner in which they were compiled, no correct idea can be obtained from them as to the frequency with which, in comparison to each other, the three different classes of refraction occur.

The statistics to which I shall call your attention are those made by Erismann(30) in Russia, Max Conrad(31) in Germany, and those by Dr. R. H. Derby and myself in this country. I choose these as examples since they are the only ones which I know of where sufficient numbers were used, and where, at the same time, the conditions of investigation were the same, or nearly the same, in each. Thus they were all upon the eyes of school children between the ages of six and twenty-one years, and the trial by glasses and Snellen's type at twenty feet were the basis of the examination; while all the more commonly occurring degrees of refraction from the lower ($\frac{1}{60}$) to the highest grades were included. Every eye was examined by the ophthalmoscope in the cases reported by Dr. Derby

and myself. Erismann's statistics were made in St. Petersburg on the eyes of 4358 scholars; Conrad's at Königsberg on 3036 eyes, and Dr. Derby's and mine on 2265 eyes in New York. The results of these statistics are arranged in a tabular form in the diagrams marked respectivly

Table I.

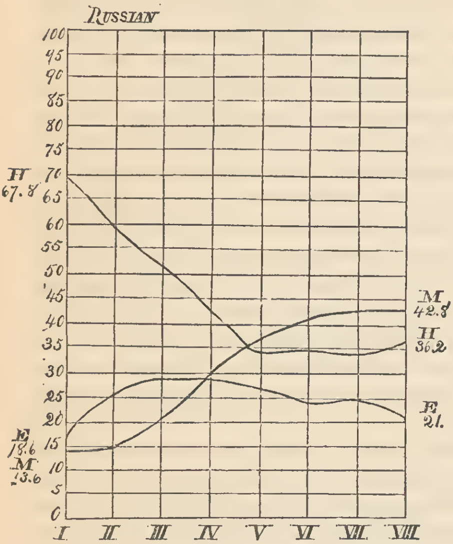
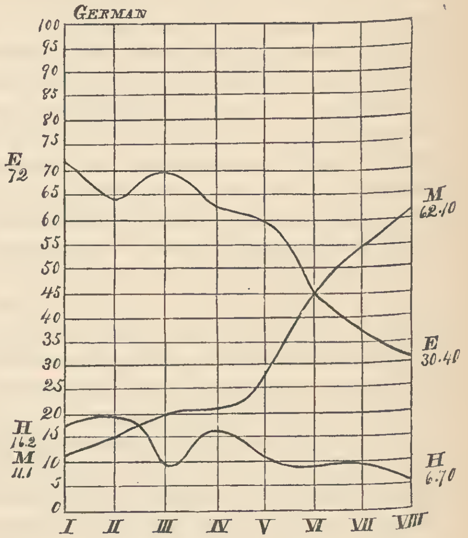


Table II.



Russian, German, and American, Tables I., II., III. The numbers at the foot of each chart represent the progression of the classes from left to right, that is, from the lowest to the highest, or, what is the same thing,

Table III.

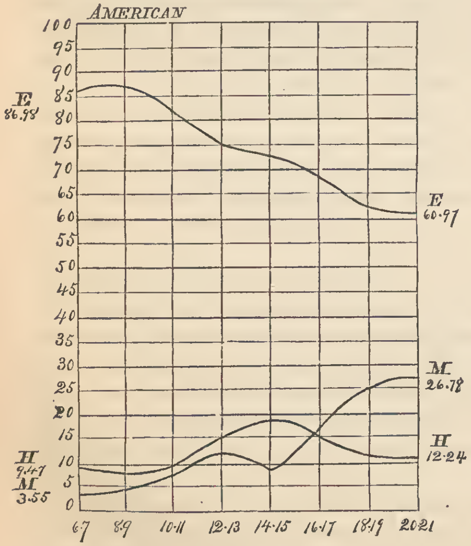
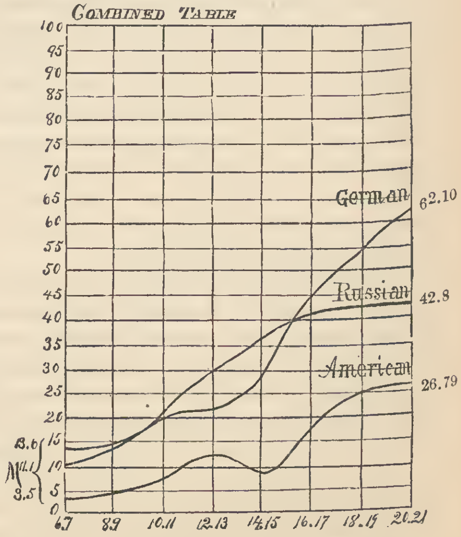


Table IV.



from the youngest to the oldest, as in the American diagram. This last is made necessary from the fact that the arrangement of the classes differs in this country from that in the foreign schools. Since, however, the successive classes correspond almost exactly to the successive ages, this can make no practical difference, especially as both the Russian and German statistics were calculated in both ways, and no material variation found between the results of the two methods. As it is the result of the entire school term, that is from six to twenty-one years, which now interests us, and which is the same in all countries, the comparison between the different nations is a just and fair one.

The numbers running longitudinally on the diagrams show the increase and decrease in percentage, each of the larger spaces representing ten, and each of the subdivisions five per cent. The line marked E represents the curve which emmetropia takes in the different years, M the myopic and H the hypermetropic curve. It will be seen on inspection, that, however much these curves differ in degree in the different diagrams, and amongst themselves, they all show that the refractive power increases with the advance in life.

Thus in the Russian diagram the frequency in which H occurs diminishes from 67.8 per cent. in the lowest class to 36.2 per cent. in the highest, while M increases from 13.6 per cent. to 42.8 per cent. Emmetropia increases a little, from 18.6 per cent. to 21 per cent. In the German[1] diagram, H descends from 16.2 per cent. to 6.70 per cent.; M rises from 11.1 per cent. to 62.1 per cent.; while E decreases from 72 per cent. to 30.40 per cent. In the American diagram, H rises from 9.47 per cent. to 12.24 per cent.; M rises from 3.55 per cent. to 26.79 per cent.; E sinks from 86.98 per cent. to 60.97 per cent. It will be seen from the Russian diagram that the emmetropia remains about the same through the entire series of classes, while the myopia steadily increases. If we assume that this increase, which is equal to 29.2 per cent., is due to emmetropia passing to myopia, we are forced to assume that the balance is maintained by hypermetropia passing to emmetropia to supply the deficiency. According to this diagram a part of the hypermetropia must become emmetropia, and if a hypermetropic eye can become emmetropic by increasing its refraction, it would be absurd to deny that it could not, by a little further increase, become myopic. So too in the German diagram it is seen that the increase in myopia, which equals 51 per cent., is greater than the decrease of emmetropia, which is 41.60 per cent., that is, is greater by 10 per cent. It must consequently have taken this increase from the hypermetropia, which is indeed seen to have decreased 10 per cent. From the American diagram it will be seen that the emmetropia decreases 26 per cent., while the myopia increases only 23 per cent., so that in this case the myopia may have been derived from the emmetropia alone. The slight increase in the hypermetropia may be explained by a small amount of latent H becoming manifest as the age advanced. The increase in refraction, however, though not so great as in the other two diagrams, is nevertheless very marked. It will, moreover, be seen in both the Russian and German diagrams that, while the lower grades of refraction E and H predominate

[1] Conrad also gives a separate curve, as determined with the ophthalmoscope. For the sake of simplicity and uniformity in the diagrams I have omitted this curve, and taken only those which were determined with glasses. The increase in refractive power is, however, more marked with the ophthalmoscope than with glasses. Still I cannot persuade myself that it is, on the whole, as accurate with the instrument as with test types.

in the lower classes, a complete change occurs, and M becomes the predominating refraction, and that this change takes place at a comparatively early age. Thus in the Russian diagram the line M crosses the line E at the fourth class, or what would correspond on the American diagram to the years 12–13. It crosses H at 14–15, when myopia becomes the predominating refraction, being in the highest class twice as frequent as the normal eye. Precisely the same thing occurs in the German diagram, where M, rapidly rising, crosses both H and E, being in the highest classes twice as common as the normal eye. In the American diagram, however, E remains all through by far the predominating refraction, the line E not only never crossing those of H or M, but remaining at a distance amounting at its lowest point in respect to M at its highest, to 34 per cent., and in respect to H at its highest, to 43 per cent.

This would show, if the numbers were great enough to prove it, and I think they are, that not only is there less myopia in this country in school children than in either Russia or Germany, but that emmetropia is the normal eye in all classes, which agrees with what Donders(32) found among the Dutch. For it may be assumed that after the age of twenty-one the refraction as a grand whole changes but very little, and that any slight change towards myopia in early adult life is more than counterbalanced by acquired hypermetropia in later life. Moreover, as we have taken as a standard the very class where myopia would be most frequent, that is in over-worked school children, it is fair to assume that the proportion of myopia would be less and not greater in other classes, especially in the lower and middle ranks of life. In order to make a better comparison between the three nations, as far as the myopia is concerned, the myopic curve of the three preceding tables has been transferred to a separate diagram, Table IV.

As an additional means of estimating the effect which the hereditary influence has on myopia, a comparison was made as to the frequency in which myopia occurred in the three principal nationalities of which our public schools are composed, that is, among the pupils of German, American, and Irish parentage. It was found that of all the German scholars 24 per cent. were myopic, of the Americans 20 per cent., and of the Irish 15 per cent.; so that even in this country, and under the same school influences, myopia occurs more frequently among the descendants of Germans than among either the Americans or Irish. The comparatively low percentage of myopia among the Irish is certainly remarkable. It is, however, in accordance with the assertions of various authors as to the immunity from myopia among the inhabitants of Great Britain. There can be no doubt that as a nation the Germans show a strong tendency towards myopia. Whatever may be the cause of the trouble, it is certainly a fact that the statistics of myopia taken in German schools show a decided increase over those taken in this country, which can be seen by comparing the statistics taken by Dr. Cheatham(33) in New York, by Dr. D. B. Williams(34) in Cincinnati, and Drs. Prout and Mathewson(35) in Brooklyn, with those taken in Germany. In comparing these statistics the different basis on which they are compiled should be taken into consideration, especially with those of Cohn, as he did not include myopia less than $\frac{1}{36}$, while in those cited above and made in this country $\frac{1}{60}$ was included.

From the evidence furnished by such statistics made in different countries and by different observers, I do not, while admitting the hereditary tendency, see how we can exclude the fact that there are many eyes

which, under prolonged tension of the accommodation, pass from hypermetropia to emmetropia, and thence to myopia with all its attending signs and symptoms, and this too in spite of hereditary influence to the contrary.

This factor of prolonged tension of the accommodation brings us at once to a consideration of the second proposition of our subject, which is whether posterior staphyloma and progressive myopia can be produced through the influence of the ciliary muscle. Before proceeding to discuss this, I would call your attention to the fact that the question does not refer to the entire act of accommodation, which implies the action of the recti muscles as well as that of the ciliary muscle, but is limited to the latter alone, and consequently I am desirous that it should be kept in mind that my remarks are confined to the share which the ciliary muscle alone takes as an agent in the production of myopia and the cone. There are two ways by which the ciliary muscle could render an eye previously not near-sighted, myopic. The first would be by increasing the curvature of the lens by active, that is, positive, contraction; and the second, by producing an elongation of the antero-posterior axis. That such an increase in curvature of the lens does, by contraction of the muscle, take place, with the effect of temporarily increasing the refraction of the eye, is now universally admitted. The only question which interests us here is whether the increased curvature of the lens through the agency of the ciliary muscle ever becomes permanent, and thus transforms an eye from an emmetropic or hypermetropic to a myopic eye. That such was the opinion of the earlier oculists I need not remind you, down even to the time when Cramer declared that there were myopic eyes in which the curvature of the lens corresponded to that in the normal eye when adjusted for near objects. Nor need I dwell on the revulsion of feeling which followed when it was learned from the labors of Helmholtz, Knapp, and Donders, that this last fact was not correct, and that the curvature of the lens in myopic eyes was shown to be if anything less than in the normal eye. A revulsion of opinion which culminated in Donders's(36) declaring, though formerly a firm believer in its occurrence, that he had never, since the true nature of refraction had been understood, been compelled to resort to an increased curvature of the lens as an explanation of myopia, while Giraud Teulon declares that there never has been a single well-authenticated case put on record.

The opinion that myopia is never due to increased curvature of the lens, has, however, been combated from the first by a few of the very best authorities, especially by Jaeger(37), who declares that "frequently the sole cause of myopia is the increased curvature of the lens," and by Stellwag(38), who in his last edition affirms that "increase in convexity of the lens is indisputably an important pathogenetic cause of short-sightedness;" and it cannot be doubted that this opinion, certainly as regards the initial cause of myopia, is gaining ground among the younger school. Still, notwithstanding the plausibility of the theory that increased curvature of the lens causes myopia, there is, it must be confessed, but little actual proof of its existence from such a cause. Indeed it must, I think, be admitted that the proof both experimentally and clinically points just the other way. Thus experimentation shows that by actual measurement the curvature of the lens is less if anything in a myopic than in a normal eye. Then comes the great clinical fact that in those cases of hypermetropia of a high degree, proved to exist in young children who are watched from year to year, the amount of the *total* hypermetropia does not decrease under

accommodative efforts, while the manifest does increase year by year in spite of such efforts. Thus the patient is compelled to go, not from stronger to weaker glasses, but from stronger to stronger, in spite of every effort to maintain the former amount of curvature of the lens. The very frequent occurrence, too, of adults with very high degrees of hypermetropia, who have been straining their ciliary muscle to the utmost for years to overcome their optical defect, is decidedly against the view that the curvature of the lens becomes permanently increased by over-tension; as is also the fact that in high degrees of hypermetropia where there is little or no manifest, the total is revealed by the ophthalmoscope, and then coincides with an amazing exactness with that produced by the full effect of atropine. Such facts as these, and many others might be quoted to sustain them, warrant us in the belief, until more convincing proof to the contrary is offered, that as a rule the action of the ciliary muscle has no effect in producing a permanent increase of curvature of the lens, and thus becoming a cause of myopia—no matter what the original condition of refraction. In regard to the second point in our investigation, that is, whether the ciliary muscle is capable of producing an elongation of the axis, thus causing myopia, it will be sufficient to call to mind that Young's(39) and Helmholtz's(40) investigations prove the impossibility of such a result.

But it may be urged—as, indeed, it has been frequently, especially since Dobrowolsky's(41) investigations—that, even if the natural, or what may be called the tonic, contractions of the muscle, do not produce myopia by directly increasing the curvature of the lens, spasmodic or clonic contractions do; and that the myopia first produced by increased curvature of the lens is followed by irritation of, and traction on, the deeper-seated membranes, which lead to true myopia—that is, to an elongation of the axis. If this were so, we should expect to find the signs of irritation more frequent where the strain of the ciliary muscle was the greatest—that is, in hypermetropic eyes. But it is an undeniable fact, as pointed out by Donders, and confirmed by every observer with the ophthalmoscope, that the injection of the nerve and the signs of irritation are not any greater than in the normal eye. Moreover, we should find, if simple traction on the choroid could produce the cone, that these would be most frequent where the traction of the muscle was the greatest; and this, too, independently of any increase of the length of the axis—that is to say, in hypermetropic eyes. For, if simple traction on the choroid could produce the cone, it would make no difference how short the axis was; all that would be necessary would be an increased action of the muscle, and this we get in hypermetropia. Now, not only clinical experience, but actual statistics, show, as has already been pointed out, that the crescent is found less frequently in hypermetropic eyes than any other. Moreover, if traction on the choroid could and did produce the cone, we should expect to find it in all eyes, no matter what their refraction, at the *inside* of the nerve, since this would be the place of all others in the circumference of the nerve where the traction would be soonest and most powerfully felt, for the simple reason that it is the shortest line between the two points of attachment. Moreover, the crescent would have a rapid tendency to become circular. But ninety-nine times out of a hundred the cone is at the outside of the nerve, and there remains.

But not only do we have positive proof in hypermetropic eyes that increased action of the ciliary muscle, *per se*, does not produce the cone,

but we have equally strong *negative* proof, in myopic eyes; for the greater the myopia, the less the positive contraction of the ciliary muscle. And many cases occur of the development of the cone towards adult life in eyes which were not at the time of development of the crescent using, nor had ever used, any active accommodation at all; that is, in eyes whose far point had always been from twelve to eight inches. Of all eyes these are the most prone, on over-use, to progressive myopia and the formation of the cone, while it is a clinical fact that, in many of these cases of rapidly increasing myopia, the myopia is at once checked by carrying out the far point by glasses which decrease the amount of convergence, but which increase sometimes to a great degree the demands on the muscle, especially where, as in young people, we completely neutralize the error in refraction. I do not mean to say that the cone is not due to irritation and traction, for I am firmly convinced that traction on the choroid in the plane of its extent, and pressure on its surface, are the principal if not the only causes of the cone. But what I do mean is that this traction takes place from extension at the posterior pole of the eye, not by the contraction of the ciliary muscle at the anterior parts.

It has never been demonstrated in the slightest degree that the ciliary muscle exerts the least traction on the posterior parts of the choroid. Indeed, the little experimental evidence which we have is directly opposed to such a supposition, as is indeed all the clinical evidence. There is, moreover, to say the least, a strong doubt whether the muscle exerts any traction even on the anterior portions of the choroid, and some of those who have been hitherto the most enthusiastic supporters of the theory of the meridional portion of the muscle being a direct "tensor choroideæ," seem to have modified, if not completely changed, their views. Thus Iwanoff (42) latterly, after describing the anatomy of the meridional portion, and its mode of insertion into the *L. suprachoroidea*, says: "Now, with such a disposition of the meridional portion of the muscle with the *L. suprachoroïdea*, and with such a structure of this latter, it is self-evident that the deep layers of the choroid (choriocapillaris and middle choroid) cannot be essentially stretched. Consequently, the entire effect of the contraction of the ciliary muscle will limit itself to an extension of the ciliary body, which, as is known, is in the closest connection with the zonula of Zinn, while in the choroid itself, at the most, there will be only an extension of the L. suprachoroidea. This is perfectly evident when we bear in mind that that part of the choroid which is situated behind the *ora serrata* has no direct connection whatever with the zonula." Nevertheless, that enforced and maintained contraction of the ciliary muscle should produce that nervous exhaustion and irritation, with altered nutrition, which always occurs in the case of an overtasked muscle, is most natural. That this irritation should extend to the surrounding parts, causing increased vascularity, and, what is more, increased secretion, by which the intraocular pressure is augmented, might, I think, occur to some degree, in spite, too, of the assertion of Leber that the vascular supply to the ciliary muscle is so arranged that the circulation is not impeded by its contraction. Still, from what has already been stated, I am inclined to believe that this increased intraocular pressure, which is so potent a factor in the production of myopia, occurs much more frequently from other causes, especially from faulty convergence, than from the simple action of the ciliary muscle. This leads me to believe, although I appreciate most highly the labors of

Dobrowolsky,[1] that spasm of the ciliary muscle, so well recognized and described by the elder writers, has been somewhat exaggerated by the younger school both as to its frequency and the amount of influence it exerts in the production of myopia.

I have not the time to go into an extended discussion of this question, which it surely deserves, and must therefore content myself by pointing out the fact that it would appear that two important conditions have been ignored in the works of Dobrowolsky and his followers, Schiess-Gemuseus(43) and Dr. Derby(44) of Boston. The first is the physiological effect of atropine, which is to reduce the refraction to an amount which, according to Donders(45), would be expressed by $+\frac{1}{60}$, or even $+\frac{1}{40}$. The ordinary emmetropic eye then under atropine would be a hypermetropic eye of $\frac{1}{60}$. Are we to assume, then, as Dobrowolsky does, that a decrease of refraction which amounts to only $\frac{1}{360}$, $\frac{1}{240}$, $\frac{1}{120}$, or even $\frac{1}{96}$, is the result of spasm, and not the action of the drug, when the physiological action is often equal to six times the amount; and especially are we to assume this when solid atropine has been used three or four times a day for three or four weeks, or even months?

Dobrowolsky(46) gives a table of 105 cases of myopia. In 69 of these there was a decrease after the use of atropine—that is, in seventy per cent. But of these cases, when we make an allowance for the physiological effect, only thirty remain. Of these thirty, after this allowance, six show a decrease of $\frac{1}{240}$ or less, twelve of $\frac{1}{120}$ or less, seven of $\frac{1}{60}$, three of $\frac{1}{40}$, and two of $\frac{1}{15}$ or less. So that there are only five cases in the entire number which show a moderate degree of spasm, and only two of these a high degree; and in all these five cases the myopia is of an excessive grade, varying between $\frac{1}{3.3}$ and $\frac{1}{2.2}$. Now, these high degrees of myopia are just the conditions in which we always expect, whether there be any insufficiency or not, an apparent increase in the refraction, caused by an excessive muscular strain at convergence. This is invariably accompanied with a displacement of the relative accommodation inward, and the slight temporary decrease in the refraction caused by the use of strong atropine invariably returns as soon as the atropine is left off, unless the strain on the convergence is relieved by suitable means. Exceedingly interesting in this connection are some of the results published by the earlier writers—Bonnet, Cuvier, Philips, and Jules Guérin—but particularly the case related by Giraud Teulon(47), of extreme myopia, in which tenotomy of the external rectus suddenly diminished the myopia by $\frac{1}{13}$, or from $\frac{1}{5}$ to $\frac{1}{8}$. Had this amount of decrease in refraction been brought to light by the use of atropine, it would certainly have been put down by these observers as due to spasm; and it just as certainly would have returned the moment the atropine had been discontinued, unless the strain on the interni had been removed. The analogue of this, the sudden development of manifest H, which had been previously latent, after tenotomy, is too common an occurrence to need any comment whatever. It strikes me, therefore, that want of attention to these two points, namely, the physiological action of atropine and the associated action of the recti interni on the accommodation, has led these observers into a false estimate of the frequency of spasm, and its predominating influence in the production of myopia. Certainly my own

[1] I cannot agree with a recent writer, who says: "To Professor Schiess belongs the credit of utilizing the researches of Dobrowolsky for ophthalmic practice." Whatever there is of merit in this matter belongs, both theoretically and practically, entirely to Dobrowolsky.

experience does not coincide with that of these observers, and I fail to find a frequent or great diminution, even in myopic eyes of young children, after the use of atropine. This I was glad to see had been the experience of Donders(48), as expressed by him at the last International Congress.

Up to this point we have been occupied with a consideration of the action of the ciliary muscle as a whole, but a theory has been started that contraction of the muscle can take place in the direction of one meridian alone, or in different meridians in different degrees at the same time. Thus, according to Dobrowolsky(49), a complete interchange of kind and degree of astigmatism, as well as change of direction in the axis, may result from these meridional contractions, so that an eye which was originally hypermetropic in one meridian may by a counterbalancing contraction of the muscle become an emmetropic eye, or an emmetropic eye an astigmatic eye, with M in one meridian alone, or M in all meridians, and more pronounced in one than the rest; and so on with a change in all the varieties of astigmatism which could be produced through positive accommodation. It is claimed that this is a fertile source of myopia.

That a slight change in a meridian, whether of refraction or direction, should take place, would not appear strange; for there is nothing absolutely stable in the human body. This would, moreover, be in accordance with what has been supposed possible by many and corroborated in some degree by certain investigations made by Woinow(50). But any such remarkable changes as are pointed out by Dobrowolsky, either in kind or degree, I have never met with myself, nor do I know of any sufficient corroboration of them by others. For this reason I do not feel warranted in expressing an opinion in regard to them; certainly not one which would imply a belief as to their active agency in producing myopia.

These views of Dobrowolsky were followed by those of Dr. Thomson(51), of Philadelphia, based on the old theory of the antagonistic action of the two portions of the ciliary muscle. According to these views, not positive accommodation alone, as with Dobrowolsky, but even negative accommodation took an active part, through the contraction of the radial fibres of the ciliary muscle as opposed to the circular fibres. Thus a defined traction in a meridional direction was brought to bear on the choroid, which would naturally exert its influence at a corresponding point at its attachment at the circumference of the disk. This would produce the cone, which would then lie in the plane and direction of this traction.

I can only say in regard to this that our present knowledge of the action of the ciliary muscle does not support this conclusion. For notwithstanding the many attempts to prove the fact of negative accommodation, ever since the discovery of the ciliary muscle, not the slightest trace of its existence has ever been demonstrated; while on the other hand the fact of its non-existence has been, it would seem to me, incontestably shown. Until, then, the existence of negative accommodation has been proved, it would be a waste of time to speculate on what would be its effect on myopia, or rather what its effect is. We have no right to assume both the cause and effect too. But admitting the existence of negative accommodation to the fullest degree, even to the extent that it surpasses the positive, many facts would have to be explained before its effect on the cone could be admitted. First, why is it that we see the

highest degrees of myopia with immense cones, and either not a trace of astigmatism, or, if present, of such a very low degree as to preclude the idea that the cone is the result of the astigmatism? Secondly, why is it that we see such very high degrees of simple myopic astigmatism without a vestige of a cone? Thus in the past few months I have seen the following cases where there was no trace of a crescent: one case of $\frac{1}{5}$ c., two of $\frac{1}{6}$ c., one of $\frac{1}{7}$ c., two of $\frac{1}{8}$ c., one of $\frac{1}{10}$ c., three of $\frac{1}{11}$ c., and four of $\frac{1}{12}$ c.; all simple, myopic astigmatism. Thomson(52) himself has reported some cases of excessive asymmetry of the cornea, in which, to use his own words, "it is worthy of remark that in this last case, with such high degree of myopia and the use for years of glasses so unsuited, there was not the slightest appearance of the crescent at the optic disk." Again, it would have to be shown why it is that the cone sometimes runs in the same direction as the meridian of greatest curvature, and sometimes in that of the least, or again in neither. Then, too, if traction in one meridian had anything to do with the direction of the cone, why is it that it is limited so often to the outside of the nerve, and does not extend equally in the same direction on the opposite side? since it can hardly be assumed that these meridional contractions take place only in one-half of the ciliary muscle, and this, in the vast majority of the cases, the outer half.

We should have also to explain why it is that in sections of myopic eyes we find the choroidal pigment layer drawn away from the outer edge of the nerve and *over* the inner edge. In regard to the appearances of traction upon the vessels, and other details of the fundus as seen with the ophthalmoscope in astigmatism, I would say that this, as far as my experience goes, corresponds to the distortion produced by the optical defect of the refracting media, which causes the line of traction to appear to be always in the meridian of greatest curvature.

This brings me to the close of my remarks, from which I would in keeping with the condensed character of my paper make the following brief conclusions:—

I. From the fact that there is so large a percentage of children who are myopic, but whose parents are not near-sighted, while the myopia increases directly with the amount of increased tension of the eyes, and from the fact that an interchange of refraction may occur, whereby an eye which is not congenitally myopic may become so in spite of hereditary tendency, it would seem to follow that hereditary predisposition, though undoubtedly a potent cause, is not only not the sole cause, but is not even the predominating cause of myopia.

II. In regard to the second question, as to whether the ciliary muscle acting through faulty refraction can produce myopia and the cone, I conclude that the action of the ciliary muscle, taken by itself, exerts but little influence on the production of myopia, and still less on the formation of the cone.

I cannot refrain before closing from asking you once more to bear in mind that the questions submitted were not as to what the causes of myopia were, but simply the influence of two fixed and defined factors in its production. The briefest consideration of these two subjects which I could possibly make, has already led me beyond the limits of the time allowed, and I fear also of your patience. I have for these reasons been compelled to forego saying much which I am conscious that a just consideration of the subject would demand, and to discard much that I had already written.

Bibliography.

(1) Lucas, P. Traité philosophique et physiologique. Tome i. p. 391, 1847.

(2) Funari. Annales d'Oculistique. Tome x. p. 145.

(3) Piorry. Heredité dans les Maladies. p. 120.

(4) Portal. Considération sur les Maladies de Famille.

(5) Stellwag von Carion. On the Eye. American Transl., 1873, p. 354, p. 703.

(6) Jaeger. Einstellungen des diopt. Apparates, u. s. w., 1861. S. 71.

(7) Donders. Accommodation and Refraction of the Eye, 1864, p. 350.

(8) Cohn, H. Untersuchungen der Augen von 10,060 Schulkindern, u. s. w., Leipzig, 1867. S. 63.

(9) Ibid., p. 129.

(10) Erismann, F. Ein Beitrag zur Entwickelungs-Geschichte der Myopie. Græfe's Archiv, Band xvii. Ab. 1. S. 1.

(11) Stellwag. Die Accommodationsfehler des Auges, 1855. Wien Sitzungberichte der mathem-naturw. Classe der kais. Akademie der Wissenschaften, Bd. xvi. S. 225.

(12) Jaeger. Einstellungen. S. 27.

(13) Ibid., 37.

(14) Donders. Accommodation and Refraction, p. 354.

(15) Græfe. Archiv für Ophth., 1854, B. 1. S. 394.

(16) Donders. Accommodation and Refraction, p. 443.

(17) Cohn, H. Untersuchungen. S. 61.

(18) Conrad, Max. Die Refraction von 3036 Augen von Schulkindern mit Rücksicht auf den Uebergang der Hypermetropie in Myopie. Inaugural-Dissertation. S. 38.

(19) Jaeger. Einstellungen. S. 37.

(20) Iwanoff. Græfe's Archiv, B. xv. Ab. 3. S. 284.

(21) Donders. Accommodation and Refraction, p. 384.

(22) Ibid., p. 349.

(23) Jaeger. Einstellungen. S. 104.

(24) Stellwag. On the Eye. American Transl., 4th Edition, p. 723.

(25) Ware, J. C., F.R.S. Observations Relative to the Near and Distant Sight of different Persons. Phil. Trans. Royal Soc., 1813.

(26) Szokalski. Prager Vierteljahrschrift für pract. Heilkunde, 1848.

(27) Schürmayer, J. H. Handbuch der Medicin Policei. S. 61, 1856.

(28) Jaeger. Einstellungen. S. 20.

(29) Ruete. Untersuchungen über die Augenkrankheiten bei Schulkindern. Zeitschrift für Medicin, Chirurgie, u. s. w. Neue Folge B. v. Heft 4. Leipzig, 1866.

(30) Erismann. Græfe's Archiv, Band xvii. Ab. 1. S. 13.

(31) Conrad, Max. Inaugural-Dissertation, Könisberg. S. 16.

(32) Donders. Accommodation and Refraction, p. 341.

(33) Cheatham. Boston Medical and Surgical Journal, May 27, 1875, p. 633.

(34) Williams, D. B., Ibid.

(35) Prout and Mathewson, Ibid.

(36) Donders. Accommodation and Refraction, p. 394.

(37) Jaeger. Einstellungen. S. 26.

(38) Stellwag. On the Eye, 4th Edition, American Transl., p. 704.

(39) Young, Thos. Mechanism of the Eye. Phil. Trans., 1801.

(40) Helmholtz. Karsten's Encyclopædia der Physik, 1856.

(41) Dobrowolsky. Beiträge zur Lehre von den Anomalien der Refraction, u. s. w. Zehender klin. Monatsblätter. Ausserordentliches Beilageheft, 1868.

(42) Iwanoff, Græfe, und Sämisch. Handbuch der Augenheilkunde, Band i. Theil. l. S. 277.

(43) Schiess-Gemuseus. Beiträge zur Therapie der Myopie, Basel, 1872.

(44) Derby, H. Trans. American Ophth. Soc., 1874, p. 138.

(45) Donders. Accommodation and Refraction, pp. 174, 600.

(46) Dobrowolsky. Zehender ausserordentliches Beilageheft, 1868. S. 37.

(47) Teulon, Giraud. Ophth. Hosp. Reports, vol. v. p. 381.

(48) Donders. Report of fourth International Congress, London, 1872, p. 69.

(49) Dobrowolsky. Græfe's Archiv, xiv. Ab. 3. S. 50.

(50) Woinow. Græfe's Archiv, xv. Ab. 2. S. 167.

(51) Thomson, Wm. Trans. American Ophth. Soc., 1874, p. 132.

(52) Thomson, Wm. Trans. American Ophth. Soc., 1875, p. 310.

Discussion on Dr. Loring's Paper.

After the reading of the preceding paper, Dr. J. Green, of St. Louis, said:—As bearing on the question of the influence of hereditary transmission, we must remember that we scarcely ever see myopic children whose parents and grandparents have been equally subjected to the general exciting causes of myopia. Taking, therefore, into account this absence of exciting causes in the case of the ancestors, it will not do to assume the absence of hereditary predisposition to myopia even where there has been no declared myopia in parents and grandparents. Again, in all our published statistics there is great imperfection as regards the recognition of astigmatism, and yet among the causes which produce indistinctness of vision, and thus favor the development of myopia, astigmatism certainly holds an important place. The statistics are thus of less value than many suppose, in clearing up this part of the subject. It is a very significant fact that in such cases of myopia as give the patient trouble enough to lead him to consult an oculist, we find astigmatism oftener present than absent; hence, in investigating the causes of myopia, we must make more careful studies of the refraction, not merely with reference to myopia and hypermetropia, but also with reference to astigmatism.

Dr. W. Thomson, of Philadelphia, said:—I think that Dr. Green has called attention to an important fact in alluding to the large number of cases of astigmatism which complicate myopia. That myopia is very often progressive, we know, but I think from observation that cases of myopia, pure and simple, are hard to find. High grades of myopia are, as a rule, characterized by astigmatism, and therefore we ought to consider whether most cases of propressive myopia have not commenced as slight cases of astigmatism. If we accept the conclusions of the paper, the question of progressive myopia would be hopeless, and we would have nothing to do but to look on and see people growing worse day by day. If it is conceded that astigmatism is often an efficient cause of myopia, we have then a remedy in cylindrical glasses. I think that astigmatism is one of the active causes of progressive myopia.

Dr. Loring said:—I would call attention to the fact that astigmatism was not mentioned in the question for discussion, but I admit that it has something to do with myopia. Dr. Thomson takes a gloomy view of the subject. I believe that, if we go on as in Germany, the time will come when myopic refraction will predominate. In keeping children in school twelve hours a day, poring over books, a great hygienic principle is overlooked, and, as long as

this is done, so long will there be myopia. I cannot agree that the time will ever come when myopia will be prevented by the use of cylindrical glasses.

Dr. H. W. Williams, of Boston, said:—Dr. Loring says justly that myopic astigmatism is a form of myopia. I think it is conceded that hypermetropia and astigmatism are hereditary. I know several instances in which persons are myopic in one eye, and emmetropic or hypermetropic in the other, and I would ask if there is a hereditary predisposition to myopia in one eye, and not in the other? And I have seen such cases, in which at a later period the myopia has increased, and in which the hypermetropic or emmetropic eye has become myopic. I am satisfied that myopia is largely due to artificial causes.

The President, Mr. R. Brudenell Carter, of London, said:—I remember the case of a young lady who was brought to me with a low degree of hypermetropia in one eye, and a high degree in the other. In the case of her father and mother both eyes were hypermetropic. I do not believe that statistics will enable us to arrive with certainty at any conclusion upon this subject. In looking back I can recall a few cases of myopia which were the result of accident. I remember at least one case in which this condition was produced by a blow.

Dr. Thomson said:—With regard to Dr. Loring's second conclusion, I think that the whole question resolves itself into whether or not the ciliary muscle is a single or a double muscle, and that unless new light is thrown on the subject by an appeal to clinical observation, there is nothing to do but to accept this conclusion. I am convinced that the changes in the back part of the eye in myopia have something to do with the existence of astigmatism, and that the appearances which they assume are largely dependent upon the direction of the principal meridians of the astigmatic eye. If a conus is found in any part, there will be found to be a relation between the direction of the conus and that of the cylindrical glass which gives the highest vision. I believe that I have established the fact that in many cases of myopia of high grade, say one-fifth, one-sixth, or one-seventh, in which neutralizing spherical glasses have been habitually worn, the optic nerve is free from distortion, and has no displacement, but that when astigmatism, even of low grade, is found conjoined with a high degree of myopia, a conspicuous conus will be found.

Dr. E. Williams, of Cincinnati, said:—I have seen several cases which I may cite in confirmation of Dr. Thomson's remarks. I remember the cases of a distinguished lawyer and of a merchant, both over fifty years of age, who had worn glasses since they had left school, and in neither of them was conus found. When we find a patient wearing the proper glasses, and seeing perfectly with them, we rarely find a conus, but when vision, even with the glasses, is more or less indistinct, we may expect to find the conus.

Dr. S. D. Risley, of Philadelphia, said:—I think that we rarely see progressive myopia with conus without astigmatism, and, on the other hand, that conus is not confined to myopic eyes, but is a very frequent occurrence in hypermetropic eyes with astigmatism. I recall two cases in which conus and myopia had appeared while under observation, and were attended by marked asthenopia which was relieved, and further choroidal change arrested, by the rest afforded the ciliary muscle by a carefully adjusted, correcting glass. I am convinced, from clinical experience, that the ciliary muscle has much to do with progressive myopia and the formation of the conus.

Dr. Thomson said:—I know of a number of cases of asthenopia from insufficiency of the internal recti, which bear upon this question. In one case, in which the asthenopia had existed over a period of thirty years, and had caused much distress, and in which the asthenopia was perfectly relieved by the use of proper prismatic glasses, there was absolutely no trace of conus in either eye.

Dr. Loring said:—The use of concave glasses often stops the development of the conus and the myopia, and, nevertheless, the use of the glasses forces the accommodation more than when they are not employed. All admit that concave glasses are useful in myopia, yet concave glasses act directly upon the ciliary muscle. That the ciliary muscle has two antagonistic functions is, to say the least, extremely improbable.

ON THE RELATIONS BETWEEN CORNEAL DISEASES AND REFRACTIVE LESIONS OF THE EYE.

9

BY

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Of the various structures of the human body, few have been subjected to such thorough, critical, and we may add profitable, examinations in respect to the histology and pathology of their tissues, as has the cornea. The names of many of the most learned and illustrious investigators are inseparably connected with this small structure, and the extent and value of their researches indicate its importance. Yet it must be conceded that of the advantages derived from the histological and pathological studies of this part, comparatively few have reverted to the structure itself, and candid observers must agree that in the etiology of corneal diseases the advance has not been in proportion to the added knowledge of its structure and pathology.

A review of the history of the etiology of corneal diseases would show that, since writers of half a century ago, after allowing for the agency of mechanical and chemical injuries and the pressure incident to certain forms of conjunctival disease, united in ascribing the cause of corneitis to scrofula and arthritis, few important advances have been made in determining the causation of these diseases. To the genius of Mr. Hutchinson we owe the discovery that in a clearly defined class of cases of corneal disease, the cause may be found in the diathesis peculiar to inherited syphilis, and that if we would best meet the indications for treatment in these cases we must directly attack the inherited taint. But the proportion of such cases is not large, and there remain after excluding those which arise from mechanical pressure, injuries, chemical irritations, and a few other exceptional causes, the very large residuum of cases which by nearly all authors are regarded as manifestations of struma.

Of phlyctenulæ of the cornea, the form of corneal disease which the surgeon most frequently encounters, a learned French writer of recent times[1] says that the pustules are the first and the most frequent manifestation of scrofula; they are, he says, benign scrofulous exudations. Although this view is not fully accepted by some modern writers, it meets with but the mildest opposition, and the doctrine that the prevailing cause of corneal diseases and especially of phlyctenulæ is struma, is, I believe, the one generally accepted. The truth is that the condition called struma is far more frequently developed by corneal diseases than are corneal troubles developed by struma. One of the most positive evidences of the existence of struma, is supposed to be found in the presence of enlarged cervical glands, but the enlargement of these glands does not usually precede the corneal disease, but is its result.

The fact, well known to ophthalmic surgeons, that patients who have been cured of corneal diseases are quite liable to return after a few months,

[1] Bazin, Leçons sur la Scrofule.

or even weeks, with renewed attacks of the disease, showing conclusively an inherent tendency to such troubles, led me a year ago to seek for the cause of the tendency. The excessive nervous irritability, photophobia, and muscular spasm, and the general failure of nutrition in these cases, induced me to regard corneal diseases, especially of the phyctenular form, as a manifestation of functional nervous derangement. The fact of the easy cure and the frequent recurrence of the diseases strengthened this opinion, and as I had at that time been led to the belief that many functional nervous diseases owed their origin to anomalous refraction of the eyes, I turned to the eyes themselves to seek for the cause of irritation. A few careful examinations satisfied me that a more extended search should be made in this direction, but the difficulties encountered in ascertaining the refractive condition of the eyes of patients suffering from corneitis, especially from phlyctenular corneitis, are great; the intolerance of light, the youth of the patients (for phlyctenular corneal troubles are most frequently found in children from two to eight years of age), and their general irritability, all contribute to render such examinations tedious to both patient and surgeon. To these difficulties I attribute the fact that the well-known maxim among ophthalmic surgeons, to test the refraction of all diseased eyes, has been so far disregarded; for the practitioner whose time is fully occupied by the usual duties of his profession, will find the attempt to examine a large number of these patients in this respect a severe tax upon his time and strength.

I cannot better illustrate the result of my search in this direction than by relating the history of the following case. The first part of the history well illustrates the oft-repeated experience of every ophthalmic surgeon, while the second part will illustrate my own experience in several cases since my attention has been specially directed to this subject.

L. R., a bright boy seven years of age, was, about two years ago, brought to me, suffering from phlyctenular ulcers of both eyes. He had, previous to the eye trouble, enjoyed excellent health, had passed through the usual experienee of childhood in respect to infantile diseases with safety and ease, and was regarded as a perfectly healthy and robust child. There was no history of syphilis or of scrofula in the family of either father or mother, both of whom were perfectly healthy people. Treatment for a few days by means of atropine and stimulants applied to the mucous surfaee of the eyelids, sufficed to effect a speedy cure of the ulcers, and the boy remained well for about three months, when he passed through a similar experienee which was repeated a third and a fourth time within a little more than a year. At the close of the seeond attack, I observed that the cervical glands were swollen as they had not been previously, and with each attaek there was more general irritability and more evidence of defective nutrition. During an attaek about a year sinee, his mother called my attention to a defeet in her own eyes, which, on examination, I found to consist in hypermetropie astigmatism of $\frac{1}{30}$, and whieh indueed me to test the refraction of the boy's eyes. I found hypermetropia $\frac{1}{30}$, without astigmatism, and advised that he be required to wear + 42 glasses when engaged in studies or in playing with toys. My advice was accepted, and the boy's eyes have sinee remained well, and with the correetion of the refractive evil the so-ealled strumous symptoms disappeared.

The following case, which has been reported in another connection, will illustrate the relations existing between this and another form of manifestation of nervous trouble.

Mr. F. A. R., of Minnesota, consulted me in September last in regard to his little daughter's eyes. She was a bright girl of ten years, with a defect in her

speech, and was evidently suffering from chorea. On account of her nervous condition she had been kept out of school for more than a year. The trouble for which I was consulted was the presence of a number of small ulcers at the border of the cornea. On testing the refraction, I found astigmatism requiring for its correction a + 36 cylinder. Glasses were procured, and after brief treatment for the ulcers, resulting in their cure, the father returned with his child to his western home. In the latter part of March, of this year, he again presented the child for treatment, she being again subject to corneal ulcers. I learned that from the time of leaving Albany the child's health had greatly improved, and her nervous symptoms had disappeared. A month or two previous to the last visit, however, her glasses were broken, and she was without them for some time. The nervous symptoms began to return, and the corneæ were again affected with ulcers.

In looking over the records of one hundred and fifty cases of corneal disease in which the refraction has been examined with as much accuracy as the difficulties necessarily encountered would permit, I find that in 80 per cent. there have been serious refractive lesions. Even in two cases which were typical of the physical conformation so clearly described by Mr. Hutchinson as characterizing the offspring of syphilitic parents, a high degree of hypermetropia was found after the infiltrated corneæ had become sufficiently transparent to allow of a determination of the refractive condition, and it is interesting to remember that the flattened facial features described by Mr. Hutchinson are those recognized by ophthalmologists, and so well described by Prof. Donders, as indicative of hypermetropia. In these cases, as in all others in which the refraction has been determined, after the cornea has been affected, allowance must of course be made for changes in the form of the cornea which may have resulted from disease, but as in each of the cases alluded to a high degree of hypermetropia was found ($\frac{1}{18}$ and $\frac{1}{14}$) the defect could hardly be ascribed to this cause.

In my efforts to determine the refraction in these cases, I have often been obliged to be satisfied with an examination of the unaffected eye by means of test letters, or by the ophthalmoscope alone. In several instances the examinations have been made after recovery from the disease. Among the refractive errors, hypermetropia seems to rank first as a cause of corneal affections; second, astigmatism; and third, unequal degrees of myopia. A larger number of cases than I have examined would be necessary to determine the relative frequency of these conditions. Simple myopia of equal degrees in the two eyes, I have not found except in young children in whose cases the refraction has been determined by the ophthalmoscope. In these cases, I suspect an astigmatism not recognized by this means of examination.

It is important to notice two conditions which are often associated with corneal diseases: First, the herpetic eruption which is so common in phlyctenular corneitis; and second, an inflamed condition of the free border of the eyelid. The herpetic eruption which appears about the lips, cheeks, and nose, in cases of phlyctenular corneal troubles, is conceded to be the result of irritation of branches of the fifth nerve. That it is due to the reflex irritation from the ciliary nerves, I think it reasonable to suppose, for I have often seen these herpetic eruptions about the cheeks, nose, and lips, disappear quickly after correcting the refraction of the eye, and with no further treatment. The blepharitis would also in most instances seem to be one of the manifestations of reflex irritation from the same cause, as I have long observed; and the

swelling of the lids, which is of itself sometimes the immediate cause of corneal lesions, is, I believe, one of the various results of the peripheric irritation beginning in the ciliary body.

That a reflex nervous irritation, like that arising from the strain upon the ciliary muscle in erroneous refraction, can interfere in the nutrition of the cornea, there can be no doubt; and, if we watch the course of a corneal phlyctenula, we shall see that nutrition is disturbed first by irritation of the vaso-motor nerves of the conjunctiva; for here we observe, before the appearance of any efflorescence of the cornea can be discovered, the fasciculus of red vessels, and indeed, if promptly met by appropriate treatment, this can usually be made to disappear before the herpetic pustule is developed at all.

May it not be a mistake to regard the herpetic pustule as the centre of irritation around which the vascular injection of the conjunctiva forms an objective expression? Were this the case, would not the pustule appear first, whereas, frequently, it is entirely aborted? Is it not more probable that the condition of the vessels in this congested fasciculus, is such as to clog the passages through which nutritive material should pass to supply certain portions of the cornea, and that the portion in which nutrition is most defective, suffers first from this interference?

SECTION ON OTOLOGY.

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THE IMPORTANCE OF TREATMENT OF AURAL DISEASES IN THEIR EARLY STAGES, ESPECIALLY WHEN ARISING FROM THE EXANTHEMATA.

BY

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The object of selecting the subject of this paper for discussion by a body of men interested in otology, must be, not to draw out the different opinions of otologists on the subject (for there can certainly be but one opinion), but to have the attention of the profession at large directed to the importance of early treatment in aural diseases. The pleading of a single individual would perhaps fail to carry conviction to the minds of many members of the profession, while the same statements, made by the same individual acting as the mouth-piece of such a body of skilled men as is here gathered together, may be likely to receive the thoughtful consideration which the subject demands.

In discussing this subject, I shall take into consideration chiefly the acute, purulent affections of the middle ear, as it is in these that judicious treatment proves so beneficial, and the want of it at times so disastrous. In catarrhal affections it might also be shown that treatment in the early stages of the trouble was in general productive of very satisfactory and marked results, while in the later stages it rarely proved of any benefit whatever. It will be sufficient, however, for my present purpose, to make use only of those acute affections of the middle ear which naturally end in the formation of pus, and which very often become chronic.

The question of the frequency of chronic otorrhœa, is a difficult one to determine with any degree of accuracy. Owing in part to the prevailing popular belief that it is dangerous to arrest a discharge from the ear, and in part to the answer which very many physicians give to the parents of

such patients—" your child will outgrow it"—hundreds upon hundreds of individuals never present themselves for treatment either in private (aural) practice or in the clinics of our public institutions. Hence we have no means of finding out accurately how large a percentage of the population of any given community is afflicted with this morbid condition. We know, however, that these cases constitute from twenty to thirty per cent. of all cases of ear disease that come under the aurist's care, either in hospital or in private practice. We also know that among our acquaintances a certain number are troubled, and have been perhaps for years, with a discharge from one or both ears. Again, if we inquire how the disease originated, we find that, in a large proportion of the cases, it is referred back to an attack of scarlet fever, or measles, or nasal catarrh. In this way we get hold of two important facts: first, that chronic discharge from the ear is a common disease; and, second, that taking cold, and the exanthematous diseases, are the chief sources from which it originates. Two questions here present themselves: (1) Is a chronic discharge from the ear a sufficiently serious affection for either the physician or the patient to give much thought to it? and (2), admitting the serious nature of the disease, can anything be done to diminish the prevalence of such chronic otorrhœas?

Satisfactory answers to these two questions will furnish the best elucidation of the title which stands at the head of this paper.

I. *Is Chronic Discharge from the Ear a serious Affection?*—I answer unhesitatingly, yes. In the first place, it is annoying to the patient and disgusting to those who associate with him. In the next place, it is an indication of the existence of a process which may at any time seriously threaten the patient's life. Here, again, statistical proof of the correctness of my statement is not easy to furnish. Medical literature contains numerous accounts of cases of chronic otorrhœa that have terminated fatally. Almost every practitioner, too, has met with one or even several such cases in the course of his practice. Many also have seen such cases terminate fatally under the form of meningitis, and have never once thought of the part played in the disease by the chronic otorrhœa. These are well-known facts, and admitted, I think, by all. But how does an inflammation of the ear pass into the condition of a chronic otorrhœa, and how may the latter cause the death of the patient? To render this clear, let me narrate to you the probable history of such a case.

A strong, healthy boy, ten years old, is taken ill with scarlet fever. The disease runs a moderately severe course, and bids fair to subside without any serious after-effects. The tonsils are red and swollen, and the entire naso-pharyngeal region is evidently in a markedly inflamed condition. While in this state—say in the second or third week of the disease—the boy begins to complain of an earache. The first night, warmth is applied to the ear, and in this way the boy gets some temporary relief. The next day, however, the pain continues, and the family physician, who perhaps by this time has discontinued his daily visits, is consulted as to what shall be done to relieve the boy of his earache. Laudanum is prescribed; a drop or two of it to be instilled into the ear, and a piece of cotton-wool saturated with the remedy to be inserted into the outer canal. This too, perhaps deadens the pain a little, but still the boy passes a second night in suffering, and the third day finds him steadily growing worse. By this time the earache proper has become merged in a painful and throbbing sensation, involving not only the ear itself, but

also the entire side of the head, and especially the mastoid and occipital regions. The instillation of laudanum is repeated, and perhaps sweet oil is added. The third night is passed in miserable suffering, the boy obtaining a few snatches of sleep by the aid of opium administered internally. On the fourth day—a large blister in the mean time having been applied behind the ear, only to add to the boy's discomfort—a slight discharge makes its appearance at the outer orifice. As this discharge becomes more copious, the pain subsides, and in the course of a few days the patient finds himself able again to eat, sleep, and move about as when he was well. For a year or more afterwards he remains free from serious pain in the ear; the discharge, however, persists, and at times there is a sense of discomfort in the ear, perhaps not amounting to actual pain, but still sufficient to indicate the existence of a tolerably active inflammatory process. Finally, the patient some day contracts a bad "cold;" the otorrhœa ceases, the pain in the ear and corresponding side of the head becomes intense, the parts behind the ear become very painful and tender, there is fever—in a word, the inflammation of the tympanum has spread to the adjacent bony structures and to the meninges of the brain, and death soon follows. Death may also follow in other ways, but for our present purpose it is not necessary to describe other modes of fatal termination.

Now, to appreciate clearly how dangerous a disease an inflammation of the middle ear may become, and to understand fully the *rationale* of a successful treatment, it is necessary that one should possess a tolerably clear idea of the anatomical relations of the middle ear. These are, in brief, the following:—

The middle ear (or tympanum) is a small cavity about five-eighths of an inch in length, half an inch in height, and from an eighth to a quarter of an inch in breadth. The drum-head, or membrana tympani, constitutes the greater part of one of the sides of this cavity—the outer one—but at all other points (with some unimportant exceptions) its walls are of bone, covered by an exceedingly thin, vascular mucous membrane. In health, this cavity possesses an outlet (the Eustachian tube), but when its lining mucous membrane becomes inflamed, this outlet undoubtedly closes, and we then have to deal with an inflammation occurring in a closed cavity, the walls of which are everywhere of bone, except at one spot, and here the resistance of a strong fibrous membrane must first be overcome before the fluid can escape, and before the conditions of a closed cavity can be done away with.

Now let us suppose that inflammation has attacked this cavity. What takes place? The vessels of the mucous membrane become gorged with blood, the mucous membrane itself becomes swollen and œdematous, and the valve-like tympanic orifice of the Eustachian tube becomes closed to all secretions that might otherwise find an escape through this channel. This is the *first stage* of the trouble, and it may last only a few hours, or it may be prolonged for even three or four days. This is the stage in which the application of a few leeches in the neighborhood of the ear may suffice to check the entire trouble. But, if the inflammation continue, stasis will occur in the capillaries and veins, and serum will be forced by arterial (cardiac) pressure through the walls of the bloodvessels, through the surrounding tissues, and finally into the cavity of the tympanum. After the cavity has become filled with the exudation, each succeeding portion of fluid exuded must find room for itself by forcing the membrana tympani outwards. The disease has now reached its *second*

stage. The tympanum is filled with serum, in which pus corpuscles are rapidly increasing in number, the membrana tympani is bulged outwards and pulsates, capillary and venous stasis is general, and great pressure is being exerted upon all the soft parts contained not only within the tympanum proper, but also within the communicating system of bony cavities, the antrum and mastoid cells. If now the membrana tympani be not abnormally resistant, the pressure will soon cause a thinning, and eventually a rupture, of the membrane at some particular spot. The pressure, which is the chief source of the pain, then ceases, the exuded fluid becomes more decidedly purulent in character, and escapes through the perforation, and the parts in due time return to a natural condition. In a large minority of cases, however, the termination is not so favorable. Owing probably to the great resisting power of the membrana tympani, the intra-tympanic pressure continues for a much longer period—perhaps even for several days—before a perforation is finally established. In these cases, it must be remembered that the tympanic mucous membrane is also functionally a periosteum. Hence the continued pressure is apt to result in superficial death of the underlying bone in one or more places. But there are also other points that should be remembered: the bloodvessels of the tympanic mucous membrane communicate freely with those of the adjacent dura mater, which is separated from the mucous membrane by only a thin septum of bone, and the mastoid veins empty directly into the lateral sinus.

If all these points be borne in mind—the anatomical and pathological conditions, and the impotent or even harmful treatment pursued—should any one be surprised at the terrible results that often follow inflammation of the ear, or wonder any longer at the large percentage of individuals in every community who go through life with an offensive discharge from this organ?

II. This brings us to our second question: *Can anything be done to diminish the prevalence of such Chronic Otorrhœas?*—In answering this question we shall confine ourselves, in accordance with the spirit of our subject, to the consideration of those measures which tend to prevent the inflammation from reaching what might be termed a *third stage*, one that is characterized by inflammation of neighboring organs, by proliferation of the soft tissues, and by carious processes in the subjacent bone. In a few cases, in the early part of the attack, warm applications and leeches are sufficient to prevent the inflammation from reaching the higher, second stage. In the majority of cases, however, we are unable to prevent this increase, and are consequently brought face to face with a middle ear in the condition of inflammation to which I have given the name of second stage. This is the time when paracentesis of the membrana tympani produces such beneficial effects, and the earlier it is performed, in this stage, the greater its efficacy. In this one slight operation, which in itself is neither dangerous nor very painful, lies the power to prevent the whole train of disagreeable and dangerous symptoms to which I have alluded so fully above. It is not far from the truth to say that every fatal case of purulent inflammation of the middle ear must, at some time in its course, have passed through a stage in which, by a resort to this operation, the progress of the disease might have been arrested, and the tendency of its course changed from an unfavorable to a favorable one.

In illustration of the speedy relief from pain which often follows the

operation, especially in children, let me narrate briefly the following case:—

A child, three years old, had been complaining of earache in the right ear during the greater part of the day. (For several days previous to this, she had been suffering from a marked nasal catarrh.) The pain in the ear was paroxysmal in character, and the child evidently had a high fever, though no measurement of the temperature was made with the thermometer. As night came on, the pain became more severe, and the intervals between the paroxysms shorter. Toward morning I was sent for, as the pain had become constant, and the child was in great suffering. An examination with the speculum and reflected light showed an œdematous and bulging membrana tympani (posterior half), the neighboring parts being very red, though as yet but little swollen. In the most prominent portion of the membrane I made an incision scarcely three millimetres in length, and involving simply the different layers of the membrana tympani. This was almost immediately followed by a watery discharge (without the aid of inflation), which ran down over the child's cheek. At the end of three or four minutes the child had ceased crying, and in less than a quarter of an hour she was fast asleep. At first the discharge was very abundant, and mainly watery in character, but it steadily diminished in quantity and became thicker, until finally on the fourth day it ceased altogether. On the tenth day the most careful examination of the ear could not detect any trace of either the inflammation or the artificial opening.

It would have served my purpose better, had I been able to offer in illustration a parallel case of acute, purulent inflammation following scarlet fever or measles; but unfortunately I cannot find in my records a single such case which came under observation as early as the second or third day after aural symptoms had manifested themselves. The morbid process, however, is practically the same in both cases, and the benefit to be obtained by early surgical interference cannot be materially less in cases following scarlet fever than in those following an acute nasal catarrh. The tendency to desquamation, and the enfeebled state of health following a moderately severe attack of scarlet fever, may, it is true, prolong the subsequent subacute stage of the aural inflammation for a few weeks, but the prospect of ultimate complete *restitutio ad integrum* is almost as certainly secured by timely interference in the one case as it is in the other. No harm can come from perforating the membrane too early, but months or even years of annoyance and suffering may be entailed upon the patient by resorting to the incision after the mischief has already been done. It then often fails to relieve even the pain.

In calling special attention to this operation, I do not wish to be understood as advocating it as the *only* means of treatment in such cases; I simply choose it because it is the *most important* one, and because its wonderful efficacy is susceptible of proof.

This is perhaps the best place to comment upon the view held by the late Mr. James Hinton, of London, respecting the efficacy of paracentesis of the membrana tympani in acute affections of the middle ear. In his work on "The Questions of Aural Surgery" (pp. 130 and 131), I find the following: "Since the real relief of the symptoms depends upon the escape of the matter through the membrane, it would seem an obvious inference that the membrane should be, at the earliest possible period, incised. And this is recommended by almost every writer; but experience of this method has left a doubt upon my mind. In the few cases in which I have had recourse to it during the acute stage of an inflammation, I have not found the results very satisfactory, and in two in-

stances in which the inflammation existed on both sides, and I incised one membrane only, the progress of recovery seemed retarded rather than expedited; I must, therefore, hold an undetermined position on this question." Such an opinion, coming from one whose authority in otological matters is so weighty, is calculated to prevent many from resorting to the operation, and that, too, notwithstanding the fact that in another part of the same work (p. 133) he says: "But whatever may be the best method of treatment in simple, acute catarrh[1] of the tympanum, there seems to be no doubt that immense evil might be prevented by free incisions of the membrane in very many of the cases in which it becomes inflamed during the course of the exanthemata." The example which Mr. Hinton cites, in evidence of the doubtful efficacy of paracentesis, furnishes at the same time a satisfactory explanation of the reasons why the incision failed to afford relief. In the first place, the incision consisted of a mere "prick with a cataract knife," and, in the second place, it was not made until the eighth day of the attack, or, in other words, late in what I have described as the second stage. Under such circumstances it is certainly not to be wondered at that the paracentesis failed to afford the desired relief.

My own experience leads me to recommend the operation *unhesitatingly;* in fact, without it, I believe the physician will be able to do comparatively little toward preventing those disastrous results to the hearing, to the general health, and even to life, of which I have already spoken more than once in this paper.

On looking over my records of cases (in hospital and private practice), I find that I have had occasion to perform this operation seventy-six times, on fifty-eight persons, for the relief of pain. In each of these cases the membrana tympani was either "red and swollen," or "bulged outwards," from the pressure of the fluid in the middle ear. In seven cases the paracentesis was performed on the 2d day of the disease, in four on the 3d day, in seven on the 4th day, in five on the 5th day, in four on the 6th day, in two on the 7th day, in ten on the 8th day, in two on the 10th day, in six between the 11th and 15th days, in one on the 20th day, in two between the 21st and 25th days, in one on the 30th day, in three between the 31st and 55th days, and in four at unknown periods. In twenty-nine cases, permanent relief from the pain was afforded within a few minutes or hours; in six other cases immediate relief was afforded, but the pain returned, either the next day or on some subsequent day; in eight others, the pain did not subside until the 2d, 3d, or 4th day after the incision; in four others, the operation afforded no appreciable relief; and finally, in eleven instances, the patients did not return to report the result of the operation.[2] Of the twenty-nine cases in which the operation proved quickly successful in relieving the pain, six had reached the 2d day of the disease, three the 3d, four the 4th, four the 5th, two the 6th, two the 7th, four the 8th, one each the 12th, 14th, and 15th days, and one an unknown stage of the disease. In other words, the operation

[1] Mr. Hinton's "simple, acute catarrh" is nothing more nor less than an acute inflammation of the middle ear, going on to the formation of pus, and to the exertion of a pressure equally great with that which takes place in the acute inflammation following the exanthemata. While, perhaps, the ultimate results are not quite as disastrous in the cases which develop in the course of a simple nasal catarrh as in those which accompany the exanthematous diseases, there is not a sufficient difference to justify any material difference in the course of treatment that should be pursued in the two classes of cases.

[2] These eleven cases all occurred in hospital practice.

may be said to have been quickly successful in cases chiefly of recent date. On the other hand, of the eighteen cases in which the pain returned, or in which there was little or no relief from the incision, five were subsequently relieved by incision of the mastoid integuments, eight by subsequent incision (or incisions) of the drum-head, and two by leeches. In twenty-five out of the fifty-eight cases, the day (of the disease) on which the discharge ceased was noted; in the remaining cases, either no note was made regarding this point, or the patients did not return to report their condition. In regard to the twenty-five, the facts are as follows: in six cases the discharge ceased on or before the 8th day of the disease, in seven between the 9th and 15th days, in two between the 16th and 22d days, in five between the 23d and 40th days, and in five between the 41st and 65th days. As far as the incomplete records go, therefore, in not a single case did there remain a permanent chronic otorrhœa with perforated membrana tympani.

Surely this record justifies me in recommending unhesitatingly the operation of paracentesis of the tympanic membrane. I am sorry to say that I have never had the opportunity of testing the value of this operation in those very cases in which we have every reason for believing that it would prove of the greatest value, namely, in those which occur in the course of the exanthematous diseases. Hinton, however, as we have seen, speaks in strong terms in its favor under these circumstances, and I believe that all or nearly all the authorities hold the same opinion.

Having shown how serious are the results which often follow an unchecked, or improperly treated, acute inflammation of the middle ear, and having also shown, as far as it is possible to demonstrate such a fact, that we are in possession of at least one means of preventing (in the great majority of cases, if not in all) these disastrous results, what more can I say to impress upon the profession "the importance of treatment of aural diseases in their early stages, especially when arising from the exanthemata?" The opinion expressed by Prof. Edward Clarke,[1] of Boston, in 1858, and reiterated by Von Tröltsch and Hinton—that a physician who treated a case of exanthematous fever without inquiring into the condition of the ear, was guilty of great neglect (or words to that effect)—might be uttered with as much appropriateness to-day. I would go still further, and say that a general practitioner who is unable to obtain a view of the external auditory canal and membrana tympani by means of the speculum and reflected light, is not properly fitted to practise medicine. While it may not be necessary for him to study otology, or to become skilled in aural manipulations, it is very necessary, if he wish to fulfil his duty toward his patient, that he should be able to determine by actual examination, whether or not the membrana tympani is being pushed forcibly outwards by the products of inflammation within the tympanum. If he be able to determine this point, and an hour's schooling will put him in possession of the necessary technical knowledge, I am sure that, where it is indicated, he will not be slow in taking successfully the second step, namely, the incision of the membrana tympani.

Medical students are taught very thoroughly how to amputate a leg, and their knowledge of the relations, distribution, etc., of arteries and nerves, will be found, at their examination for the degree of Doctor of Medicine, to be quite perfect; but has the new graduate the slightest

[1] American Journal of the Medical Sciences, 1858.

practical knowledge of how he should deal with an acute inflammation of the ear? Upon the medical schools, then, rests the chief responsibility for the prevailing indifference and ignorance among practitioners regarding these matters, and to the schools, therefore, the profession must look for assistance in bringing about a reform.

In conclusion, let me recapitulate briefly the main points which a consideration of the subject suggests:—

I. Chronic otorrhœa is at the present time a very common disease, due, in most cases, to the want of proper treatment during the acute stage of the affection.

II. It is by no means a harmless affection.

III. It may be fairly classed as a preventable disease, at least among those who possess a healthy constitution.

IV. Paracentesis of the membrana tympani, if resorted to during the first few days of the acute attack, and if not carried out too timidly, *i. e.*, if a free incision be made and not a mere prick, is almost a sure preventive of the subsequent, chronic disease.

V. The Profession at large, and especially the medical schools, should give this subject more earnest thought than they have in the past.

As some may be interested to ascertain further details regarding the cases referred to above, I append in a tabulated form the main points of interest in each of the fifty-eight cases, as far at least as my imperfect records furnish this information. And here I may say that it is not unfair to assume that the great majority of the patients in whom the result of the incision was not ascertained, failed to report their condition simply because they had been relieved of their sufferings, and did not feel the necessity of visiting the hospital again.

Tabular View of Fifty-eight Cases of Paracentesis of the Membrana Tympani.

No.	Age	Cause.	Condition of membrana tympani.	Condition of mastoid region.	Day of disease on which paracentesis was performed.	Relief from pain.	Day of disease (approx.) on which perf. healed and otorrhœa ceased.	Remarks.
1	17	Exposure.	Red and bulging.	Tender	7th	Speedy.	28th	
2	21	Not stated.	Bulging, but not very red.	Normal	2d	Immediate.	Not stated.	
3	32	"Cold in the head."	Œdematous and bulging; neighboring parts red.	Tender	6th	Entirely free from pain at end of 6 hours.	10th or 11th	Paracentesis repeated twice (with equal benefit) in same ear during following two years.
4	6	Not stated.	Posterior half bulging, red.	Not stated.	2d	Speedy.	Not stated.	
5	40	Sitting in a drau't of air.	Post. half red and bulging; small perf. in the ant. inf. quadrant.	Tender	14th	Relieved in 12 hours.	Not stated.	Three leeches also applied at same time.
6	40	Not stated.	Membrane bulging both anteriorly and posteriorly.	Not stated.	8th	Immediate.	Not stated.	Subsequent fresh attack. Incision repeated. Result not stated.
7	23	Not stated.	Red, bulging, and pulsating.	Tender	12th	Relieved in a few hours.	Not stated.	
8	25	Not stated.	Red and swollen.	Not stated.	11th	No marked relief till 4 days later.	26th	
9	32	Caught cold.	Post. half swollen and red; small perf. anteriorly.	Not stated.	8th	Pain gradually subsided.	Not statod.	
10	6	Not stated.	Red and swollen.	Not stated.	6th	Not known.	Not known.	Patient did not return to the Infirmary.
11	24	Caught cold.	Red and swollen.	Not stated.	About 21st	Not known.	Not known.	Patient did not return to the Infirmary.
12	33	Not stated.	Red and bulging.	Not stated.	3d	Pain gradually subsided; slight recurrence.	14th	Leeches also applied at the same time.
13	30	"Run over by a wagon."	"Much inflamed."	Not stated.	8th	Pain gradually subsided; no return.	Not stated.	
14	21	River bathing	Post. half red and bulging.	Not stated.	4th	Only temporary relief.	Not stated.	On the seventh day, two incisions in the membrana tympani, one anteriorly, one posteriorly; also leeches. On the thirty-seventh day opened a large abscess behind the ear.
15	14	River bathing	Post. half red and bulging.	Not stated.	5th	Pain soon subsided entirely.	Not stated.	
16	27	Not stated.	Red and bulging.	Tender	8th	Almost immediate relief.	Not stated.	
17	47	"Severe cold in the head."	Bulging.	Slightly tender.	End of 1st	Within two hours.	In the course of a few days.	Leeches also applied at the same time. Two days later the other ear became similarly inflamed, and at end of first day I incised membrana tympani in two places, affording escape, as in the case of the first ear, to bloody serum. Leeches also applied. Immediate relief followed.
18	8	Not stated.	Bulging.	Not stated.	2d	At the end of a few minutes.	7th	On the fourth day the other ear became painful. The following day incised the membrane and afforded immediate relief from pain, as in the case of the first ear.
19	49	Not stated.	Diffusely red background; landmarks not recognizable.	Very tender.	3d or 4th of decided pain; 21st of disease.	Very slight if any relief.	Not statod.	Relief obtained only by an incision of the mastoid integuments.
20	8	Not stated.	Membrane bulging.	Tender	Not stated.	Not known.	Not known.	Patient did not return to the Infirmary.
21	34	Not stated.	Bulging and red.	Not statod.	Not stated.	Not known.	Not known.	Patient did not return to the Infirmary.
22	28	"Took cold."	Œdematous, and bulging posteriorly.	Not stated.	Not stated.	Almost immediato relief.	Not stated.	

No.	Age	Cause.	Condition of membrana tympani.	Condition of mastoid region.	Day of disease on which paracentesis was performed.	Relief from pain.	Day of disease (approx.) on which perf. healed and otorrhea ceased.	Remarks.
23	17	River bathing	Red and bulging.	Not stated.	3d	Relieved soon.	18th	Two days later (fifth day) membrane again bulging and parts painful. Incision repeated. No return of pain afterward.
24	70	Not stated.	Posteriorly and superiorly, red and bulging.	Not stated.	About 42d	Pain mitigated, but not entirely relieved.	About 60th	Permanent relief not obtained until after incision of mastoid integuments on about the fifty-second day.
25	3	Not stated.	Post. half red and bulging.	Not stated.	7th	Relieved soon.	About 28th	
26	25	Syphilitic nasopharyngitis.	Membrane red and swollen; small perf. anteriorly.	Tender	About 8th	No material relief.	About 20th	Relief obtained permanently only by an incision of the mastoid integuments on about the fifteenth day.
27	29	Not stated.	Membrane red and swollen.	Tender	2d of acute exacerbation (about 53d since first attack).	Gradual subsidence with no return.	About 65th	
28	19	Not stated.	Membrane red and swollen.	Not stated.	4th	Gradual subsidence with no return.	39th	
29	57	Not stated.	Red and bulging.	Not stated.	10th	Immediate relief, but pain returned later, though not so violently	Not stated.	
30	22	River bathing	Posterior half bulging and red.	Not stated.	5th	Soon afterwards.	12th	
31	49	Not stated.	Post. half bulging and red; small perf. anteriorly.	Not stated.	5th or 6th of actual pain.	But little if any relief.	Not stated.	Poor health (syphilitic). Acute exacerbation of an otitis med. pur. of two months' standing. Four days after first incision, which in the mean time had healed, pain again became severe. Made a second and very free incision through posterior half of membrane. During following ten days, three more incisions, each one affording only temporary relief. Permanent relief was finally obtained after an incision had been made through the mastoid integuments, and, still later, one through the skin lining the external auditory canal.
32	5	Not stated.	Red and swollen.	Not stated.	14th	Not known.	Not stated.	Patient did not return.
33	40	Not stated.	Red, parched, and slightly bulging.	Tender	3d	Immediate.	36th	Two days later pain returned, though with less severity than before. On seventh day of attack made a second incision. Relief from pain lasted twelve days, when, the perforation having nearly healed, the pain returned. It was not severe, however, and may have been due to a furuncle in the meatus. After this, rapid and complete recovery.
34	7	"Cold in the head."	Bulging posteriorly.	Not stated.	2d	Speedy.	Not stated.	
35	30	Not clear.	Red and swollen.	No tenderness.	4th	Relief in 1 or 2 hours.	5th	
36	4½	From use of post. nasal syringe and salt water.	Red and bulging.	No tenderness.	3d (a mere prick).	Only slight relief.	Not stated.	On fourth day, I made a free incision through the posterior half of membrana tympani. Entire relief in a few hours.
37	11	Not clear.	Red and swollen.	No tenderness over mastoid but some over occiput.	20th	Very little until after 48 hours.	Not stated.	

No.	Age	Cause.	Condition of membrana tympani.	Condition of mastoid region.	Day of disease on which paracentesis was performed.	Relief from pain.	Day of disease (approx.) on which perf. healed and otorrhœa ceased.	Remarks.
38	7	Not known.	Red and swollen.	No tenderness.	3d or 4th	Not ascertained.	Not ascertained.	Patient did not return to the Infirmary.
39	42	Not known.	Red and swollen.	No tenderness.	14th	Not ascertained.	Not ascertained.	Patient did not return to the Infirmary.
40	24	Not known.	Post. half bulging: small perf. in ant. half.	No tenderness.	15th	Speedy relief.	Not stated.	
41	5	Not known.	Inflamed and swollen.	Not stated.	Not ascertained	Not ascertained.	Not ascertained.	Patient did not return to the Infirmary.
42	54	Blow on the ear.	Post. half red and bulging.	Tender on pressure.	About 30th	Not ascertained.	Not ascertained.	Patient did not return to the Infirmary.
43	14	River bathing	Much inflamed.	Not stated.	5th	Not ascertained.	Not ascertained.	Patient did not return to the Infirmary.
44	24	River bathing	Congested and uneven.	Not stated.	About 25th	Very little relief until 2 days later.	About 48th	Second incision on fortieth day, the pain having returned.
45	47	"A cold."	Red and bulging; small perf. anteriorly.	Tender on pressure.	8th	Almost immediate relief.	Not ascertained.	Pain returned next day, and on the eleventh day, the membrana tympani being again in a bulging condition, I made a second and free incision. Leeches applied at same time to mastoid region. Patient did not return to the Infirmary.
46	16	Not stated.	Red and swollen.	Not stated.	8th	Pain soon subsided.	14th	Pain returned on the eleventh day, and was relieved by leeches. It returned again on the thirteenth day, and was again relieved by leeches. Rapid recovery afterwards.
47	22	Not stated.	Red and swollen.	Not stated.	5th	Relief in a few hours.	In the course of a few days.	
48	38	Caught cold.	Red and bulging, especially posteriorly.	Not stated.	5th	Relief in a few hours.	13th	
49	3	Acute nasal catarrh	Red and bulging.	No tenderness.	2d	Relief in a few minutes	4th	A year later the incision was repeated in the same ear, under the same circumstances, and with equally beneficial and speedy results.
50	6	Not stated.	Red and bulging.	No tenderness.	8th	No relief until 24 hours later, when a discharge appeared.	Not stated.	High fever, apparently total deafness, and symptoms of general meningitis at time of making the incision. The other ear also involved.
51	25	Not stated.	Red, diffusely swollen, and œdematous.	No tenderness.	8th (leech's having failed to give relief)	Entire relief in a few hours.	14th	
52	34	From use of nasal douche.	Œdematous and slightly bulging.	No tenderness.	2d	Relief in a few hours.	3d	
53	35	Not stated.	Red and bulging.	No tenderness.	3d	Relief in less than an hour.	15th	Leeches failed to give relief on second day.
54	63	Not stated.	Red and bulging posteriorly.	Tender on pressure.	4th	Marked relief in a few minutes.	About 65th	
55	50	Unknown.	Red and bulging; perforation anteriorly.	Not stated.	10th	Not ascertained.	Not ascertained.	Leeches also applied. Patient did not return to the Infirmary.
56	21	River bathing	Diffusely swollen.	Not stated.	About 50th	No decided relief until 48 hours later.	About 64th	Abscess formed behind the ear and was opened by an incision. By the sixty-eighth day, the abscess had also healed.
57	20	Not stated.	Not stated.	Not stated.	8th	But little relief.	Not ascertained.	Second incision on the eleventh day. Patient did not return afterwards to the Infirmary.
58	22	Not stated.	Not stated.	Not stated.	6th	Marked relief in a short time.	Not ascertained.	Second incision on the ninth day, the pain having returned, and the discharge having ceased. Patient did not return afterwards to the Infirmary.

ON THE BEST MEANS OF TESTING THE HEARING.

BY

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In the following paper I shall endeavor to give a sketch of the known relation of hearing to the watch, the tuning-fork, and speech, the generally accepted tests. After a consideration of normal hearing, it shall be my aim to show how the diseased ear is affected by these tests, and wherein they give information as to its state, and wherein they fail to do so. Afterwards, a conclusion may be better arrived at as to the best form of test or tests for the hearing. It will be necessary to review the writings, discoveries, and opinions of leading physiologists and aurists of the last two or three decades, and by this means the ground of the best labors will be fully gone over. Nothing is promised as original on my part; rather shall it be my aim to bring together, at such a time as this, the doings of others, as impartially and completely as lies in my power.

No precise standard of normal hearing has been defined. The normal ear hears all sounds which fall upon it; but it cannot be said, *à priori*, where good hearing ceases and defective hearing begins, for, in many senses, *hearing* is a relative term. Just as there is an average standard for the amount of food, excretion, etc., there might be an average standard for hearing. The sense of hearing must be regarded as composite, consisting as it does of the ability to perceive not only a great variety of sounds, musical or periodic, but also those which are irregular or noisy. All such sounds can be heard singly or together. Hence, the sense of hearing may be said to lie in a collection of elements, largely nervous, which can be aroused singly or together. The mere fact that more than one sound can be heard at the same time, shows that the organ of hearing is composed of a number of structures arranged so that each can receive its definite impression.

The Watch.

Some form of watch-work, or ticking apparatus, has been used a long time as a ready means of testing the hearing. In this way the pocket-watch, mantel-clock, metronome, or an especially contrived, ticking machine, has been called into requisition. Such a test has usually been all that has been deemed necessary, and in many cases has seemed to satisfy both patient and physician as to the amount or state of hearing. Gradually, as the anatomy and physiology of the internal ear became better known, the idea arose of assigning to the cochlea the hearing of music or musical notes, and to the rest of the labyrinth, more especially to those parts in the vestibule, the perception of noises or irregular vibrations. When this theory obtained currency, the watch was classed

among noises and not musical sounds, though its beats were regular in their occurrence.

Be this theory of the physiology of the two parts of the internal ear false or true (it is not my province nor intention to review or criticize it here), Oscar Wolf has shown that the tick of a watch is a low form, if I may use the term, of musical note, with a not easily determinable pitch. Its impulse, therefore, cannot produce irregular waves of sound. All watches, metronomes, and the like, must therefore be classed under the head of instruments producing musical tones, and of course each watch, ticking apparatus, etc., has its own peculiar pitch. But at best these instruments give out but two notes, of short duration, and decidedly poor in harmonics.

This accounts for the inferiority of the watch, etc., as a test for hearing, and also explains why some affected ears appear to hear the watch better than others, and also why it is that there is so much difference in the relative manner of hearing the voice and watch, on the part of those hard of hearing.

According to Wolf, the tick of a watch should not be classed under noises, for it is really a musical sound with very high overtones, the short duration and intensity of which fall in a few seconds from their maximum to nothing. "With the ticking of a watch, we test the patient with but two tones out of the whole scale, and those neither very pure nor precise. The hammer or anchor strikes upon the cog-wheel, and by its impact calls forth two weak tones, which are somewhat intensified by the resonance of the watch-case. The pitch of these notes is not easily determinable; the intensity is measured by the observer according to the average distance at which persons of normal hearing are able to perceive the tick."[1] This has proven very unsatisfactory as a test, because it is often found that a watch is heard relatively better or worse than the voice, etc. This is very easily explained when we reflect that with the watch we have only tested the power of the ear to hear two relatively weak and impure tones. Wolf therefore says: "Should it happen that the patient has a disturbance of the nerve-fibres corresponding to these two tones, it is conceivable that, while absolutely deaf to the ticking of the watch, he may hear other far weaker tones which hold a different position in the scale, and, on the other hand, that the patient may hear the ticking of the watch at a relatively greater distance, and at the same time misunderstand certain spoken words, and even loudly spoken tones."

Such a disturbance of fibres in the nervous apparatus of the ear most likely happens even with the diseases of the middle ear, for Politzer[2] has shown that the tympanic vessels are connected most closely with the vessels of the cochlea; hence, tympanic disease is more than likely to bring about labyrinthine disease. Therefore, the above test is just as applicable in diseases of the tympanum as when the disease is primary in the labyrinth.

The Tick of the Watch or Clock.—Even a child will tell us, "The clock says tick, tock;" to its ears, therefore, there are in the sound made by the watch, vowel sounds of "i" and "o," and consonant sounds of "t" and "ck." Now, vowels and consonants are musical notes of certain, definite pitch, and therefore it may be said that even in this simple way a watch-tick is musical. This view of the watch-tick, viz., that it is an alternation

[1] Archives of Ophthalmology and Otology, vol. iv. p. 69.
[2] Correspondence with the writer, 1876.

of two notes simply, would explain in a measure why one clock is heard better than another by an affected ear. For, even when the intensity is the same, one watch is apparently heard better than another. This is due to the fact that the pitch of one appeals more strongly than that of another, to the ear which is being tested.

The Stop-watch.—Of all forms of ticking apparatus, the stop-watch is the most useful. Besides its power as a test, we also possess in it a means of finding out whether the patient really hears the sounds of the watch, or whether he thinks he does because he knows a watch is being held near his ear. This means has often been the first to declare that the patient's statements respecting his subjective state are not entirely reliable. Such occurrences, while testing with the watch, serve to show how unreliable this apparatus is, as must be all tests in the use of which the surgeon must accept the patient's statement alone, as to how much is perceived. Not only are adults of the greatest intelligence and fairness of intention sometimes utterly self-deceived by the watch-tests, but children as a rule give entirely unreliable statements respecting their ability to hear a watch. This, of course, is perfectly well known to all aurists, and I am sure that they have long ago concluded that accuracy in the use of any form of ticking apparatus can only be obtained by the employment of some form of stop-watch, since by this means they can find out whether a patient really is aware of the going or the stopping of the ticking.

Some observers have gained a similar end by alternately holding and removing a diaphragm of paper between the ear and the watch. In some instances, even while the ticking continues, the patient who has been hearing it will state that he now no longer hears it. This may be a perfectly true statement, and is explained by the fatigue of the ear. Every one must have noticed that in some forms of chronic aural catarrh the ear seems to lose its power of accommodation, or, as one may say, the power of continued listening. This has been specially noted by some writers (Weber-Liel and others), and is explained as being due to a failure in the muscular functions of the middle ear. It appears not only when testing with a watch, but also with speech. It is more likely to occur with a watch, however, because of the sameness of the test note, and the consequent continued use of the same parts of the hearing apparatus. Hence the watch may be a better test for this condition of the ear than the more variable vocal sounds, since the latter, playing up and down a series of notes, allow momentary rest of some parts of the ear, while others are being called into action.

Not only can the smallest amount of hearing be detected by means of a loudly ticking watch, but the same means may aid in prognosis in chronic diseases. The watch which I have found preferable, can be heard from 50 to 60 feet. If this watch is heard by a patient only a few inches, I feel obliged to give an unfavorable, or at best a guarded, prognosis as to the recovery of hearing. In such a case of excessive impairment of hearing, the loudness of the watch rather than the pitch of its note is to be considered, for if the ear cannot hear such intense ticking as that alluded to above, it certainly cannot retain much of its function. In such cases, a small pocket-watch would be useless, for not only can it not be heard by a very defective ear, but even an ear far from being very deaf cannot hear the faint sounds of a watch. Hence the intensity of the sounds of a ticking apparatus is of more importance than the pitch of its poor musical tone.

The Use of the Watch in Bone Conduction.—The watch is heard, *per ossa*, poorly by most persons over fifty years of age. Hence, it must be expected that the young will hear the watch much better by bone conduction than the old. Bearing this in mind, the ticking apparatus, of whatever form it may be, will generally give some clue to the state of bone conduction in our patients. But the usual weakness of its vibrations will interfere with its value as a test in this respect. The tuning-fork, however, is able to communicate its powerful vibrations through the bones of the head to the auditory nerve.

Conclusions respecting the Watch.—It may, therefore, be said that the watch, being a low form of musical apparatus, giving out one or at best two notes, poor in harmonics and lying near each other in the scale, is a test of only limited value. It may, however, be of great use, if its intensity is increased, in judging of the quantitative hearing in a given case. In very defective hearing, it is to the small amount of hearing still remaining what a lens is to a small object, for it enables the surgeon to find out if *any* hearing be present, and about how much. As a test for bone conduction its value is limited, both by the age of the patient and by the weakness of the notes. Although of some value in this respect, it falls far below the tuning-fork as a test for the sound-conducting power of the bones of the head.

The Tuning-Fork.

Little, if anything, has been added to our knowledge of the use of the tuning-fork in aural diseases since Politzer wrote his papers on this subject in 1868.[1] Dr. Urbantschitsch, of Vienna, has pointed out what he calls the deaf points (*tauben Punkte*) of the normal ear. These "deaf points" he discovered while experimenting with the small tuning-fork, passing the latter to and fro past the ear. Any one can satisfy himself on this matter by simply passing a small tuning-fork, with the line of the vibrating tines at right angles to the side of the head, backwards and forwards in the plane of the auditory canal. A point will be reached where suddenly the sound fades or disappears, to be heard again when the fork is moved either way from this point. This is purely physiological, and should be borne in mind in testing.

The tuning-fork, like the watch, may be used in two ways as a test: (1) By the air, aerial conduction: this is a test applied to the sound-conducting apparatus and secondarily to the nerve; (2) By the bones of the head, bone conduction: a test applied primarily to the auditory nerve, and secondarily to the sound-conducting apparatus. For the fork thus used shows whether sound can be conducted away from the ear in a given case, or whether, being impeded in their escape from the sound-conducting parts, the sound-waves are thrown back on the perceptive part of the organ.

If the note of the fork be not very loud, and if the tines of the instrument be properly clamped, the sound will not be heard further than a few inches from the ear. Thus one ear can be tested through the air, without risk of participation in the test on the part of the opposite organ. In addition to this advantage, a tuning-fork of low note and with well clamped tines, when used alongside the ear, cannot be heard through the

[1] Wiener med. Wochenschrift.

bones of the head. For these reasons a tuning-fork is of value as an aerial test. But, while highly musical, it has the great disadvantage, like the watch, of giving out only one note at a time, which may or may not be heard relatively better than another note by the affected ear, and therefore, like the watch, the tuning-fork is of limited usefulness as a test.

In order, therefore, to derive a wide advantage from the tuning-fork, one must either be at the inconvenience of shifting the clamps on a single fork, or else possess a large number, permanently graded in their pitch. Such a set of forks, to be of real value, must be most carefully graduated in order to correspond with the power of the ear to perceive the intervals between musical notes, otherwise certain fibres of the organ of hearing will be left out in testing. Such a set of tuning-forks for testing is not only difficult to obtain, but extremely costly. The tuning-fork, therefore, though possessing many excellent qualities as a musical test, through the air, is limited in its practical application.

It may be stated, as an axiom, that the normal ear hears the tuning-fork better through the air than through the bones of the head; but chiefly in the latter way has it been used as a test.

According to Politzer, E. H. Weber first settled the point that a vibrating tuning-fork, in contact with the bones of the head, was better heard in that ear the external auditory canal of which was stopped by the finger. This phenomenon was long unexplained, until Mach, on purely theoretical grounds, advanced the view that the reason of this lay in the hindrance offered by the finger in the auditory canal, to the escape of the sound-waves from the ear. Politzer, having in mind this phenomenon, thereupon made a series of experiments upon the human ear, and came to the conclusion that the above-named, augmented perception of sound, upon closing the external auditory canal, was due to (*a*) the reflection of sound-waves from the bones of the head through the air of the external auditory canal to the membrana tympani and auditory ossicles; and (*b*) to the hindrance which the sound-waves, passing from the bones of the head to the labyrinth and tympanic cavity, met in escaping from the ear. In the latter conclusion he was in entire agreement with Mach.

If hearing a tuning-fork better on the deafer side is due to hindrance of the escape of sound-waves, even while the external auditory canal is open, stopping up the latter with the finger should increase the sound of the tuning-fork. Dr. Roosa[1] is of the opinion that if closing the auditory canal by the finger does not increase the hearing of the deaf side, it is a sign that the nerve is affected. The late Mr. Hinton,[2] of London, was inclined very strongly to the view that, when for other reasons he could diagnose a nerve-affection on the deaf side, an increase of hearing for the tuning-fork by stopping the deaf ear, was confirmatory of the presence of disease of the auditory nerve. Politzer has shown (*loc. cit.*) that a nerve-affection may exist on the deaf side to such a degree that the delicate noise of a ticking watch is not heard better on the deaf side by bone conduction as long as the auditory canal is left open, but that by increasing the quantity of sound made to impinge on the nerve through the bones of the head, either by stopping up the auditory canal and forcing more sound from a given source, as when using a watch, or by increasing the original amount of sound employed in the test, as

[1] Treatise on the Diseases of the Ear.

[2] Questions of Aural Surgery.

when a large tuning-fork is used, then the diseased nerve, which is too weak to reply to a small amount of sound, is stimulated into action by the greater amount of sound-waves thrown on it. By remembering this latter fact, the views of Roosa and Hinton are made to appear less at variance than they seem at first sight, for the correctness of the theory of each of these distinguished observers is dependent upon the amount of nerve-disease, since it will be found that Dr. Roosa is correct if the nerve is paralyzed, and that Mr. Hinton's theory is right if the nerve still retains some of its power.

In alluding to sound-conduction through the bones of the head, Moos says: "We have in it one of the *most important* means of making a diagnosis of disease of the internal ear." He recalls the fact, already pointed out by Johannes Müller, that, in cases of bone conduction, conveyance of sound through the auditory ossicles and through the air must not be lost sight of. "Since in every case of bone conduction, *i. e.*, in cases where the vibrating tuning-fork is placed on the vertex, or brought in contact with the bones of the head, sound-waves pass into the ear in these two directions, the tuning-fork cannot be used in this way as an infallible means of making a differential diagnosis between diseases of the labyrinth and diseases of the middle ear; for it is well known by experience that the tuning-fork fails to aid in cases of well-known disease of the labyrinth, as well as in those of disease of the middle ear."[1]

Three-limbed Auscultation-Tube.—To confirm or correct the patient's statements, there has been advised the use of a three-limbed auscultation-tube,[2] two arms of which are to be placed in the auditory canals of the patient, and the third in the ear of the observer. If now a vibrating tuning-fork be placed on the vertex of the patient, the auscultator can perceive the sound of the fork, streaming from the ears of the patient. By alternately pressing the two arms of the apparatus connected with the patient's ears, the auscultator can learn from which ear the greater amount of sound comes. It seems but natural to suppose that more sound-waves must come from the less obstructed ear. The latter will, as a rule, be the better hearing ear, unless its fellow be deaf, not from obstruction in the sound-conducting parts, but from paralysis of the auditory nerve.

The Interference-Otoscope.—A similar instrument, though one used in a different way from that just mentioned, has been devised by Prof. Lucæ,[3] of Berlin, and is named by him the interference-otoscope. This instrument consists of a double stethoscope of Scott Allison, the limbs of which, intended to fit snugly into the auditory canals of the patient, are eleven Paris inches long. At the junction of these symmetrical arms is placed a T-shaped, glass tube, from the portion of which representing the standard of the letter, passes a rubber tube to a collector of sound, half paraboloid in shape. Here the vibrating tuning-fork is placed. To the other end of the cross piece of glass tube is fixed the rubber tube, two feet long, for the auscultator. Lucæ's experiments in this direction were based on the fact that sound-waves, falling on a stretched membrane, are only partly taken up and transmitted by it. The supposition then naturally follows that sound-waves entering the external auditory canal are only partly transformed into the peculiar, pendulum-like, to-and-fro movements of the sound-conducting membrana tympani and

[1] Klinik d. Ohrenheilk., S. 41. [2] Moos, op. cit., p. 42.
[3] Archiv für Ohrenheilk., Bd. iii. 1867.

the ossicles. According to the greater or less extent to which the membrana tympani takes up the sound-waves falling on it, this so-called reflection of the waves of sound will vary in amount. The investigations made tend to elucidate experimentally this reflection, and the probable influence on it of the changes of tension in the sound-conducting apparatus; also, from a study of these phenomena of reflected sound-waves, the endeavor is made to obtain an objective expression of the sound-waves taken up by the ear.

Physical Experiments.—Physical experiments show that (1) A stretched and inclined membrane of India-rubber, placed in an artificial ear made to represent as closely as possible the natural organ, will reflect a certain quantity of the sound-waves entering the external auditory canal; (2) Closure of the Eustachian tube increases slightly this reflection; (3) Increased tension of the membrane shows that the reflection is directly proportional to the tension; (4) This outward reflection of sound-waves is greatest whenever the tension occurs simultaneously with considerable changes in the density of the air contained in the tympanic cavity.

In order to make practical application of these laws, Dr. Lucæ devised his interference-otoscope, by which the relative amounts of reflection from both ears, it is claimed, can be determined. The results obtained in the normal ear, by the use of this instrument, are thus given: (1) The normal organ of hearing reflects a certain amount of the sound-waves entering the external auditory canal; (2) The reflection increases in all changes in the sound-conducting apparatus, especially those in the middle ear, which lead directly or indirectly to an increased tension of the membrana tympani; (3) The examination of persons with normal hearing, by means of the interference-otoscope, shows that the different sensibility of both ears, for the same tone, is caused by the different amounts of reflection brought about by different tensions in the two sound-conducting apparatuses. Respecting the diseased ear, the conclusions are: (1) The interference-otoscope shows in the majority of cases of disease, in analogy with the observations made on those with normal hearing, a *greater* reflection from the diseased ear; (2) This is found in a number of cases in which the ear-mirror and the catheter reveal a disease in the external or middle ear; (3) In the numerous cases of ambilateral chronic catarrh of the middle ear, without perforation of the membrana tympani, the examination usually reveals a *greater*, though often a *less*, reflection from the worse ear; in the latter instance a simultaneous disease of the labyrinth may be supposed, and the prognosis becomes by far less favorable; (4) The greatest worth of the method lies in the not uncommon cases in which all other diagnostic means fail to show morbid changes in the external and middle ear; here too, as a rule, a stronger reflection is observed on the worse side, which points to a deep-seated disease of the sound-conducting apparatus. Only in some few cases does the examination reveal a less reflection from the worse ear, in which cases a primary disease in the labyrinth may be assumed with great certainty.

Tuning-Fork Vibrating on the Parietal Protuberance in a Normal Case.—If a vibrating tuning-fork be placed on either parietal protuberance of a person with normal ears, it will usually be heard in the opposite ear. This is most easily perceived when a large fork of deep and heavy note is used. This phenomenon, if it may be so termed, will often lead to confusion in diagnosis, inasmuch as the examiner would at first expect the fork to be heard best in the ear nearest to which the fork is placed.

As it is heard best in the more distant organ, a conclusion might be made that the latter ear was diseased in its conducting parts. Care must, therefore, be taken to have the vibrating instrument in the central line of the head, either on the vertex or glabella, or held between or on the teeth. An explanation of the above is perhaps most satisfactorily sought for in Dr. Lucæ's[1] demonstration that vibrations which fall perpendicularly on the membrana tympani produce much the strongest vibrations, and that hence a tuning-fork placed on the parietal prominence, or the side of the head, will be chiefly heard in the opposite ear. According to Dr. Lucæ, this is best perceived when the meatuses are stopped. But it can also be perceived, as any one can find out, with perfectly open meatuses.

Conclusions respecting the Tuning-Fork as a means of Testing and Diagnosis in Diseases of the Ear.—It has been shown that the tuning-fork has a twofold use: (*a*) as a test through the air, and (*b*) as a test through the bones of the head; *i. e.*, it is a test of the sound-conducting power of the ear, and also of bone conduction of sound to the auditory nerve, and of the perceptive power of the latter. In the first instance, its advantages are that one ear may be tested without communicating the sounds of the fork to the other ear, since a properly clamped fork cannot be heard far enough when held before one ear to be perceived by the other. The disadvantage in the tuning-fork is that it gives only one note at a time, and, therefore, testing with that gives no information respecting the ability of the ear to hear other notes. Its applicability, therefore, is limited. Various notes may be produced by the same fork by clamping the tines at different points. But this necessitates some delay.

The tuning-fork finds its greatest usefulness in testing bone conduction. While it has never fully realized in this way all that was hoped for it, as an aid in diagnosis, it is still the best means, and a very good one too, of determining how much sound is conveyed to the auditory nerve through the head-bones. Its musical nature, as well as its powerful vibrations, render it far superior to the watch as a test for the conducting power of the head-bones, unless the ticking of the watch is made to occur with great force. But should the ticking of the watch equal in intensity the vibrations of the fork, the former could never approach the latter in musicalness. The tuning-fork is a means of comparison between the bone conduction and aerial conduction of sound in the same person; for if the vibrating tuning-fork be held on the vertex until its note is no longer perceived by the person examined, and then held before his ear, if he now perceive that the fork is still vibrating, it is fair to conclude that the sound-conducting apparatus is normal; but if the fork, when no longer heard through the air before the ear, be heard as soon as it touches the vertex, the conclusion is inevitable that there is some impediment in the sound-conducting part of the ear. This is all the more convincing if it be borne in mind that there is being used the same note, and one, too, growing a little weaker all the time, because, if vibrations of a tuning-fork cease to be heard in front of an ear by aerial conduction, but are able to communicate themselves while growing constantly weaker, through the bones of the head, the inference of great derangement in the middle or external ear (aerial sound-conducting parts) cannot be avoided.

Bone conduction, however, is of little importance to the patient, and of great uncertainty as a guide to the aurist, because he is forced to rely

[1] Berlin klin. Wochenschr., No. 10, 1871.

almost entirely on the patient's statements as to what is perceived by the latter when the test is applied. Therefore, the tuning-fork as a means of testing bone conduction can never become of pre-eminent value. At best, it must be used in conjunction with other tests, and its results carefully weighed. Then, too, the musical education of the patient most probably influences his answers respecting the sensations produced by the tuning-fork vibrating on the vertex.

Speech.

By hearing speech, the intellectual development of the human being is accomplished. There is no sound so familiar, nor so fondly longed for at times, as the sound of the mother-tongue. A person with good hearing can never realize the feelings of a deaf person so vividly as when travelling in a strange land, surrounded by those speaking with each other happily, gayly, and with varying expressions, but in a language unknown to the lonely traveller. One falls into the position of an invalid, is treated with a kind of pity, and, alas, finds himself growing a little suspicious and morose.

The deaf person feels the loss of hearing the voice of others more than the loss of the power to hear anything else. To recover the ability to hear the familiar tones of the voices of his friends, he would gladly give up all other hearing. So great is this struggle to hear what others say, that the deaf gradually learn to understand the thoughts and words of others by watching the lips of the speakers. The power to hear other sounds well, may begin to fail without the knowledge of the patient, but all his endeavors are concentrated, almost unconsciously, to catch the varying sounds of speech. I have known young physicians to be almost deaf to the ticking of a watch without knowing their loss until it was accidentally discovered, for their ability to hear speech was retained. All aurists are aware that patients are constantly surprised to learn how *deaf* they are, as soon as their faces are averted from the speaker. Thus the failure of hearing, in this respect, is detected very often, for the first time, in the summer, when all are used to sitting either on the porch or in the parlor, in the twilight and dark. As the light fades, and the faces of those around are no longer plainly visible, the hitherto apparently hearing person becomes aware that he is deaf. This is often assigned to the night air, but, in reality, it is due to the loss of vision in the approaching darkness. The surgeon will often gain great aid by a knowledge of these facts, and also by observing the manner in which a partially deaf person will look at the one he is addressing, and from whom he expects a reply. Those of delicate sensibility soonest become aware of their inability to hear the voice, for speech is not only a delicate sound, but is highly valued by the cultivated. Those of less sensibility are not aware of their loss of hearing speech, for they still hear loud sounds and even music, as the latter is more powerful than the notes of speech. Hence, it is surprising to find how large an amount of unconscious deafness exists in the uneducated; they do not feel their loss as the cultivated do.

Very often the deaf are, in a measure, unaware of their aural condition, because they feel, instead of hear, so many sounds. Roosa has found that the deaf and dumb may hear, *i. e.*, feel, at the diaphragm, a tuning-fork vibrating on the vertex. A man who had been struck on the head, and in consequence had become totally deaf, informed me that

he could *hear* a tuning-fork vibrating on his vertex. This I doubted: I believe that he merely *felt* the vibrations. Whether he heard them or not, he could not hear the loudest shout into his external ear. The loss of hearing speech was his great misfortune; this he desired to recover; and in his case *speech* became the great test. The tuning-fork was valueless because unreliable.

The human voice was considered an admirable quantitative test, long before the researches of Donders, Helmholtz, and Wolf, showed it to be equally valuable as a qualitative test. Why it was that a patient could hear some words much better than others, though spoken at the same distance, was not explained and applied until O. Wolf published his investigations into the acoustic characters of the various elements of speech.

The human ear perceives as musical sounds, tones varying from 16 vibrations to 20,000 vibrations in a second. Preyer (Jena, 1876) has lately placed these limits from 15 vibrations to 40,960 in a second. Speech embraces only eight octaves, according to Wolf,[1] viz., "R" with 16 vibrations, and "S" with 4324 vibrations in a second. In music, the most agreeable notes, with the most distinct pitch, lie between 40 and 4000 vibrations per second, *i. e.*, within the limits of seven octaves; those which are admissible in the widest sense as musical, lie between 20 vibrations and 38,000 vibrations per second, *i.e.*, within eleven octaves.[2] In this respect the ear far outstrips the eye, for the limit of the perceptive power of the eye for vibrations of light rarely exceeds one octave.

Blake's experiments[3] with König's rods, have shown that the human ear, in some instances, hears distinctly, as musical, 35,000 vibrations in a second. The ability to hear these very high notes is greater in youth (12 years) than in older persons. The former may distinctly hear a note of 40,960 vibrations per second, at a distance of 34 feet; from 18 to 20 years of age, this note is heard only at 13–16 feet; and at the extreme limit of 34 feet, only the tone of 36,864 vibrations per second is heard. This ability to hear high notes greatly diminishes as age advances. Dr. Blake says: "In the majority of the cases of persons over fifty years of age, and possessing apparently normal hearing, who were subjected to these tests, an examination of the membrana tympani showed a varying degree of opacity of the membrane, corresponding to a thickening of the mucous coat." All such sounds were heard much better when the membrana tympani was perforated, especially near the posterior superior periphery. In one case, where a small band of cicatricial tissue extended from the posterior border of the perforation to the long process of the incus, a tone of 65,536 vibrations per second was distinctly heard.

Wolf's Determinations respecting Hearing, Voice, Vowels, Consonants, etc.—According to Wolf,[4] it is impossible to establish the distance at which the separate vowels can be heard, because the sound-sources from the speaker and the hearing of the listener vary in different individuals, and also because the sound-waves are so dependent upon the condition of the atmosphere in which we are placed. At night we hear much further than by day, because the ear is less distracted by a multitude of sounds, and because there is a much less number of sound-waves crossing each other than in daytime. (1) Vowels are endowed with the greatest strength of tone, *i. e.*, they are heard and understood at a distance at

[1] Sprache und Ohr, 1871.
[2] Helmholtz, Tonempfindungen, S. 31.
[3] Trans. Amer. Otolog. Society, 1872–1873.
[4] Op. cit., p. 58.

which all the consonants are inaudible; and (2) The consonants differ very greatly in the strength of tone.

(1) *Vowels.*—Vowel-sounds are composed of a number of beautifully harmonic overtones, which accompany the fundamental note, and strengthen the fundamental tone of the mouth. A good musician can hear a perfect accord when a vowel-sound, especially "A," is uttered with clearness, which is said (Wolf and Appunn) to be specially observable when the sound is made in the open air, where the sound-waves can escape with greater precision than in a room. "A" (German, broad) has the most overtones, five, and is heard the furthest, *i. e.*, at 360 paces (Wolf). The vowel "U" ("Oo," English) has the fewest overtones, three, and is heard with most difficulty of all vowels. It can be distinctly heard only at 280 paces. The vowel "O," containing many beautiful harmonic overtones, is heard nearly as far as "A," broad. German "E" (English "A") is heard at 330 paces; German "I" (English "E") is heard at 300 paces; English "I" is somewhat more powerful than English "A," and weaker than broad "A" or "O"; "Oi" comes close behind "E"; the weakest of all diphthongs is "Ou," as in *out;* it is a little stronger than "Oo."

(2) *Consonants.*—The investigations of Wolf have led him to classify consonants according to their acoustic and physiological laws, under two heads, viz., those which are self-sounding (*selbsttönende*), and those which are sound-borrowing (*tonborgende*). The former class embraces those sounds which are formed by the apparatus of the oral cavity, with a sound which is independently audible, and which can be defined in its pitch, intensity, and timbre. The second class comprises those consonant sounds which attach themselves to a vowel-sound, and are obliged to borrow from it in order to be distinguished by the ear; musically they are to be termed the appogiatura, and note of complement to a vowel sound; such consonants are H, L, M, N, W.[1]

"H" is the weakest of all consonants when pronounced without a vowel. It is lost at the distance of a few paces. Next in strength stands "B"; "Ba" is heard further than "Ha." "B" alone is heard at a distance of 18 paces. The deeper a note is, the less effect it has upon the ear. "R," with only 16 vibrations in a second, is not distinguishable further off than 41 paces. "K" and "T" stand next; they are both heard about equally well at 63 paces. "T" resembles pretty closely a simple note, but it has a pitch which appeals more readily to the ear, and is therefore much better heard than "B," which otherwise is very similar to it. "K" is formed with relatively favorable circumstances—that is, by means of a powerful movement of the root of the tongue. The soft "F" is heard somewhat further than the foregoing letters, *i. e.*, at 67 paces. "S" is perceived at a relatively greater distance than the foregoing on account of the pitch of its fundamental note, which appeals to the ear by its sharp character, especially when sharply pronounced; and finally on account of its properties as a sibilant consonant. "To this latter property it owes its ability to express disapprobation in public assemblies, to cry down opposing sentiments, and to enforce silence. Both its moral use and physical character are inharmonic." "S" can be heard very distinctly at 170 paces. "Sch" is heard furthest of all consonants, because it possesses a full and rich clang-tint, and is composed of three harmonic notes, which predominate, while the inharmonic overtones or noises recede. The composite conso-

[1] See Wolf, Archives of Ophthalmology and Otology, vol. iv. p. 78.

nant "Sch" (nearly = "sh") can be heard at 200 paces. "M" and "N," unaccompanied by vowels, are only meaningless blowing of air through the nostrils. "Mama" and "Nana" are understood at 180 paces, but at greater distances the sounds of "M" and "N" are lost, while the vowel "A" is still heard.[1] In calm weather it is extremely interesting to ascend a high hill and listen to the voices of men below in the valley or town. Words are no longer distinguishable, or, at best, only those which are composed of "M" and "N" and vowels, as "Mama," "Nein," etc. Vowels in all the other words are distinctly heard, following each other in a curious interchange and with remarkable cadences, because we are no longer able to join them together into words and sentences.[2]

It is thus shown that in the component sounds of ordinary speech, a wide range of tests, with different intensities of sound, is offered to the aurist. Such a numerous set of sounds is needed in order to discover which are heard best by an affected ear. One sound is not sufficient, because an ear may be unable to hear some sounds while it hears others comparatively well. Hence if only one or two sounds should be employed, as in the watch, just those notes, it might be, the ear could not hear as well as others. Therefore, speech becomes most valuable, because it is very varied in sound-nature, and also because it is ever at the command of the aurist, and the use of it is fully comprehended by the patient. If one sound would do as a test, a sound-unit could be established; but it is seen upon short experience that a solitary and fixed sound is not heard in the same way by all similarly affected ears, and that, therefore, one sound cannot be assumed as a fair test. This throws the surgeon back on speech, as containing a variety of sounds, by means of which an average condition of the ear can be ascertained.

Very often whispers and words spoken in low tones are heard much better than loudly spoken words, by an affected ear. This is due to the damping of the vowels, whereby the consonants, which are less sonorous, have a chance to be heard. And thus it is that a person hard of hearing often asks not to be addressed loudly. Words may be heard, even when the letters composing them, spoken separately, are not heard. This is especially so for the letters B, P, T, K, and R (Wolf). The reason of this is very apparent, for when letters are pronounced alone, they are words. Thus "B" is composed of sounds of "b" and "e," "P" of p e a, or p e e, "K" of k a y, while "R" is equivalent to sounds of a r e. In the latter instance, "R," when pronounced, is altogether of a different phonetic value from that which it has when standing at the beginning of a word, as in "rat," or at the end of a word, as in "tar." Whispering has an advantage over loud words in testing, since the former cannot be as easily conveyed as the latter through the bones of the head, to the auditory nerve.

However, it must be borne in mind, that in Dr. Dennert's observations[3] made in a case in which the cochlea had been lost by necrosis, the normal ear, though artificially stopped up as thoroughly as possible, could yet hear whispers at a distance of six feet. Patients often state that they hear music either of a piano or of an orchestra, but that they cannot hear speech. This is due to the delicacy of the sounds of the human voice in speech. The most delicate sounds of a violin are far more powerful than

[1] Wolf, loc. cit., p. 63. [2] Helmholtz, Tonempf., S. 118.
[3] Archiv f. Ohrenheilkunde, Bd. x.

speech, and hence the ability of those hard of hearing to perceive instrumental music much better than the component sounds of speech, as has been shown by Wolf.

Variable Hearing.—In some cases of chronic aural catarrh, the statement that the hearing varies with the weather, health, etc., is made by the patient. This is not always demonstrable, nor, if so, are the variations in hearing very striking. But in some other forms of aural disease, variable hearing occurs in a marked degree, and may lead to a knowledge of the nature of the disease. There are not many cases of the kind I allude to, recorded in aural literature. I have observed two cases; one in which the hearing varied greatly with the position of the patient, being least when he was in the upright position, and greatest when he lay down. This variability was due to movements of fluid in the tympanum.[1] The other instance of variable hearing occurred in a well-marked case of acute aural vertigo, presenting features which, like those in a case related by Hinton,[2] naturally led to the conclusion that the disease as well as the variable hearing was due to derangement in the muscles of the tympanum; for the attacks of vertigo were ushered in by the onset of tinnitus and hardness of hearing, which gradually increased in severity, culminating in half an hour in nausea and vomiting, which were gradually followed by relief of all the symptoms. During these attacks of buzzing in the ear, the hearing was diminished, as could be shown by the watch and voice. But as the function was restored as the attack passed off, the idea naturally arises that the symptoms might have been due to spasm in muscular structures in the middle ear. It also leads to the conclusion that the term Ménière's disease, by which such attacks have generally been called heretofore, has been applied too widely, if by it disease only of the labyrinth or parts of it, has been meant. Aural vertigo should be the general term; Ménière's disease may be limited to the labyrinth. It is highly probable that hearing, varying quickly within short periods of time, could in no case be referred to alterations in the labyrinth, for the latter, as far as they have been demonstrated to exist, have been permanent changes, and not recurrent. Recurrence, however, is a well-known feature in muscular spasms.

Hearing Lower Tones better than Higher Ones.—In many instances which have come under my notice, I have been informed by the patients that they could hear some kinds of sounds very much better than others. Thus, in two cases the patients voluntarily said that they heard thunder (bass note) better than the chirping of crickets, or high tones on the piano. And, on being tested with watches, one patient said that he heard that watch better which gave the tick of a deeper note. Experimentally, I have shown[3] that a deep note has the advantage over the high ones in cases of increased labyrinthine pressure. In an increase of the pressure within the labyrinth, the stapes becomes more fixed, and it is at this point that the vibrations begin to grow less marked as the pressure in the labyrinth increases. In such a case it is manifest that if vibrations from without be normally conveyed to the stapes, they must there meet with hindrance as they endeavor to reach the labyrinth. Only the most powerful waves are able to overcome this obstacle, and to force the stapes to undergo a to-and-fro movement with the rest of the

[1] See Philadelphia Medical Times, Oct. 17, 1874.
[2] Questions of Aural Surgery, pp. 262–263.
[3] Archives of Ophthalmology and Otology, vol. ii.

chain of ossicles. I have, therefore, thought that it might be asked: Could not the inability to hear high notes, while low ones are heard almost normally, be construed into a sign that the stapes is impeded, either by undue pressure in the labyrinth, or by catarrhal fixation in the oval window? That the cause of such a peculiar alteration in hearing probably does not lie in an undue tension of the membrana tympani, appears from the well-known physical fact that the tense membrane is more sensitive or susceptible to vibrations of high notes than to those of low ones.

Do the position and extent of perforations in the membrana tympani cause variations in hearing-power for certain sounds? This question may be answered in the affirmative, according to the experiments of Wolf. He divides defects of the membrana tympani into five groups, viz., (1) small defects in the lower segment of the membrana tympani; (2) defects as large as a quadrant in the lower part of the membrana tympani; (3) defects as large as a quadrant or a quadrant and a half; (4) defects as large as three-quarters of the membrana tympani; and (5) complete defect of the membrana tympani and loss of the malleus and incus. Vowels are heard invariably better than consonants in all of these cases. I have observed that defects of the membrana flaccida are always attended with great hardness of hearing. This is probably due to an implication of the joints of the ossicles.

In testing hearing, we must consider the way in which the sound is conveyed to the ear, whether through the apparatus of the middle ear, or by the bones of the head. The latter, without doubt, has more influence than has generally been supposed.[1] It is really astonishing to discover the amount that one may hear with a normal ear stopped up. This shows how much sound may be conveyed to the auditory nerve through osseous tissue. A want of proper appreciation of this fact has led to great errors in testing hearing, in cases of one-sided deafness. Merely closing the good ear, and leaving the poorer one open, is not sufficient. The hearing must be tested, first, in the better ear when stopped up and turned towards the examiner, the worse ear being open. When the amount of hearing in the better one, made artificially deaf for sounds through the air, has been determined, then we may see what effect opening and closing the deafer ear has on the hearing of the stopped-up, better one. The result will be the amount of hearing for the worse ear.

A plan similar to the foregoing, and one which I have used for a long time, in cases of one-sided deafness, is the following: Place the patient so that the affected ear is towards the surgeon. Then stop, with the finger, the better ear, the ear not to be tested. This may be done by the patient or by an assistant; preferably by the latter where great certainty is desired. Then, with the affected ear open and turned towards the surgeon, let tests of its hearing-power be made. When the limit of hearing on that side is obtained, let the ear be closed as the other is, and then, with the ear still turned towards the examiner, let tests be made again. If the closure of the deaf ear, the one turned towards the surgeon, cause no difference in the hearing-distance already obtained, it is fair to conclude that whatever amount of hearing exists is not due to a passage of sound through the external auditory canal of the ear turned towards the surgeon. In such a case the conclusion must be that either sound goes more easily through the bones of the head on the

[1] See important papers by Dennert and Lucæ, Archiv f. Ohrenheilk., Bd. x.

affected side than through the meatus to the auditory nerve, which would be absurd, or that sound has reached the brain by the other ear, and that the affected ear is totally deaf. If, however, stopping the ear turned towards the examiner—the ear supposed to be the deafer—make that ear still deafer, let the examiner approach the patient and repeat the tests till they are heard once more. The second hearing of them is evidently due to conduction of sound through all obstacles represented by cranial bones and the finger in the meatus, and must not be regarded as aerial conduction. The extent of the power of the ear to hear by aerial conduction will be expressed in the difference between the limit of the first test and the limit of the second. Thus a patient may hear speech as far off as eight feet, with the good ear stopped and turned away, and the affected ear open. When the latter is stopped, speech is no longer heard at eight feet, but it may be perceived by the patient at a distance of three feet, both ears still being kept firmly stopped. In such a case, not the former distance, but the difference between it and the latter distance, viz., *five feet*, must be considered the amount of aerial conduction by the external auditory canal, for that represents the *amount* of loss of hearing caused by stopping the meatus.

Wolf's Word-Tests.—In testing the power of an ear to hear the human voice, it seems to me that we gain some very practical hints from Wolf's system of word-tests. Wolf, in his very valuable work, has divided defective drum-heads into five groups (vide supra, p. 971), and in each of these he has applied a certain list of chosen words as tests. At the end of his observation of the hearing in each group, he has struck an average of the words heard, and has thus, as it seems to me, made a practical suggestion. Printed slips with words containing the different vowels and consonants at the beginning, middle, and end, might easily be arranged in all languages, and the aurist provided with them could always discover the ability of his patient to hear given vocal sounds. The average of the patient's misses and successes would give his hearing power.

A Conventional Sound-Unit.—A conventional sound-unit might be constructed so as to be of uniform and invariable intensity, but "intensity of sound-sensation and intensity of sound-producing vibration are two entirely different things. The first is a physiological phenomenon, and varies with individuals like all such phenomena, and the second is a dynamical phenomenon. To assert that the first is proportional to the second, as all authors do, is to use *à priori* physics in which I have no faith."[1] Hence if such a sound-unit were made, it would possess only a limited value. It would also be open to the same objections as the watch, or the solitary note of any other test. One sound can no more become the test of hearing than one object can be the test of vision. Even speech itself, which affords the most convenient as well as the most ample test, will not fulfill all its capabilities unless it be studiously varied so as to bring into play all its elements, with their varied phonetic characters and intensities.

Kessel has laid down four conditions which any form of acoumeter should fulfill in order to satisfy the wants of the aurist of the present day: (1) The acoumeter must embrace a large range of tones in the most carefully graduated series; (2) The strength of the tones must be always the same; (3) The acoumeter must be able to test the hearing in one ear at a time, both respecting air and bone conduction; (4) The intensity of

[1] Prof. A. M. Mayer; Correspondence with the writer.

the notes must correspond as nearly as possible with that of the voice. Kessel has had an apparatus made which corresponds in the main with the above conditions. The tones are produced by means of metal teeth, which are set in vibration by keys placed in a brass cylinder, like those of an ordinary music-box. The teeth give the notes of the scale of "C major," throughout seven octaves, and, if desired, can produce even eight octaves. Discordant sounds may be produced by striking the notes of several octaves at the same time. The cylinder is turned by a handle, to which is fastened a pointer which indicates on a dial the pitch of the note given out. The air conduction is effected by means of a rubber tube, connected with the cavity of the apparatus. The latter is covered with poor conductors of sounds, and packed in a case, so that the sounds produced in the apparatus are inaudible even at a short distance from it, except through the gum tube. Bone conduction is effected by bringing the mastoid process into connection with a metal prong or bar, passing from the back of the comb-like piece containing the teeth heretofore alluded to.

Since by this means the air in the external auditory canal is alternately rarefied and condensed, and thereby the tension of the membrana tympani and the pressure in the labyrinth altered, we have a means of finding out under which of these conditions the hearing is best. The apparatus also shows that when both ears are unequally excited by the same sound, the latter is referred to the more stimulated ear, independently of direction. Thus if the right ear be brought near enough to the case to hear the sounds through the packing, and if then the rubber tube be brought around to the left ear, the observer will think the sound comes only from the left.

I have never had any experience with the apparatus devised by Dr. Kessel, but it seems fair to assume, from what is known of the character of the voice in speaking as a test, that the instrument alluded to provides the aurist with no better means of testing than he always possesses in the use of speech.

There are needed, in any form of test, comprehensibleness, scope, delicacy, gradations without great intervals, convenience of application, inexpensiveness, and invariability. The watch and tuning-fork have been described, and conclusions drawn respecting them. The further the investigations have been carried, the more conclusive have appeared the reasons for assuming that the voice will render most aid in determining the state of the hearing. Nothing is more convincing of this than the fact that all artificial means, yet proposed, have been modelled more or less after the voice, or some of the component sounds of speech.

Invariability would always be an argument in favor of any form of test, and it has been urged in behalf of the watch, the tuning-fork, and other forms of artificial test-means. This certainly is desirable for comparisons during treatment, no matter what form of test be used. But every aurist can learn to modulate his voice so as to be able to command always the same degree of loudness or lowness in making tests with it. Even if artificial means were as superior to the voice in invariability as some have thought, the far greater range of the latter as a musical apparatus especially adapted to the ear, would place it far ahead in the list of tests for hearing.

THE FUNCTION AND UTILITY OF THE ARTIFICIAL DRUM-MEMBRANE.

BY

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I SHALL present a resumé of this subject, as far as it has been possible, in a limited time and with the literature at my command, to obtain it, and in addition I am happy in being able to submit so much of the *present* status of the subject among aurists at home and abroad, as will be my privilege.

With the desire to present something practical on a subject of such practical importance as this, and which would carry authority with it, I early addressed letters to as many of those who are engaged in this special field as my time allowed, submitting questions which appeared to me to cover every point, so that if possible I might be able from these advanced sources to obtain the material which would admit of a crystallization into truth.

It would be desirable to know accurately the condition of the ear which indicates the employment of an artificial drum-membrane, the contra-indications, and the best form of membrane under varying circumstances. It should not be a matter of taste, like the preference of one prospect or one piece of music to another. As scientists, we should, as far as possible, have fixed laws to govern us; yet, dealing with forces which themselves vary, we recognize that this exactness is not always to be attained.

It appears that Marcus Banzer, in 1640, first suggested the use of the artificial membrane, not with the view of improving the hearing, but as a protection to the cavity of the tympanum in cases where it was exposed from loss of the normal membrane by ulceration. The construction of his apparatus was exceedingly rude, consisting of a tube cut out of the hoof of the elk, with a bit of pig's bladder to represent the membrane covering its proximal or inserted end. This was to be introduced to half the depth of the meatus. There are allusions to this apparatus, or to similar contrivances for the same purpose, in the writings of Leschevin, 1763, Autenreith, 1815, and Lincke, 1840. The appliance of Autenreith, a description of which he published in the *Tübinger Blättern für Naturwissenschaft und Arzneikunde*,[1] consisted of a short tube of lead, very thin and elliptical-shaped, the inner end of which was covered by a membrane made of the air-bladder of a fish, which was put on wet and varnished after it became dry. Lincke describes his apparatus in his treatise on diseases of the ear;[2] it is substantially the same as that of Autenreith. In 1842, Yearsley accidentally discovered an altogether new use of the artificial drum-membrane, viz., as a means of improving the hearing. I quote from a paper published in the "The Lancet" (July, 1848),[3] entitled "On Deafness."

[1] B. i. H. 2, S. 129. [2] Handbuch der Ohrenheilkunde, 1845, B. ii. S. 446.
[3] Vide Allen, "On Aural Catarrh and Curable Deafness," 2d edition, 1874, p. 366.

Up to the present time, no successful mode of treating perforations of the membrane of the drum, or the relief of the accompanying deafness, has been discovered by the profession at large. The only means resorted to has been the removal of pus or mucus from the tympanal cavity by syringing, or rendering it free by passing air through the perforation, by way of the Eustachian tube. Either of these proceedings will produce a temporary improvement of the hearing in cases where the tympanum suffers from obstruction, but in many others, when such a state does not obtain, they are of little if of any service. I have, however, the extreme gratification of promulgating a mode of relief for deafness attended by loss of the membrana tympani, which will cause great surprise among the readers of The Lancet, not less from its extreme simplicity than from the extraordinary success which generally attends its employment. In 1841, a gentleman came from New York to consult me under the following circumstances: He had been deaf from an early age, and, on examination, I found great disorganization of the drum of each ear. On my remarking this to him, he replied, "How is it, then, that by the most simple means I can produce in the left ear a degree of hearing quite sufficient for all ordinary purposes? in fact, so satisfied am I with the improved hearing which I can myself produce, that I only desire your assistance on behalf of the other ear." Struck by this remark, I again made a careful examination of each ear, and, observing their respective conditions, I begged him to show me what he did to that ear which I should have unhesitatingly pronounced beyond the reach of remedial art. I was at once initiated into the mystery, which consisted in the insertion of a spill of paper, previously moistened at its extremity with saliva, which he introduced to the bottom of the passage; the effect of which, he said, was "to open the ear to a great increase of hearing." This improvement would sometimes continue an hour, a day, or even a week, without requiring a repetition of the manipulation.

Dr. Yearsley seized upon the idea which this case presented, and gave to the profession an artificial drum-membrane which is known as "Yearsley's cotton-wool," consisting of a small pellet of moistened cotton-wool which is inserted to the bottom of the external auditory canal. Without detracting from the rude attempts which had preceded this, and without claiming for Yearsley that he had made a discovery, strictly so called, of anything previously altogether unknown, yet it is due to him and to ourselves, since discovery is a relative word, to credit him with being practically a discoverer, for bringing to our knowledge and for giving to the world this new *use* of a contrivance of which we otherwise might have remained in ignorance until now. In 1853, eleven years later, Toynbee gave to the profession another form of artificial membrane, which he described in his "Treatise on Diseases of the Ear," and which was composed of a vulcanized India-rubber disk (the thinnest layer of it that was procurable), through which was passed a silver wire, fastened in the centre by means of two very fine plates of silver, having a diameter of about three-quarters of a line. "The silver wire is of sufficient length to admit of the membrane being introduced or withdrawn by the patient, but is not perceivable externally except by special observation." I shall have to notice hereafter other forms of artificial drum-membrane, which, while deserving a special mention, are not properly referred to in this connection.

It will be interesting now to consider the conditions of the ear which admit of the use of the artificial membrane, and this will be profitably studied by passing in review the theories and practical thoughts, on this subject, of explorers in otology, from the period of Yearsley down to the present time, augmented by views which are entertained by men of to-

day. To preface this step: It is not deemed necessary to refer to the function of hearing, further than to state in general terms that the conducting media involved are the membrana tympani, the machinery of the cavity, and the membranes of the oval and round windows of the inner wall of the tympanum. Like the media through which vision is effected, there is so nice an adjustment of these forces that to have one affected injuriously, influences the efficiency of each of the others, and of the whole machinery. This need not to be demonstrated. It will be understood, then, that, in considering what are the conditions of the ear admitting of the use of the artificial membrane, we have not only to take into consideration the membrana tympani and its functions, but what are the functions of the tympanic cavity, etc., which are influenced by every deviation from the normal condition of the membrane, and *vice versâ*. The artificial membrane is designed to supply some force that is wanting, or that operates inefficiently, and this inoperative condition of the conducting apparatus which can be thus measurably remedied, may be due to a variety of causes, such as solutions of continuity of the membrana tympani; changes in the lining membrane of the tympanum, associated with perforation of the membrane; loss of power in the tension-muscles of the cavity, with partial or entire destruction of the membrane; loss of two of the ossicles, the membrana tympani being gone, the stapes *in situ;* flaccidity of the membrane; solution of continuity in the articulation of the ossicles. It may be considered, then, that it is the design to show in what follows the rationale of the functional gain by the artificial membrana tympani in these conditions, which I have enumerated in the order of their frequency of occurrence.

Toynbee reasoned that the artificial membrane "confined the vibrations to the tympanic cavity, and concentrated them upon the labyrinth." This conclusion he founded upon experiments which proved to him that the guttural orifice of the Eustachian tube was closed "except during the momentary action of certain muscles," which made the tympanum a shut cavity for all purposes relating to the passage of sonorous undulations, and next upon the experiment with the tuning-fork, which showed that the sonorous vibrations communicated to the bones of the head appeared much louder when the meatus externus was closed than when the orifice was open. This led to the conclusion that the sonorous vibrations imparted to the cavity of the tympanum, could only make their due impression on the membranes of the labyrinth when strictly confined to the tympanic cavity, and Toynbee's artificial membrane was designed to supply this, which he considered was the office of the normal membrane.

And in directing its application, he says: "As will be imagined, great care must be taken to cut the membrane so that it shall fit the inner extremity of the meatus with exactness; since if too large it would cause discomfort, and if too small it would not fulfil its purpose of rendering the tympanum an air tight cavity. It is not easy, in all cases, to fit the artificial membrane so as not to allow of any communication between the air in the tympanum and that in the external meatus; this however is the object which should always be aimed it." This view of Toynbee's did not obtain for any length of time, as practical experience with the use of the artificial membrane soon showed that, to effect the improvement in the hearing, it did not require that the perforation should be closed; and this should have been apparent earlier when we consider that Yearsley's artificial membrane of cotton-wool preceded Toynbee's rubber disk, and the increase of hearing effected by it could not be accounted

for by Toynbee's mode of reasoning. As far as I know, Erhard and Von Tröltsch were the first who suggested that the effect of this mechanical aid to hearing did not depend upon its rendering the tympanum a closed cavity, nor upon its affording a surface for the reception of sound, nor upon its furnishing a conducting medium, but that it acted by virtue of its pressure upon the stapes. This was first concluded when it was discovered that in this position the greatest amount of improvement was effected, and no philosophical explanation was essayed. It is but doing Toynbee justice to add that his views subsequently underwent considerable modification, as will appear from a statement of the conditions of the ear in which the artificial membrane will be found most efficient, given in the supplement to the edition of his work, edited by Mr. James Hinton,[1] and which he was partly led to adopt, it is stated, by intercourse with Dr. Julius Erhard, "who, without any perforation, found his hearing much improved by the use of cotton-wool, and published his experience in a paper entitled 'Deafness Curable by Pressure.'" These conditions are as follows:—

(1) Disconnection of the incus and stapes, the membrana tympani being entire, but the tensor tympani ligament, or the mucous membrane of the tympanum, being relaxed. (2) Partial or complete absence of the long process of the incus, the membrana tympani being entire. (3) Disconnection of the stapes and incus, the membrana tympani being perforated, and the tensor ligament or the mucous membrane of the tympanum being relaxed. (4) Partial or complete loss of the long process of the incus, the membrana tympani being perforated, and the ligaments of the stapes being relaxed.

Erhard[2] says:—The improvement in the hearing is appreciated at the moment that the surface of the artificial membrane which is being introduced comes in contact with the surface of that which was formerly the conducting apparatus, restoring the continuity which has been destroyed.

Von Tröltsch[3] says:—It is not yet determined definitely in which way the artificial membrane produces its often astonishing effect. Various explanations of its mode of operation are in favor. According to Lucæ, it is the increased pressure which is exercised upon the labyrinth-fluid, which is of outweighing importance to everything else. Politzer refers the improvement in the hearing to the removal of the irregular nodes (*Schwingungsknoten*) which are produced in every perforated membrane—and also to the rubber disk acting like the true vibratory membrane, and serving to convey a considerable number of vibrations to the ossicula. In the least number of cases is the increased hearing to be referred to the closure of the cavity, which was Toynbee's theory. In many instances we observe marked improvement where the rubber disk does not effect closure of the opening in the membrana tympani. . . . . That the closure of the opening is not essential, and not the true explanation in solution of this enigmatical question, I was able to see in a case that presented itself to me where I could close a very small opening in the membrane with collodion, or a thick solution of gum, without producing the least effect upon the hearing; whereas a very marked and immediate improvement resulted from the introduction of the rubber disk, or of any other foreign body, so that it would press upon the membrane. This pressure upon the membrane, or upon the manubrium mallei, seems in most cases to be essential to effect the desired result.

Politzer, in a very interesting *brochure*, entitled "Ueber das künstliche Trommelfell und seine neueren Modifikationen," which first appeared as an

[1] 1868. p. 452. [2] Krankheiten des Ohres, 1875, S. 230.
[3] Lehrbuch der Ohrenheilkunde. Fünfte Auflage, 1873, S. 400.

article in the *Wiener Medizinal-Halle*, 1864, after a statement of the views of Toynbee, says:—Although the phenomena upon which he founds his explanation present themselves to every practical physician as correct observations, on the other hand, it must be allowed that they cannot altogether clear up the question of mode of action of the artificial membrane. Opposing views to those of Toynbee have been urged by other authors (V. Tröltsch, Erhard), and are in more general favor at the present time. It may be stated positively that the view entertained by the latter author, *i. e.*, that the artificial membrane operates principally through pressure upon the remnant of the membrane and the chain of bones, is the true explanation in the majority of cases; this is more evident when we regard the pathological changes which occur in the tympanum as a result of the catarrhal process, and which have been more fully appreciated since Toynbee made his observations upon this subject. If a post-mortem examination be made of a person who has suffered from a suppurative, middle-ear catarrh, with perforation of the membrane, there will be found in many cases that the ossicles are fixed through adhesions, or are enveloped in adventitious tissue-formation. In other cases the ossicles have become so loosened in their articulations from the inflammatory process, that the slightest touch with an instrument causes them to fall apart. It is evident that in such a looseness of the articulations of the chain of bones, the sound-conduction would be interfered with in greater or less degree; especially would this be the case if there were complete separation, for instance, in the incudo-stapedial articulation. In a case of this kind, now, the artificial membrane would operate to improve the hearing through pressure, by restoring continuity in the chain of bones and aiding sound-conduction.

The above-cited observations of Toynbee, upon which he rests his theory of the mode of action of the artificial membrane, permit of another solution to this question. If the membrana tympani is perforated, the deafness which follows depends, *first*, upon the hole in the membrane by which the surface for the reception of sound-waves is diminished, and consequently a less number of sound-waves are conveyed to the chain of bones: particularly is this the case if the destruction of the membrane is in the neighborhood of the manubrium mallei; *second*, upon change in the conditions surrounding the ossicles, as by accumulation of secretion, or by loosening or fixation of their articulations. Upon the latter changes depends the degree of deafness, as experience proves that in large perforations, where no considerable change has occurred in the condition of the chain of bones, there is, of course, a diminution in the hearing, but yet the hearing will still often be sufficient for ordinary purposes, and the deafness never is in degree proportional to the extent of the defect in the membrane. This phenomenon, which is so striking, has its explanation in this, that in the large perforation, the sound-waves, without being received and conducted by the remnant of membrane, and malleus and incus, fall direct and full upon the foot-plate of the stapes and the membrane of the round window, and so reach the labyrinth in considerable numbers. The improvement in the hearing, in cases where the perforation is not extensive, which is effected through the closure of the opening by means of a drop of fluid, has its explanation in this fact, that, the continuity in the membrane being restored, the irregular nodes are removed which exist in every case where there is an opening existing in the membrane.

Moos[1] says:—According to the observations of Lucæ, Politzer, and myself, it has been proven that, by the application of the artificial membrane, the intra-auricular pressure is increased. There is at the same time this advantage, that in some cases in which the ossicles are still preserved, but are loose in their articulation, which has resulted from an active, purulent inflammation of the middle ear, the artificial membrane, by pressure, restores them to a proper condition for the conduction of sound-waves.

[1] Ohrenkrankheiten, 1866, S. 275.

Dalby[1] says:—The increased hearing may therefore be said to be produced by approximating the articulations of the ossicles, or supplying their place when these bones are partially wanting; which Dalby further states was in substance the explanation offered by Dr. Yearsley.

Roosa[2] says:—The improvement to the hearing is probably due to the restoration of the interrupted continuity of the ossicula auditus to the fenestra ovalis and the labyrinth.

Hinton[3] says:—The question whether the artificial membrane operates by closing the orifice in the membrana tympani, or by supporting the ossicula and especially the stapes, is now decided in favor of the latter view.

Allen[4] says:—The benefit is derived from support being given to the ossicula, by which they are enabled to exercise that due pressure at the fenestra ovalis which keeps the membrane of the fenestra rotunda in a condition susceptible of vibrations, and capable of transmitting them to the nerve expansion in the labyrinth.

In response to inquiry, I have obtained, relative to this question of conditions of the ear and function of the artificial membrane, these further very interesting views. (I would state that I did not submit these questions to many whose views would be highly esteemed, for reasons which have already been assigned, and regret it as detracting from the value and interest of this report. Three to whom the questions were submitted, made no reply.)

Dr. Clarence J. Blake, of Boston, summarizes the conditions of the ear in which the artificial membrane may be used, thus:—(1) Solution of continuity of the membrana tympani, in which the artificial membrana tympani restores a surface for the reception of the sound-waves, or, by closing a small opening, prevents the simultaneous impact of the sound-waves upon the base of the stapes and the remaining portion of the membrane, a condition which is shown by experiments to result in interference;[5] (2) Thickening of the mucous membrane of the tympanic cavity, associated with perforation of the membrana tympani, the increased pressure given by the artificial membrane resulting in the readier conduction of short sound-waves; (3) Destruction of the membrana tympani, the artificial membrana tympani receiving the sound-waves on its outer surface, and conveying them directly by contact to the incus or stapes; (4) Loss of the incus in suppurative otitis media, the artificial membrane being made to press directly upon the stapes, thereby restoring the degree of tension of the membrane of the fenestra ovalis. ("I have seen in Politzer's clinic good results in such a case from the pressure induced by a small cube of rubber-elastic, one millimetre in diameter, resting upon the stapes and extending across the tympanic cavity.")

Dr. D. B. St. John Roosa, of New York, states that, according to his experience, the artificial membrana tympani may be used where there is a partial or complete loss of the natural membrana tympani, with a moderate amount of suppuration, or no suppuration at all, and with one or more of the ossicles *in situ*. If there be much suppuration, the irritation of a foreign body, such as an artificial drum-head, will always be apt to increase it. In a few rare cases it is useful, where the natural membrane is entire but very much thinned by atrophy. Its use should be limited to those cases where with the above conditions there is a great impairment of hearing, *i.e.*, inability to hear ordinary conversation. . . . The improvement in hearing is effected primarily by pres-

1 Diseases and Injuries of the Ear, 1873, p. 127.
2 Treatise on Diseases of the Ear, 1873, p. 380.
3 Aural Surgery, 1874, p. 189.
4 On Aural Catarrh and Curable Deafness, 1874, p. 370.
5 C. H. Burnett; Weber-Liel, Func. d. Memb. d. runden Fensters; Test with high musical notes, Moos, Archiv f. Ophth. und Otol.; High Musical Tones, C. J. Blake; Sprache und Ohr, O. Wolf.

sure upon the stapes, re-establishing the proper relations of that bone to the fenestra ovalis, vestibule, and cochlea.

Dr. O. D. Pomeroy, of New York, says:—Any condition in which the base of the stapes fails to exert the requisite pressure upon the oval window, and consequently upon the waters of the labyrinth. The lesions resulting in this condition are: (1) Perforation or loss of membrane; (2) Flaccidity of the membrane, either due to a cicatrix or to other causes whereby the membrane fails to close down firmly upon the ossicles; (3) Loss of one or two of the ossicles, the stapes being *in situ;* (4) Separation from any cause of the ossicles from each other; (5) Partial luxation of the base of the stapes, or great relaxation of its annular ligament. The discharge from the ear must be moderate in quantity or absent. Polypi or granulations should be removed or destroyed. The ear must bear the pressure of the membrane without the production of either pain, tinnitus, vertigo, nausea, etc., increased discharge, or irritation of the chorda tympani.

Dr. Arthur Mathewson, of Brooklyn, says:—Like the natural membrana tympani, the artificial serves to concentrate the force of the sound-waves through the ossicles on the nervous apparatus of the inner ear, focalizing these waves, as it were, as rays of light are brought to a focus on the retina by the refracting apparatus of the eye. In cases in which there is stiffening or anchylosis of the joints of the ossicles, so that they cannot transmit the sound-waves, the artificial membrane only impairs the hearing.

Dr. Albert H. Buck, of New York, says:—As I comprehend the matter, the artificial drum-membrane serves to intensify the sound-impulses. In the case of Hassenstein's membrane, I can readily understand how the vibration communicated to such a solid body, projecting as it does beyond the orifice of the meatus, may be transmitted to the hammer, and so on to the labyrinth, with a greater force or intensity than would have been the case had the same waves of sound reached the auditory nerve without the intermediation of such a solid body—the larger solid body offering a greater surface to the waves of sound than does the hammer which is a smaller body. If there is a condition of anchylosis, an entire membrana tympani would, it seems to me, be a much less efficient intensifier of sound-impulses than would Hassenstein's contrivance. Hence, where there is anchylosis, it is quite possible that the total absence of the membrana tympani may be rather a benefit than otherwise; for, in its absence, the sound-impulses can strike directly against the foot-plate of the stirrup (in other words, upon the fluid of the labyrinth) with greater force than they could *viâ* the chain of ossicles, the membrana tympani being intact. The temporary benefit of artificial perforation, in such cases (of anchylosis with entire membrane), may be explained in accordance with this view.

Dr. C. H. Burnett, of Philadelphia, in reply to the question "What is the office of the artificial membrane; how is the improvement effected?" says:—This question is one not only of intense interest, but of the greatest importance, for upon its answer depends, as it seems to me, the gist of the entire subject of application and usefulness of the false drum-head. Its office is emphatically to restore the proper isolation of the ossicles of hearing. These will not perform their normal functions if they are too tightly locked, or if they are too loosely connected. Hence, in some cases, the false drum-head would have to be adjusted so as to gently overcome the indrawing of the malleus by the tensor tympani, while in other instances the benefit derived from the false drum-head would be entirely due to pressure inward, since it would thus overcome a too great relaxation of the joints of the ossicles, and thus enable them to swing with each delicate wave of sound. The latter is more easily accomplished, and perhaps is the more usual demand made on the false drum-head. But, now and then, the more the false drum-head is made to press on the remnants of the sound-conducting parts, the less the patient hears. In the cases of this kind which I have seen, the membrana tympani has been largely destroyed, but the ossicles have been present, and their attach-

ments normal, especially that of the tensor tympani to the malleus. Manifestly, here the bones have been already too much drawn inward, and pressing down a pellet of cotton would but increase the defective isolation. If, however, in such cases the artificial membrana tympani be placed so as to gently overcome the indrawing of the malleus, the entire chain of ossicles may be freed, and allowed once more to swing normally. This I have successfully done by tucking the upper edge of the round or flattened pellet of cotton *behind* the manubrium, *not in front of it.* The latter would be only an additional inward force, but the former adds *outward* force just enough to overcome the retraction of the malleus. The base of such a false drum-head should rest as nearly as possible along the line of insertion of the lower half of the membrana tympani. In doing this, should some of the lower portion of the artificial membrane extend into the tympanum, as it almost surely will when the membrana tympani is largely destroyed at its lower border, no harm comes of it, if there be no discharge from the ear when the artificial drum-membrane is inserted, and if the ear be examined by a competent person from time to time in order to learn the condition of the parts where the cotton pellet lies. The great objects to be gained in such cases of undue retraction of the malleus, and consequently of the entire chain of ossicles, are: (1) a good support for the base of the false membrana tympani; and (2) the adjustment of its upper edge *behind* the lower end of the manubrium. Of course, large perforations involving the lower half of the membrana tympani are supposed in what has just been said. A small perforation does not as a rule cause as much deafness as a large one, and in any event the artificial membrane used in the former is to be laid over the perforation.

Prof. Schwartze, of Halle, says:—The indication for employment of the artificial membrane exists only in a partial or total defect of the membrana tympani, and it is contra-indicated by a subacute inflammation, or a profuse otorrhœa.

Prof. A. Lucæ, of Berlin, writes:—The artificial membrane should not be employed, and is of no value, in ancient perforations where the suppuration has ceased, and where the edges of the perforation have cicatrized.

Prof. Politzer, in his communication to me, says:—It is important for the action of the artificial membrane, as well the rubber disk as the pellet of cotton, that it should be fastened on a solid or elastic stem, by which a certain pressure is exercised upon the deeper parts. The rubber disk or the cotton pellet which is fastened on a *thread*, will not produce so marked an effect, because this pressure from without is not then exerted.

Prof. Weber-Liel,[1] in a preliminary communication, states that according to his researches the band connecting the foot plate of the stapes with the edge of the oval fenestra, described heretofore as the "ligamentum orbiculare baseos stapedis vestibulare," should be considered rather as a circular membrane. It is true that it appears on ordinary examination only as a very narrow band, yet well prepared microscopic specimens, more than physical experiment on fresh preparations, demonstrate that we have to deal with a *relatively broad, independently vibrating, membranous band.* On account of the peculiar position and mode of union of the stapes with the edge of the oval window (the membrane generally going abruptly down to the edge of the stapes looking toward the tympanum), it is only under certain conditions that we obtain a demonstrative view. In well-prepared specimens, where the covering mucous membrane of the tympanum has been previously removed, well-defined, radiating fibres can be observed in the circular membrane. The elastic-fibre layer of Rüdinger, between the so-called "ligamentum orbiculare baseos stapedis vestibulare" and the "ligamentum baseos stapedis tympanicum," constitutes the real, very elastic attachment of the stapes-plate in the foramen ovale. In illustration, there was presented, with this communication, a preparation of the

[1] Verhandlungen der physiologischen Gessellschaft zu Berlin. 3 Juni, 1876.

human ear in which the wall of the vestibule opposite to the oval window had been opened, the edge of the opening having been filed and widened to admit a thin glass, which was made to close the opening hermetically, and through which the circular membrane could be closely observed, enlarged 15–40 times. In the external auditory canal there was loosely placed a glass tube, to which was attached a piece of India-rubber tubing, through which sound-waves could be introduced. At once, very low speaking, or singing high and low tones, into the tube, produced plainly visible excursions of the light reflexes on the circular membrane, but none whatever on the stapes-plate. The light reflex excursions were very marked with stronger and lower sounds, but only very loud singing into the tube produced the light reflex excursions on the stapes itself. The tensor tympani and stapedius muscles appeared to effect the changes of tension in the circular membrane. It was shown how, by pulling on the stapedius muscle, the vibrating faculty of the circular membrane was at certain points reduced or entirely checked. The counter-pressure of the labyrinth-fluid was replaced in the preparation by air, which could be compressed by means of a rubber balloon through a glass tube introduced into the closed vestibule, a piece of rubber tubing connecting the distal end with the air bag.

I am indebted to this author for a personal communication, in which he applies this newly discovered anatomical relation to the question we are considering: "The stapes, regulating the intra-labyrinthine pressure, exercises also an influence upon the circular membrane through the operation of the sound-waves and the tensor tympani and stapedius muscles. If the normal tension of the membrana tympani and chain of bones and muscles be impaired, in consequence of defect in the membrana tympani, it is perhaps the pressure which is exercised by the artificial membrane on the stapes to which the improvement in the hearing can be referred. The intra-labyrinthine pressure and the tension of the circular membrane may by this means be brought nearer to a normal condition." In connection with this interesting statement of Prof. Weber-Liel's, I may refer to the experiments performed by Mach and Kessel,[1] conducted with a view to determining "whether a traction upon the muscles of the tympanum will produce an alteration in the consonance of the auditory apparatus for notes of various pitch," and as a result of which they state, "our conclusions are in unison with those of former investigators. All observers agree that contraction of the tensor tympani, whether produced artificially or by natural causes, will render high notes more audible than low ones." This phenomenon has presented itself, I do not doubt, very many times to every gentleman present, in the course of treatment of suppurative and non-suppurative inflammations of the tympanum, there being at periods a marked improvement in the hearing of high tones, with no increase, or a positive decrease, in the hearing of low tones.

We may now consider the contra-indicating conditions to the use of the artificial membrane. After what has gone before, I shall consider that a mere statement here will be sufficient, and will not be controverted by any. In brief, then, the contra-indications to the employment of the artificial membrane are found (1) in a profuse suppuration; (2) in the presence of any adventitious growth in the tympanum or external auditory canal; (3) in acute disease; (4) where there is pain, or where there are cerebral symptoms; (5) where the artificial membrane has been tried and found to produce irritation; and (6) in all cases where there does not exist a degree of intelligence sufficient for the self-introduction and

[1] Versuche über Accommodation des Ohres; Sitzb. der k. k. Akad. der Wiss., 1872.

removal of the artificial membrane, or for the formation of a judgment as to the indications for its continuance or discontinuance, and in cases of young children.

The *forms of membrane*, and I shall enumerate only those which have met with some favor, and are in use among aurists to-day, are as follows:—(1) Yearsley's cotton-wool. (2) Toynbee's[1] rubber disk. (3) Hassenstein's (of Gotha) modification of Yearsley's membrane, made by attaching it to a pincette for its easier introduction and removal, and in order to supply an increased pressure. (4) Lucæ's modification of Toynbee's membrane; consisting in the removal of the metallic button and wire, and in the substitution of a fine rubber tube attached by a solution of gum to the outer surface of the disk; the tube serves for its introduction by means of a probe. (5) Politzer's artificial membrane, which is cut out of the wall of a heavy India-rubber tube; it is from four to five lines long, and should be one and a half or two lines thick; the upper end, through which the wire of Toynbee's membrane is passed, is somewhat broader than the lower. It is recommended in charity practice, for the readiness with which it is made, and its comparative cheapness of cost. Politzer has further modified Toynbee's artificial membrane by fastening to the disk an artificial stirrup, to be used in cases where the head, neck, and crura of the stapes have been destroyed, and only the foot-plate remains in the oval window. The foot-plate must be susceptible of its normal movements. (6) Gruber overcame the danger which is liable to occur in the use of Toynbee's membrane, of the disk becoming separated from the stem and being left behind in its attempted removal, by attaching a thread to the disk, and also to the ring which is formed in the outer end of the wire, for its extrication in the event of such an accident. (7) Hartmann[2] has proposed an artificial membrane, to be made of whalebone, six to seven centimetres long and one to two millimetres broad, and as thin as possible. It is doubled upon itself to form a loop, after it has been wrapped with cotton. It is recommended for the elasticity of the pressure which it makes, for its easy introduction and removal, and as a means of treating the cavity.[3] (8) Blake[4] recommends the use of a disk of writing paper, to be employed only in a certain class of cases; at the same time that it improves the hearing, the disk has its chief value in its office as a therapeutic agent.

The conditions of the ear presenting the indications for the employment of the artificial membrane, which have been stated at such length, occupying the greater part of the time allotted to this paper, with the anatomical peculiarities of the location, constitute the argument upon which is based the preference for Yearsley's cotton-wool. Of all the forms of membrane which may be committed into the hands of the patient—and it should be kept in mind that this is its fate—this form, in my experience, has proved to be the simplest of introduction, the easiest of adaptation in the cavity, and much the least likely to produce irritation—which latter must be the constant fear of every intelligent wearer of such a contrivance. In its employment, it will not be forgotten that the artificial membrane (of whatever form) is a foreign body; and that, in those cases in which it will produce the greatest amount of improvement in the hearing, it will be liable to defeat what

[1] For description, see page 975.
[2] Archiv für Ohrenheilkunde, Band xi., Heft. 2.
[3] See page 985.
[4] See page 985.

should be our first concern, the arrest of a process which endangers the health of an individual, if not his existence, and compared to which the improvement in the hearing is a matter of very secondary importance. The high degree of deafness which so frequently obtains in chronic non-suppurative inflammation of the middle ear, is comparatively of rare occurrence, associated with suppuration in the cavity; and where it exists, we are apt to find more or less extensive destruction of the tympanal machinery, with the destructive process continuing. These are the cases which when first presented necessarily excite some apprehension in our minds, and which require a guarded prognosis. They are cases concerning which I have been led by sad experience to feel an unusual solicitude; and when after a patient care of months, it may be, such an ear has been recovered from this state of danger to a comparatively healthy condition, it is a question for the surgeon to decide, "Have I a right to hazard this result?" When the continued use of this mechanical aid to hearing has been determined upon—and circumstances at times will warrant such a decision—the question arises, What form of membrane shall I employ? And I care not what the degree of deafness, nor what the circumstances or condition of the patient making it desirable to have the functional relief, the conclusion cannot be influenced alone by the degree of improvement produced by this form of membrane or that, but the surgeon must also consider his responsibility in the case, and his duty to protect his patient, and he will put into his hands that engine which will be the least calculated to do him harm. If the rubber disk be used, it should be Lucæ's, and never Toynbee's with its metallic button.

I am further of the opinion that such a case should never pass entirely from under observation, but that the patient should report himself statedly for inspection to the aural surgeon. It is not without significance that some of those of enlarged experience and of honor among us, if they do not discountenance the use of the artificial membrane altogether, at least may be said to endorse its use, in any form, very qualifiedly. Among such are Lucæ and Schwartze, of Continental-European aurists, and Buck, J. Orne Green, Knapp, Mathewson, and Prout, of those in our own country.

Lucæ says:—It is well known that in a continuing, chronic suppuration, the artificial membrane improves the hearing in a high degree; yet it irritates, and retards the cure of the otorrhœa, so that it would appear more rational to entirely abstain from its use, and principally to direct the attention to controlling the suppurative process. I have, therefore, in my practice, restricted myself in the use of this appliance to those very rare cases in which it effects a marked improvement in the hearing, without at the same time producing any irritation whatever.

Schwartze says:—As a general thing, I may state that, in my experience, the artificial membrane is only in rare cases to be used as a means of improving the hearing. In most instances, the improvement is not so considerable that the patient will bear the trouble and inconvenience which necessarily attend wearing it; and, continued for a little while, it is soon thrown away.

J. Orne Green says:—I must say that my experience with them is not brilliant; occasionally, in *very* rare cases, they have improved the hearing, but even in these they are often not well borne.

Knapp says:—My own experience with artificial drum-heads has been so unfavorable that I hardly ever try them again.

The functional value of the artificial membrane has been sufficiently considered; there remains something to be said of its therapeutical uses, and I incline to the opinion that this is its office of greatest usefulness. It serves, in the first place, to protect the inflamed cavity from the pernicious influences of an unkindly atmosphere, and from particles of dust and soot; and, introduced to the depth of the meatus, in more or less profuse suppuration, may absorb the vitiated secretion,[1] and act as a slight irritant, where there remains a small perforation in the membrane with a subsidence of the middle-ear trouble, to excite a further reproduction of membrane and thus close the cavity. In the second place, through this instrument, astringent solutions may be retained in application to the cavity, often with great advantage. It is proper, however, to state here a precaution which may save others from an embarrassment to which I have been subjected. The cotton-wool should be moistened merely, never saturated, with the solution; as the continuous presence in the cavity and meatus of so much fluid might lead to the development of a myringo-mycosis. Did time allow, I might report a number of cases so treated. Prof. Politzer, in a letter, suggests a different method of employing the cotton-wool with the medicated solution. I shall give it in the connection in which he states it, as the whole subject is of interest:

The most important modification, since the year 1864, is that of Dr. Hassenstein, of Gotha, who has contrived a pincette in which the cotton pellet is fastened. It possesses advantages over Toynbee's artificial membrane in that (1) its introduction is less disagreeable to the patient; (2) In speaking and chewing, there is less often that disagreeable noise in the ear which accompanies the use of Toynbee's membrane; (3) It does not produce so much irritation; (4) The cotton takes up the secretion of the cavity; (5) By means of the cotton, which has been saturated in a concentrated solution of sulphate of zinc, sugar of lead, nitrate of silver, or alum, and then dried, and moistened again when it is to be used, a medicament can at the same time be retained in contact with the diseased mucous membrane of the middle ear. . . . In the same manner Dr. Arthur Hartmann's artificial membrane might be employed.

Dr. Clarence J. Blake informs me:—Lately, in cases of small perforations of the membrana tympani, the middle-ear trouble having subsided, I have covered the perforation with a small disk of writing paper, moistened; the sizing of the paper is sufficient to secure its adherence, and in some cases it has remained for several weeks; the slight irritation caused by the paper favors closure of the perforation by cicatrization, and the paper is then removed by the natural growth of the dermoid coat of the membrana tympani. Dr. Blake kindly reports a case to illustrate the effect of this disk of writing paper, taken from the Massachusetts Charitable Eye and Ear Infirmary's Record, occurring in his service:—

Case 2024, Vol. V.—Perforation of right membrana tympani from old otitis media purulenta, discharge having ceased; hearing for voice diminished, sound of the voice being heard at six feet, but not understood readily on account of loss of softer consonant sounds. Perforation in anterior segment, a little more than two millimetres in diameter. Disk of common note paper moistened, and applied over the perforation. Voice heard plainly, and conversation understood at twelve feet (June 6, 1876).

[1] I have been in the habit of using, for making artificial membrane, a prepared cotton from which the vegetable oil has been extracted; this substance answers well for the purpose, and is also a better vehicle than ordinary cotton for conveying medicated solutions to the tympanum.

July 1, 1876. Paper still in position, but has moved (by natural process of repair) one millimetre backward and downward. Perforation diminished in size.

July 18. Paper having been moved from over perforation, is reapplied, with a repetition of the former favorable condition.

Finally, it is not easy to determine in what percentage of cases the artificial membrane will prove of practical advantage, until it shall have been tried in every case of perforation of the membrane, including cases of one-sided deafness. We are ready to infer that the relative proportion to the whole number of chronic suppurations would be exceedingly small, where its use would be continued as a means of improving the hearing. Of 261 cases of ear disease presented during the last six months, taken apart from clinical cases, as affording opportunity for more careful observation, 34 have been cases of chronic suppuration of the middle ear —the right ear being affected 11 times, the left ear 8, both ears 15. Counting out all excepting the 15 cases of double-sided suppuration, for reasons that will appear manifest, but five of these have presented the warrantable conditions for the employment of the artificial membrane, and only one has continued the use of the artificial membrane after being left with it for self-introduction.

Dr. Blake has placed me under further obligations to him by furnishing these statistics:—

The total number of cases of otitis media purulenta, treated at the Massachusetts Charitable Eye and Ear Infirmary from Jan. 1 to August 5, 1876, was 267, divided as follows: Males 187, females 80; Right ear 95, Left 90; both ears 82. In several of these cases, artificial drum-membranes were tested, no record being made. I append notes of cases in which there was special record.

Case 1928, Vol. V. Otitis media purulenta of both ears; destruction of membrana tympani, only a rim remaining; patient hears voice, except immediately, with difficulty; cotton applied in both cases with great improvement in hearing for voice.

Case 1982, Vol. V. Otitis media purulenta; hearing for voice much diminished; wall clock (60 feet) not heard at 6 feet. After introduction of cotton drum, hearing for voice improved; wall clock heard plainly at 20 feet; noises in the street, unnoticed before, became very annoying. After remaining in place with continued improvement in hearing for four days, drum removed to allow of treatment of granular condition of tympanic mucous membrane.

Case 208, Vol. VI. Destruction of left membrana tympani. Malleus drawn inward in contact with promontorium. Incudo-stapedial articulation visible. Voice understood with difficulty at 3 feet. Cotton drum introduced, being so moulded as to present a convex surface inward, pressing on the incudo-stapedial articulation, and a plane surface outward. Voice understood readily at 6 feet.

Conclusions.

I. Of the various forms of artificial drum-membrane in use, the cotton pellet is preferable, for its greater simplicity and easier introduction, for comparative safety in its employment, and for the greater uniformity of its effect.

II. It has an advantage over all other forms of artificial drum-membrane, in that, over and above the functional gain which may be derived from it, there may be added its value as a means of treating the tympanum; and this therapeutical use of the artificial membrane has a great future in otology.

III. The continued use of the artificial drum-membrane, as a means of improving the hearing, is indicated in rare conditions, which can only be determined by the aural surgeon.

DISCUSSION ON DR. SPENCER'S PAPER.

After the reading of the preceding paper, Dr. SAMUEL J. JONES, of Chicago, said:—In some cases I have found the rubber disk of considerable service, but latterly I have used the cotton pellet almost exclusively, occasionally anointing it with cosmoline. Even in some cases of active suppuration, if the secretion be carefully removed, the cotton drum-head may continue to be used with advantage. This remark applies especially to the cases of those who must continue to hear in order to attend to their daily work. Such cases, however, should remain under the observation of the surgeon.

Dr. CHARLES H. BURNETT, of Philadelphia, said:—I believe that putting the cotton behind the malleus, when the perforation is large, is a great deal better in some cases than pressing it on the malleus, for there are some cases of large perforation in the drum-head in which deafness is the result of retraction of the malleus. In such cases, the malleus being retracted, the rest of the bones are pulled in, and the trouble is only increased if the artificial membrane pushes the malleus inward.

Dr. SPENCER said:—I beg to thank Dr. Burnett for his suggestion; it may be the key to an explanation of the failure of the cotton pellet in some cases.

MODIFICATIONS OF THE METHODS OF TREATING CHRONIC NON-SUPPURATIVE INFLAMMATION OF THE EUSTACHIAN TUBE AND MIDDLE EAR.

BY

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The method of inflating the Eustachian tube and middle ear, introduced to the medical profession by Politzer, possesses so many advantages for that special purpose as to have been almost universally adopted in the treatment of obstruction of the tubes, and of disease of the middle ear. Experience, however, has shown that violence may be done by the injudicious use of so large a rubber bag as that advised by Politzer. More especially is this likely to occur when only one of the Eustachian tubes is affected. The other tube being normally free, a greater amount of force is exerted, on pressing the bag, by the air on the well ear, than can be employed with safety, even though that air should not be medicated. If, however, in those numerous cases where local stimulation, in addition, is needed, medicated air be used, still greater disadvantage may result. By this method of medication of the mucous membrane lining the air-passages, a greater impression will be made on the naso-pharyngeal part of it than on that portion lining the affected tube and middle ear, although the latter portion of the membrane needs it most. If the large bag be used, in connection with the Eustachian catheter, to introduce either pure or medicated air into the middle ear, the size of the bag is an inconvenient one, and often makes the procedure painful. The amount of force usually employed with this large bag, in the treatment of children, frequently alarms them, even if pain be not produced, and renders it more difficult to treat them.

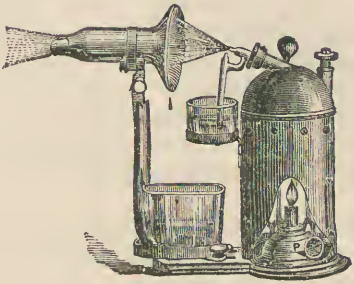
If the nasal douche be used to remove the hardened secretions often found to be present in the naso-pharyngeal space, and obstructing the mouths of the tubes, it is open to the objection that the saline solution employed may be stronger than it would be safe to have enter the middle ear, through the Eustachian tube, as it is likely to do if either the reservoir be placed too high, or the soft palate be removed from contact with the posterior wall of the pharynx during the operation. Acute inflammation of the middle ear, with occasional perforation of the membrana tympani, has thus been produced. If the soft palate remain in contact with the pharynx, that portion of its posterior aspect which is in contact, and which is very often granular and covered with altered secretion, is prevented by its position from being washed by the current as the other parts of the membrane are, thus rendering the application only partial.

In the use of the Eustachian catheter, if hard rubber be the substance of which that instrument is made, it necessitates a variety of shapes of the catheter to meet the great variations found in nostrils. Unless the proper size and right curve of the instrument be obtained, pain will be produced, and failure to reach the tube will frequently result. Heating

the catheter to give it the required curve, delays the operation, to avoid which it is necessary to have a number of different catheters. If alloyed silver catheters be used, they are too firm to have their curve readily changed, and in consequence require the surgeon to keep on hand several of them of different sizes and shapes. If Eustachian bougies be used to open the tubes, their introduction is generally painful and attended with some risk, whilst the results obtained are too often uncertain and unsatisfactory.

With a view to avoid objections incident to the use of these different instruments, I have been accustomed to substitute for the nasal douche a steam atomizer (Fig. 1), using the vapor of some alkaline solution, as

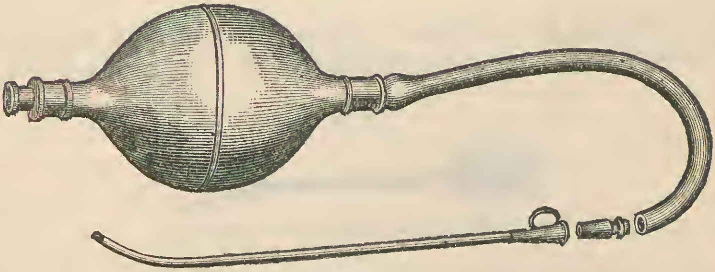
Fig. 1.



Steam atomizer.

of the chlorate of potassium, or of borax, which the patient breathes in through his mouth and expels through his nostrils—thus getting the effect of the warmth, the moisture, and the alkali, to cleanse the mucous membrane of its secretions, and to assist in removing the inflammation. Having thus cleansed and prepared the mucous membrane for further medication, a Eustachian catheter of pure silver (Fig. 2), readily adjustable to the curve required, is introduced. To this is attached, by means of a small hard-rubber tip and an elastic tube, a small rubber bag (Fig. 3),

Figs. 2 and 3.

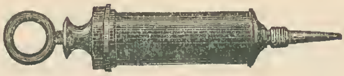


Eustachian catheter and valve-bag.

with a valve such as is used in the local-anæsthesia apparatus. The ordinary "diagnostic tube" ("otoscope," Toynbee) is then introduced into the external meatus of the patient, and into that of the surgeon. Pressure on the valve-bag forces air through the catheter into the middle ear, if the Eustachian tube be open, as is readily recognized by the sound produced. This bag has the advantage of always filling in one direction,

thus avoiding the drawing of mucus into the catheter. If the tube be not open, a small hard-rubber syringe (Fig. 4), with its tip covered with

Fig. 4.



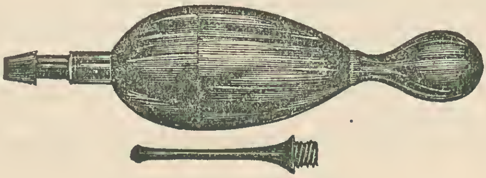
Hard-rubber syringe.

a piece of rubber tubing to adjust it to the expansion of the catheter, is filled with a warm and slightly alkaline solution, with glycerine added, and is inserted into the catheter. With this, the solution is forced into the Eustachian tube, using the liquid as the mechanical means of distending the tube, instead of relying simply upon compressed air (as in the methods of Valsalva and Politzer), or of using bougies.

Experience has shown that if the solution used be not of too irritating a character, and if its temperature be nearly that of the blood, no injurious effects follow its passage into the middle ear. On the contrary, it assists, by dissolving them, in removing the secretions that have frequently dried and remained accumulating in the middle ear and mastoid cells.

Having thus opened the tube, and dissolved the dried secretion, air impressed with iodine may be forced through the tube until the middle ear, and even the mastoid cells, may be impressed by it. This can be done by placing a few crystals of iodine between two pieces of sponge, slightly moistened and inclosed in a hard-rubber bulb (Fig. 5), (such as

Fig. 5.



Buttles's hard-rubber bulb.

Buttles's), attached to a catheter. Air is forced through the sponge by means of the valve-bag above described (Fig. 6). When the lining of the

Fig. 6.

Valve bag and hard-rubber bulb combined.

tube and the middle ear have thus been sufficiently medicated, the catheter may be withdrawn, and a blunt nozzle attached to the bulb containing the iodine. If this nozzle be placed in one nostril, the other one having been closed by pressure, medicated air can be forced in, in the act of swallowing water, as is pure air in Politzer's method—impressing the whole membrane more thoroughly, by means of a smaller bag than that used by Politzer, and with less attendant risk.

By the method here pursued, the surface is readily cleansed, and the

requisite amount of stimulation of the affected parts may be easily and painlessly obtained. Such cleansing and stimulation seem to be the basis of the most successful treatment of this form of inflammation of the middle ear, and thus better results may be obtained than are usually expected in these cases.

The modifications proposed consist, therefore :—

I. In the substitution of the steam atomizer for the nasal douche, to remove secretions from the naso-pharyngeal space and from the openings of the Eustachian tubes, and to reduce inflammation.

II. In the substitution of pure silver catheters for those of alloyed silver, or of hard rubber.

III. In more frequent use of the Eustachian catheter as a means of medicating the Eustachian tube and middle ear.

IV. In the substitution of a warm, slightly alkaline solution, forcibly injected by means of a syringe through a catheter, to open the Eustachian tubes in certain cases, instead of condensed air or bougies.

V. In the substitution of air, impressed with iodine, for irritating liquids, as a topical application to the mucous membrane lining the naso-pharyngeal space, the Eustachian tube, and the middle ear, this medicated air to be introduced through the Eustachian catheter.

VI. In the substitution of a small rubber bag with a valve, designed to inflate the Eustachian tubes, for the larger bag recommended by Politzer for that purpose.

ON THE BEST MODE OF TESTING THE HEARING OF SCHOOL CHILDREN, AND OF PROVIDING FOR THE INSTRUCTION OF PARTIALLY DEAF CHILDREN.

BY

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One of the strongest arguments in evidence of the importance of the subject expressed in the title of this paper, is the fact that, except in individual cases, or where the loss of hearing is so great as to result in deaf-mutism, no provision is made in our present school system for the special instruction of those children whose infirmity, in this respect, gives them, if anything, a larger claim upon the efforts of those to whom is entrusted the laying of the foundation of that mental development which is the basis of the prosperity of a people. To the attainment of that proper degree of mental development which raises the man above the animal, the sound mind in the sound body is necessary; and when one of the channels for communication from without, is either wholly or partially closed, there devolves upon those who have the furtherance of this development in charge, the duty of compensating in one way or another for the loss. How this may be best accomplished in the case in question, is for us to consider.

In a work on the physical and mental education of deaf children, published with the sanction and under the advice of Prof. Von Tröltsch, the author says:[1] The generally received opinion that diminution of hearing in children, in consequence of disease of the ear, is rare, is a mistaken one; indeed, in some localities, the cases of disease of the ear exceed in number the cases of disease of the eye, and moreover they are apparently on the increase; the steadily increasing demands upon the mental capacity, accompanying the advances of the times, bring the defects of hearing more and more prominently forward, because they show themselves to be a decided hindrance to the better education of the child. The results, on its later mental development, of the loss or diminution of hearing in a child, are decided and permanent—affecting the understanding, the character, the self-confidence, and, at a later period, the ability of self-support: mental tools the possession of which is invaluable, and the want of which can never adequately be supplied. In this view, it is a matter of concern that more attention has not been directed to the educational development of partially deaf children.

An examination of a number of cases of partial deafness, occurring in children under sixteen years of age, shows that, with the exception of a majority of those cases in which the disease of the ear has been of a character, or of a degree of intensity, sufficient to so far impair the hearing as to be followed by deaf-mutism, the diseases of the ear in

[1] Franciska Schaeling. Beiträge zu einer richtigen, leiblichen, und geistigen Erziehung gehörkranker Kinder. Thorn, 1872.

children are amenable to treatment, with more or less improvement in hearing, and that the impairment of hearing varies considerably in different cases, and is moreover subject, in any one case, to variation during the progress of the disease. In considering the subject of partial deafness, therefore, we have to deal at the outset with a factor in our problem representing an unknown quantity; each individual case of partial deafness must be, as it were, a law to itself, as far as the compensatory advantages to be accorded to it are concerned. To facilitate investigation, however, two general divisions of the subject may be allowed. The first will include those cases of partial deafness in which the loss of hearing is so great as to interfere with the acquirement of articulation, though not sufficient to permit the classification of the case under the head of deaf-mutism. This division will include those cases in which the diminution of hearing is so decided as to interfere with the independent acquirement of information through the medium of the hearing, and will imply the necessity for special instruction. The second division will include a much wider range of cases, from those in which the hearing is so slightly impaired as to permit of ordinary intercourse, downward through varying degrees of impairment of hearing until the degree belonging to the first division is reached.

Those whose cases come under the first division, form fortunately but a small percentage of all the subjects of partial deafness among children; and a consideration of their needs may be deferred, as secondary to the subject immediately under consideration, and because moreover no provision has been very generally made for their instruction. For the instruction of those whose cases come under the second division, there is, as far as I am aware, no provision made in our public schools, beyond the individual and independent efforts of the teachers. The children who are subjects of partial deafness are, however, sufficiently numerous to warrant some special attention to the subject, with the view of determining the best means for giving them equal educational advantages with their more fortunate companions. To do this will require an examination into the nature of the cause of their disability, and the establishment of rules according to which the degree of impairment of hearing may be determined, that the character of the instruction may be varied accordingly.

An examination into the etiology of partial deafness, shows that the diseases of the middle ear are its most frequent cause in children, and furthermore that a great many cases of partial deafness are due either to the subacute or chronic catarrhal inflammation of the middle ear, or to the purulent inflammation accompanied by perforation of the membrana tympani, which follows the simple acute inflammation, or occurs as one of the sequelæ of the exanthematous diseases of childhood. This general rule is subject to certain differences, in differing classes of society; among the wealthier classes, for instance, who perhaps merit our attention less in this connection, because their children are educated more frequently in private than in public schools, the diseases of the ear accompanied by partial deafness and more or less amenable to treatment, in children under sixteen years of age, amount to but 7.8 per cent. of the whole number of cases of ear disease in children and adults taken together. Of this number, making the 7.8 per cent. of the whole, 76 per cent. are traceable to the exanthematous diseases of childhood, especially scarlet fever, and in 56 per cent. there is found an existing purulent inflammation of the middle ear, requiring treatment, and causing a suf-

ficient degree of deafness to interfere with the patient's participation in the ordinary school exercises, on equal terms with other children.

Among the middle and poorer classes, the proportion of diseases of the ear in children is largely increased, and it is these classes which we have particularly to consider, since they more generally avail themselves of the advantages offered by public instruction. Out of 8715 cases of disease of the ear, examined in patients of this category,[1] 2175, or 24.95 per cent., occurred in children under fourteen years of age. Of this number, 2175 cases of disease of the ear accompanied by partial deafness, 1085, or 49.8 per cent., are recorded as examples of purulent inflammation of the middle ear; 113, or 10.5 per cent. of the cases of purulent inflammation, being due to scarlet fever. As many as 514 cases, or 23.6 per cent. of the cases of disease of the ear in children, are recorded as examples of either acute or chronic catarrhal inflammation of the middle ear. This brief analysis is sufficient to show the much greater prevalence of such diseases of the ear as are liable to induce partial deafness, in the children belonging to those classes in society which depend for their instruction on the advantages offered by our public schools. A more minute analysis of the material offered, which would hardly be in place here, affords interesting information as to the general causes and the average course and results of diseases of the ear in children. It may be remarked, however, that, in the analysis given, only those cases are included in which the deafness was not so decided as to interfere with the acquirement of articulation, or even with the use of the ear as a medium for instruction. The number of cases of positive deaf-mutism, and of cases in which, while there was a certain degree of hearing, it was so slight as to render instruction in articulation necessary, was 107, or, of all the cases in children examined, 4.9 per cent. The average age of all the children examined was about nine years.

We see, therefore, that in a large majority of the cases of disease of the ear occurring in children at an age when they should be profiting by school instruction, the disease is accompanied by partial deafness, and is moreover of a character to lead us to expect considerable variation in the hearing during the progress of the disease, or during the ordinary term of school life. This point is one which should be especially considered in coming to a conclusion as to the character of the instruction to be provided in these cases; we have to deal not only with a different factor, in the different degrees in which the hearing is diminished by disease in different cases, but also with a possibly variable factor in any individual case.

Admitting the necessity for making some provision for the wants of children of this class in our public schools, and excluding, for the present, those cases in which the deafness is of so high a grade that special instruction is absolutely necessary, we have to consider the advisability (1) of establishing separate classes in our schools for the instruction of partially deaf children; or (2) of instituting such a system of inspection as shall lead to a determination of the wants of individual cases, and such a consideration thereof, on the part of the teacher, in addition to the ordinary course of instruction, as will permit of education in mixed classes, compensatory advantages being accorded in cases of partial deafness.

[1] Aural Clinic, Massachusetts Charitable Eye and Ear Infirmary; Statistical Report, Service of C. J. Blake.

The objections to the former course would seem to be—

(1) The encumbering of our school system with an additional department for which especial provision would have to be made. Against this objection it may be urged that initiatory steps have already been taken toward the establishment of separate public schools for the education of deaf-mutes, and the so-called semi-mutes, and that children suffering from partial deafness could be transferred to such schools, thus making use of a department already in process of establishment, and avoiding the creation of another additional department for instruction of only partially deaf children. While the instruction of semi-mutes in the same school with deaf-mutes proper, has certain advantages which have been successfully illustrated in the public schools established for that purpose in Boston, and of which I shall speak hereafter, there are certain aguments against this disposal of the question, and in favor of a different provision for the needs of those partially deaf children who do not at present, and, from what we know of the nature of the diseases which caused their deafness, are but little likely to, in the future, deserve classification among semi-mutes. These arguments are included in the further objections to the establishment of separate classes for instruction, which are—

(2) The moral effect upon partially deaf children of making them, and recognizing them as, a class by themselves, and the exaggerated sense of their infirmity resulting from their being so set apart. The object of the public-school education is to fit the children for the best use of their mental powers in adult life, and to enable them to associate in the world under circumstances which shall make their acquired knowledge best available for their own and the general good; and this object would be in a measure defeated by the encouragement, in partially deaf children, of those feelings with regard to their infirmity which are one of the chief misfortunes of the deaf-mute. To set them apart in the manner referred to, would be to permit an underestimate of their own powers, which, though every effort to counteract it should be made, would necessarily carry its impress into adult life.

(3) The loss of the beneficial association with hearing children, and of a proper spirit of emulation; for since these children are to go out into the world, where they will be judged by the general standard of effort and success, it is best that they should associate in the beginning with those who are to be their companions or their opponents in the struggle for existence, and that they should be urged rather themselves to compensate for their defect than to expect compensation at the hands of others. More precisely, the association with hearing children helps toward the overcoming of certain faults which are common in children who are partially deaf. Partially deaf children are very apt to become heedless and inattentive; from not hearing occasionally, they get into the habit of not expecting to hear, and make less mental effort to combine such sounds of the voice, for instance, as they do hear, or to supply the deficiency of those which escape them. The ultimate effect of this habit, if uncorrected, on the mental development, is very decided; for, in these cases of partial deafness, nature does not seem to have made that effort at compensation which is so strikingly illustrated in the quickened perceptive powers of the deaf-mute. Again, many partially deaf children are almost morbidly sensitive on the subject of their infirmity, and of their implied inferiority: a feeling which, if permitted or encouraged, has a very unfortunate effect upon the child's efforts in its studies,

and subsequently upon its efforts at the self-instruction and self-control of adult life. The association with hearing children, and that degree of participation in class exercises which is possible under certain favoring conditions, tend to overcome the self-consciousness which is always more or less of a barrier in the way of advancement.

(4) The final argument against consigning partially deaf children to a special class or school, lies in the fact, already mentioned, that the imperfection of hearing is not a fixed factor in the problem. In the majority of the cases included in the analysis given, the hearing was susceptible of improvement, and in all of them was subject to temporary variations during the progress of the disease or course of treatment.

The best fulfilment of the second course under consideration, education in mixed classes, necessarily entails a determination of the degree of physical disability of the pupil, for which compensation is to be made. This may be accomplished in two ways, of which the best is by means of a competent professional examination of each case of supposed or evident deafness occurring in the pupils of, or those presented for admission to, a public school. This course offers the advantage of determining with accuracy, not only the degree of hearing, but also the character of the disease which causes the deafness, the amenability of the case to treatment, the probable course and duration of the disease, and its probable, ultimate effect upon the hearing; in very many cases, as may be judged from the statistics given, the need of treatment would be detected where otherwise it might have passed unnoticed. The establishment of the office of medical supervisor of public schools, the position to be filled by a qualified medical man who should devote his time to the duties of his office, would cover this in addition to many more important needs in connection with the hygiene of our public schools.

In default of this more thorough method of examination, the object, as far as a determination of the degree of hearing is concerned, may be accomplished by the teacher, to whom may be committed the task of classification according to certain simple rules for testing the hearing, applicable to all cases. The ordinary tests for the hearing, by means of the watch and tuning-fork, or the use of other musical tones, are each of precise value only when taken in connection with a competent examination into the condition of the diseased organ, and a careful consideration of points of which only a professional observer is competent to judge. Careful tests of the hearing by means of the voice, however, in connection with one or other of the tests mentioned, would enable a competent teacher to determine whether a subject of partial deafness could be left to participate in the ordinary school-room exercises, in common with other children, or whether it would be necessary to give the child special attention.

Following, for instance, the suggestions to be drawn from the observations of Oscar Wolf on the musical value of the vowel and consonant sounds, and the effect which diseased conditions of the ear have upon the perception of consonant and vowel sounds in combination, it is possible to arrange a series of test-words which may be used by teachers as a standard test in schools.

Each teacher may be provided with a form for tabulation of cases, which is to be preserved as a record of the degree of hearing power, accompanied by instructions as to the method of conducting the examination. These instructions should include a clear definition of the precautions to be taken in order to arrive at a correct estimate of the hearing,

the personal opinion of the teacher being required only in remarks upon any other symptoms of disease of the ear, such as otorrhœa for instance, which would seem to indicate the advisability of immediate treatment. The watch and the tuning-fork, or the tones of a musical instrument, may be employed as subsidiary tests, but the main reliance should be placed on tests with the voice, for not only does the human voice, considered as a musical instrument, fulfil certain conditions as a test not otherwise attainable, but it is the voice of the teacher especially which the child is required to hear, and for the accurate hearing of which it is necessary that the child should be placed in such a position in the school-room as shall serve to compensate for its diminished hearing. As a rule, therefore, the tests indicated should be repeated on the transference of a pupil from one room, or one teacher, to another, for every human voice has its peculiarities of pitch and quality, which give it, estimated as a test, a hearing value of its own. These tests also may be repeated at intervals, without proving a great addition to the labors of the teacher, for the purpose of determining the average degree of hearing.

The hearing distance for the voice having been determined, the point in the school-room at which all the test-words are distinctly heard, should determine the position of the seat to be assigned to the pupil, such proximity to the teacher as shall enable the particularly deaf pupil to hear the teacher's voice as readily as a pupil with perfect hearing at the extreme limit of the room, being one of the compensatory advantages which his diminished hearing demands. Furthermore, the attention of the teacher should be directed to the necessity for giving such cases special attention, in such manner as to supply the loss of parts in class exercises which the pupil may have but imperfectly appreciated. A lack of knowledge on the part of teachers of the great obstacle which partial loss of hearing may present to a proper appreciation of school exercises, and the establishment of a fixed standard of instruction to which all the pupils in a class are required to conform, results in added labor on the part of the partially deaf pupil, without the consideration which his imperfect ability in this respect should command.

Passing from the consideration of those cases of partial deafness which may be provided for in the manner above proposed, we have to consider the needs of that more unfortunate class whose hearing is so far impaired as to render special instruction absolutely necessary. The objections which were made to the separate instruction of the majority of partially deaf children, do not apply here. One of the earliest and most unfortunate consequences of extreme deafness, in children, is the impairment of speech; with the loss of hearing is lost the medium through which the knowledge of speech is acquired, and the child gradually lapses into the use of gestures (to proclaim its wants) and the production of inarticulate sounds. To retain or to educate the power of speech, is, however, but one of the objects to be attained by special instruction; some means must be supplied which will compensate as far as possible for the loss of hearing, and prove another medium through which the child may receive the same education as that afforded hearing children in the common school. The sense of sight may, in these cases, be called in aid, and the child may be taught to hear, as it were, with the eyes.

Various systems for instruction in this so-called, visible speech, are in operation, and their results, as far as I have been able to observe them, are not merely encouraging, but eminently successful. Furthermore, as every possible means of imparting instruction should be made available,

in addition to education in lip-reading and articulation, such hearing as the child may possess should be utilized by the employment of artificial means, if necessary, for the readier conveyance of sound to the ear. As a result of practical experience, and not merely upon theoretical grounds, the use of the hearing-trumpet, of one kind or another according to the requirements of the case, may be advised during school exercises; indeed, in many cases, the use of such an instrument tends to improve the hearing not merely during the time of its use, but generally, the subjection of the ear to sound waves of larger amplitude either increasing the vibratory power of the sound-conducting apparatus of the middle ear, or acting as a stimulus to the auditory nerve. The proper application of these auxiliaries to instruction, necessitates therefore the formation of a separate class, for the benefit of which, as has been said, the public deaf-mute school, which now forms an important part of the Boston school system, and which must in time be engrafted upon the public school system of other large centres, may be made available.

In the Boston school, the children are divided into two general classes, those who are total deaf-mutes, and those who are semi-mutes. Children in the latter class are instructed in articulation, for the purpose of overcoming faults which always exist where the hearing is imperfect, and in lip-reading, that they may learn to understand conversation from sight as a substitute for hearing. Such hearing as remains to them is also made useful in conveying ideas as to the formation of vocal compounds, by means of an ear-trumpet, if necessary, or by speaking directly into the ear. As the total deaf-mutes improve in articulation and lip-reading, they are advanced to the class consisting of semi-mutes, partly because the grade of instruction has become the same, and partly that they may profit by the example of those more fortunate companions who have had the advantage of having a little speech or a little hearing to start with. On this plan, the Boston school has now been in operation for five years, with increasing success and increasing evidence of its great value; it now provides for about sixty pupils, all of whom are instructed in lip-reading and articulation, the method employed being the system of visible speech introduced in this country by A. Graham Bell.

The conclusions drawn from a study of the subject of this paper, may be summed up as follows:—

I. The frequency of partial deafness in children, during the period of school life, renders it advisable to make some definite provision in our public school system for compensatory instruction.

II. Since partial deafness is a comparative term, some provision should be made for a proper determination of the degree of disability.

III. This may be best accomplished either by establishing a series of speech-tests, to be used by the teachers, or by instituting competent medical examination at the hands of a medical supervisor of schools; and the creation of such an office in connection with our public school system is strongly urged.

IV. Partially deaf children, whose hearing is not so defective as to require special instruction in articulation and lip-reading, are better taught in mixed classes with those who hear well, compensatory advantages being allowed them according to their degree of disability.

V. Partially deaf children, whose hearing is so defective as to interfere with the natural acquirement of articulation, and to render the ear of little or no value as a medium for hearing, should be accorded the ad-

vantages of special instruction, of which education in articulation and lip-reading should form a part.

Discussion on Dr. Blake's Paper.

After the reading of the preceding paper, Dr. Charles H. Burnett, of Philadelphia, said:—I would ask Dr. Blake if he has noticed, in the schools visited by him, whether adult mutes have been taught to talk?

Dr. Blake said:—I have noticed, especially in the Northampton schools, that pupils who have reached the age of sixteen years, for instance, are taught to talk with very great difficulty. A deaf-mute taught in the sign language, learns to think in a language different from ours. One sign may convey two or three meanings, and its signification often depends upon the context. The deaf-mute's arrangement of words in sentences resembles more closely that of the French, than that of the English, language. The success of deaf-mutes in the acquirement of articulation is sometimes very astonishing. The sound of the voice is at first harsh, and articulation is imperfect. But modulation of the voice is attained, in default of hearing, by the pupils learning to observe the sensations produced by speaking in the throat, by which means they soon learn to appreciate the difference between the rising and the falling inflection. There have been instances in which English deaf-mutes have been taught with such success that they have learned to speak French and German fluently, and to sing harmoniously.

ON AURAL VERTIGO WITH VARIABLE HEARING.

BY

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VERTIGO from disease of the semicircular canals has been fully recognized ever since the publication of Ménière's papers on the subject. Guided by the description of his case, and, more recently, by the investigations of Flourens and others respecting the physiology of the semicircular canals, the tendency among medical men has been to ascribe all cases of acute aural vertigo, not directly traceable to some irritation in the external auditory canal, to disease in the labyrinth, preference being given to the semicircular canals as the seat of the direct lesion.

Without entering into a discussion respecting the precise location of the lesion in labyrinthine vertigo, it can be comprehended why the deafness which is so marked a symptom of this disease, may be permanent. In fact, if the direct lesion be in the labyrinth, as it undoubtedly is, in true Ménière's disease, one cannot understand how the deafness can fail to be permanent. In the affection known as Ménière's disease, called also labyrinthine vertigo (Hinton), the chief symptoms are sudden tinnitus aurium, vertigo, reeling, falling without loss of consciousness, nausea, vomiting, and sudden, total, and permanent deafness in the affected ear. The other symptoms may occur in paroxysms for longer or shorter periods. But there are on record a few cases, and doubtless many others have occurred which have been recognized though not recorded in literature, in which all the above-named symptoms have existed, excepting the permanence of the deafness. Mr. Hinton records such cases,[1] with the statement that the recovery of the hearing was perfect, and then raises the question "were they not caused by muscular spasm?"

Especially noteworthy is that form of acute aural vertigo in which the hearing diminishes during the paroxysm, improves during the interval, and finally is restored when the paroxysms of tinnitus, vertigo, etc., cease to recur. In such cases, it is manifest that the direct lesion cannot be in the labyrinth, and the question might be asked, "Are not such cases due to a spasmodic affection of the muscles of the tympanic cavity?" Future investigations may show that such cases are caused by undue inward pressure of the stapes, which is brought about either by a tonic contraction of the tensor tympani, so powerful as to overcome the equilibrium normally existing between the latter and the stapedius, or by a relaxation of the latter muscle, which permits the normal tensor tympani to act without the antagonistic counterbalance of the stapedius. As tending to answer in the affirmative the question which I have proposed, I would cite the following case:—

Mr. X., aged 41, single, a stock broker, was brought to my door in a carriage on May 8, 1875, apparently in collapse; upon approaching him, however, I found him conscious, but very pale and weak, and his surface cold and

[1] Questions of Aural Surgery, pp. 261–262.

clammy. I was asked to accompany him immediately to his home, and while doing so learned from him that he had been suddenly attacked about an hour before, while attending a meeting of the Board of Brokers, with sudden and intense tinnitus aurium and vertigo, with entire inability to stand, and that he had finally vomited, but that, during all the time of this most disagreeable attack, his mind had been perfectly clear. I found his pulse about 75, but very weak, and he stated that there was still some vertigo, but that the buzzing in the ear had given place to a "stunned feeling" in the head, attended with a boring sensation which seemed to start behind the auricle, and to extend inward to the centre of the head. There was no complaint of altered hearing at this time, but I found that the hearing of the watch was reduced to $\frac{6}{60}$ in. in the affected ear.

The patient was put to bed, and, as his surface was very cold, a little warm brandy and water were given, and warmth was applied to his feet. In about an hour, the vertigo became much less, the head was more comfortable, and the face lost its intense pallor; pulse 80. The patient then stated that, four or five weeks before, he had noticed occasional attacks of slight tinnitus in the left ear; this was brought on or increased by cold air blowing on the ear. A week or two afterwards, he observed some dizziness with the noises in the ear, and also some confusion in hearing, especially during the playing of the organ and the singing in church. The patient had had a good musical education, and he stated that he heard all notes sharpened (heightened) in pitch, in the left ear, which, of course, produced subjective discord with what he heard in the other ear. This was also true for the tuning-fork, *small "A,"* which seemed to him higher in pitch in the left or affected ear. As the tinnitus passed off, however, notes appeared once more to have their true pitch.

With the cessation of the tinnitus, and with the return of the ability to hear notes in their true pitch, in the affected ear, the hearing also improved for the watch, rising from $\frac{6}{60}$ in., during the attacks, to $\frac{20}{60}$ in. as the paroxysms ceased. This variation in the ability to hear the watch occurred not only once, but repeatedly; and it was also observed that a mantel-clock, easily heard by the patient across the room in the affected ear, when unattacked by the paroxysms, was not heard during the latter. The left membrana tympani was more retracted than the right. The Eustachian tubes were easily inflatable; the fauces normal. There was a history of some stuffiness in the now affected ear, once after sleeping all night upon the ground, when in the army during the war. But beyond this, the patient knew of nothing out of the way in the ears.

The patient remained in bed four days: On the *first* day, it was found that rest in a reclining posture relieved the tinnitus and vertigo, but on the *second* day, a severe attack came on in bed, and lasted several hours. On the *third* day, an attempt to rise brought back all the symptoms, which were finally relieved by vomiting. On the *fourth* day, another severe and long attack occurred. On the *fifth* day, the patient observed that the "stunned feeling" in the left side of the head, alternated with the tinnitus; the latter invariably preceded the attacks of vertigo, beginning as a low and distant singing or ringing, and increasing to a loud roaring, which culminated in the vertigo and nausea. On the *sixth* day, there was no attack. On the *seventh*, he felt very much better until 11 A. M., when a severe paroxysm occurred; as a rule the attacks occurred in the afternoon or evening. On the *eighth* day, there was no attack, but on the *ninth* there was a light one. Again, on the *tenth* day, there was no attack, but at midnight of the *eleventh*, there was a very severe paroxysm of tinnitus and vertigo, which woke the patient up. Closing his eyes had always aggravated the vertigo, and he now found that the darkness of his room greatly increased the dizziness. Being entirely alone, and unable to help himself, he was obliged to call for a light in order to gain some relief from the terrible discomfort brought about by the vertigo. He felt as if his whole body were being borne through space. Usually the apparent motion of surrounding objects, during the attacks, was around the patient from the right, over his

head, to the left, under him and up again—that is, in a circle, the plane of which correspended pretty closely with that of the superior semicircular canal, and about at right angles with the antero-posterior diameter of the head.

The severe attack of vertigo on the *eleventh* day, extended into the *twelfth* day, but there was no vomiting. On the *thirteenth* day, there were two attacks, but no vomiting. On the *fourteenth* day, there were again two attacks, but they were light, and there was no vomiting. On the *fifteenth* day, there was no attack, but on the *sixteenth* day there was one very light and short paroxysm of tinnitus aurium and vertigo. On the *seventeenth* day, there was no attack, but on the *eighteenth* there was a very light one, which was the last the patient had. The hearing now became normal.

Before these seizures came on, the patient had been under intense mental excitement, and his general health had failed from the time of the financial panic of 1873 to the date of his first attack of vertigo. He had also been, in this weakened and nervous condition, obliged by his business to endure the intense and peculiar noise of the stock-brokers' board, and also to strain his ears to hear, and his vocal organs to perform his share of, the bidding which goes on in such places.

The treatment consisted in general support, with the administration of good food and some alcohol, together with large doses of bromide of potassium during the persistence of the paroxysms. As the latter diminished in severity, iron and quinine were given. On the sixth day of the disease, twenty grains of bromide of potassium were given every hour, and this was continued until the tenth day, when the patient took but ten grains every two hours. On the sixteenth day, the bromide was taken every three hours, and kept up in this way until the paroxysms had evidently ceased.

The patient went to Europe, made a short tour, and returned to business in the autumn, about six months after his first attack of vertigo. There has been no severe return; this spring, when he was under considerable excitement once more, he had a slight return of the tinnitus, and a slight tendency to vertigo, but no sickness of stomach. A few days of rest, and from six to eight grains of quinine daily, dissipated all these unpleasant warnings, and the patient has been able to continue his attention to business.

This case of aural vertigo is especially interesting on account of the variable hearing which was so prominent a symptom during the disease, and also on account of the recovery of hearing which ensued when the paroxysms of tinnitus, vertigo, nausea, etc., ceased to recur. In fact, these features of the disease would tend to place it either in the list of those of infrequent occurrence, or among those the true nature of which is not often recognized, and hence undescribed. It is well known that the prominent symptoms of aural vertigo, viz., sudden tinnitus, intense dizziness, alteration in the hearing, nausea, vomiting, and falling, without loss of consciousness, may be caused by the irritation set up by the presence of a foreign body in the external auditory canal. But here the cause is easily recognized and removed, and upon its removal the recovery is complete. The same general symptoms are notoriously characteristic of labyrinthine vertigo (Ménière's disease), but there the deafness is sudden, total, and permanent, though the paroxysms of vertigo may cease to recur.

I have lately seen two cases of tumor in the brain (the one proven *post mortem*, the other diagnosed as such, but the patient still living), in which the new growths produced symptoms very much like those usually attending labyrinthine vertigo. Still, there were points of differential diagnosis in these cases.

The subject of the first, a woman, suffered for many years with most of the distressing symptoms of Ménière's disease, with the exception that she became

slowly, entirely deaf, whereas the deafness in true, acute, labyrinthine vertigo is sudden, intense, and permanent. In this case a post-mortem examination revealed the presence of two tumors, one on each auditory nerve; on the right side, the morbid growth, as large as a chestnut, seemed to have obliterated the nerve, and extended into the porus acusticus internus, and apparently into the labyrinth. The microscopic examination of this growth has not yet been made. On the other side, the tumor, a smaller one, seemed to have simply grown in the auditory nerve, between the brain and the internal auditory canal.

In the second case, that of a man, the presence of a tumor of the brain has been diagnosed; the patient has some of the symptoms of aural vertigo, but there is permanent alteration of the gait, which is not characteristic of Ménière's disease, nor of any form of aural vertigo originating in the tympanum or external ear. Furthermore, the patient is not very hard of hearing (both ears are affected, as they were in the first case), and, though he has constant tinnitus and paroxysms of vertigo, he can always relieve the giddiness by sitting down. While these cases may be classed under the head of aural vertigo, they are manifestly not cases of primary labyrinthine disease.

The case of Mr. X., given above, presented all the marked symptoms which are found in aural vertigo of the labyrinthine variety, excepting the sudden, total, and permanent deafness; but this difference would exclude it entirely from aural vertigo due to primary labyrinthine disease. The case certainly did not originate from irritation in the external auditory canal, and that it could have had its origin in cerebral tumor, is entirely out of the question, not only because such a supposition would be unwarranted by the symptoms, but also because recovery finally ensued. May it not be, however, classified as a case of aural vertigo of the tympanic variety, in which the primary lesion lay most probably in the muscular structures in the middle ear? As tending to cause such a disturbance, I would mention the intense strain on the vocal organs as well as on the muscular accommodation of the tympanum, which necessarily occurs among those who frequent the brokers' board, where bidding aloud and listening to bids go on.

In conclusion, I would suggest that, in future investigations of this most interesting and important disease, it would be well to make "aural vertigo" a general term; then four varieties of that disease could very easily be described, according to the anatomical situation of the cause, for there might be (1) Aural vertigo from external irritation (in the external ear); (2) Aural vertigo from tympanic disease (Mr. X.'s case is an example); (3) Aural vertigo from labyrinthine disease (Ménière's disease); and (4) Aural vertigo from central irritation (cerebral tumors implicating the auditory nerve). Such careful distinctions in diagnosis are extremely important, as aiding in prognosis—this being far more favorable in the first two than in the other varieties.

ON THE SEVERAL METHODS OF EDUCATING, AND ON THE SELECTION OF PROPER SCHOOLS FOR, THE DEAF AND DUMB.

BY

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The lot of the uneducated and ignorant deaf-mute is sad indeed; cut off from intercourse with his fellow-man by loss of speech, and unable to obtain instruction or amusement from such intercourse, or through books, he is left to "Nature," and is governed by naught but his animal passions and appetites. It is no wonder then that, when these latter are in full development, he becomes allied to certain of the brute creation. From my own dissections of the brain of the deaf-mute, as from those of many careful observers, it is rendered probable that the deaf and dumb are capable of receiving and retaining as great an amount of intellectual knowledge as their hearing brothers or sisters, provided that adequate instruction be made available through the eye.

As early as the fifteenth century, some effort was made to teach deaf-mutes, but it is to the sixteenth century that is due the great honor of having been the period of jubilee to the deaf and dumb; and to the Abbé de L'Epée was given the great gift of teaching deaf-mutes by a symbolic language, so that they might know good from evil, have intercourse with a select few of their fellow-men, and above all acquire such aspirations after true knowledge as would fit them for a happy home here, and for a better world above. Another devoted man, the Abbé Sicard, who took up the clue already furnished by de L'Epée, adopted sign-language, and enlarged, improved, and reduced it to a system. The knowledge of this valuable method soon crossed the Atlantic, and its advocate, the Rev. Thomas H. Gallaudet, D.D., founded the American Asylum at Hartford, Conn. (1817), and, soon after (1820), M. Clerc (one of the favorite pupils of Sicard), who came over with Dr. Gallaudet, organized the Pennsylvania Institution for the Instruction of the Deaf and Dumb, in Philadelphia. This institution has sent out, since its organization, 1700 pupils, well educated, and with a careful moral training which has been of inestimable advantage to them and to the community at large. Germany was not long after France in this philanthropic labor, for, in 1760, Samuel Heinicke, a Saxon by birth, developed the "artificial method" of instruction for deaf-mutes. The principal aim of this good man was to improve the "French system," and to endeavor to cultivate whatever might remain of speech, by developing the latent power which exists in all save a very few. In the early stage of this system, artificial signs are absolutely necessary; but when these have been acquired, they are to be employed only as a ladder to reach the higher department, where the finger-alphabet and other artificial signs are no longer required. When successfully taught by this system, pupils are enabled to think in the

idioms of the language of their country, to hold audible conversation, and so understand much that is spoken to them, being thus rendered practically less deaf, and actually less dumb.

Another advance which has been made in the further improvement of the mode of instruction of the deaf-mute, comes from England, in the system known as "Bell's Method of Visible Speech," which, having received but little attention in the country of its origin, was brought by its author to the United States, where it has been received and adopted in seven of our forty-eight institutions, and with the most gratifying results. I do not find fault with what has been done under the old methods, but rather rejoice that so many thousands of deaf-mutes have received the advantages of an education by means of the sign language, and of articulation taught by the German method. I would also state that the United States have a "National College" at Washington, where more advanced studies can be pursued, and where young deaf-mutes are graduated with a standing and scholarship not inferior to that achieved by the graduates of ordinary colleges. This institution bears to others for the deaf and dumb, the same relation that colleges bear to schools and academies. Many of the graduates of this college have received appointments as teachers, while others are editors, authors, and writers, or are found in the various government offices, in the exercise of duties which they are quite capable of performing in an entirely satisfactory manner. In our Centennial Exhibition will be found some admirable pictures executed by deaf-mutes, as well as other products of their pencils and pens. They are also capable handicraftsmen, and are to be found in our shops and factories, as well as in the Industrial Homes founded in this city for their special benefit.

Bell's method or system of visible speech gives the pupil, by means of drawings, etc., a knowledge of the concealed parts of the mouth and throat, which are used in articulation, as also of the movements of the various parts, so that the pupil is thus better able to gain conscious control over them. This method of writing any sounds that the pupils may utter, serves to interest them in the practice of the elements and combinations, thus giving them great power over their organs of speech, and obviating the necessity of informing them that a sound is wrong if it is not the one which the teacher wishes to obtain. It is the practice of those who teach this system to write all sounds in the visible speech-symbols, and especially those that are essential in English speech. The symbolizing of odd sounds also leads the pupil to think and study about the parts of the mouth that produce them.

I shall now discuss the following questions: (1) What is the best method of classifying deaf children, and is it advisable to place them in ordinary, or in special, schools? and (2) How many deaf-mutes are capable of receiving Bell's method of instruction, and should the attempt be made to instruct all deaf-mutes by articulation, or by the sign language only? Not having heard Dr. Blake's paper, I shall attempt to answer these questions from my own personal observations.

If a child can hear sufficiently well to understand the teacher when near him, the ordinary school is, for him, decidedly better than a special school. Children sometimes become deaf after having learned to talk and read; such children may profitably attend an ordinary school, provided that the parents or teacher take time to explain the lessons; but if this be not done, the child will often recite in a parrot-like manner, without understanding what he has learned, and will go over a great deal

of ground with very little profit. This has been the experience of teachers of deaf-mutes, even when the pupil has learned to read quite well by observing the lips of the speaker. Congenital deaf-mutes, attending an ordinary school, may learn to write, or rather to copy, and may perhaps get some idea of numbers; but the teachers of such schools do not know how to reach their pupils' minds, even if they have the time to teach them. As a rule, such children might as well be at play, except that school occupies their time and their thoughts. Another advantage, however, which is gained for the deaf-mute children, is in their mingling as much as possible with those who hear.

If a child cannot profit by the instruction given in an ordinary school, let him if possible have a private teacher, but not necessarily in his own house, as he is not always subject to the best government there. If he needs stimulating, it may be well to place him in a class with four or five others of a suitable degree of advancement; and if this cannot be done, he may be placed in a school or institution where the instruction is especially adapted to the deaf.

If children are too deaf to profit by the common school, and yet have sufficient hearing to have acquired speech through the ear, instructors of the deaf are nearly or quite unanimous in the opinion that they should be taught by articulation and lip-reading. The experience of the teachers would lead them to say, "Let the attempt be made, if possible, to teach *every* deaf child in this way." Of 116 pupils in the "Clark Institution for Deaf-Mutes," three have been dismissed as incapable of learning articulation and lip-reading, and one because she required more individual instruction than could be given her. The latter, however, has since been taught, so that speech and lip-reading are her means of communication in her own home. Some of the remaining number of pupils (congenital mutes) speak imperfectly, but in every instance well enough to be understood in their own homes, while some of the indifferent speakers are fairly successful in lip-reading, an acquisition as valuable as that of speaking. Many congenital mutes speak so as to be understood by strangers, and will probably be able to make speech and lip-reading a successful means of communication throughout the world. Of the 116 pupils at the above-named institution, one-third are semi-mutes or semi-deaf, although some of those so classed have not had hearing enough to learn to talk.

My object in writing this brief paper has been: (1) To excite a greater degree of interest, in physicians, for the deaf-mute, with an endeavor on their part to prevent deafness, and so diminish the number of deaf-mutes; (2) To induce a more conscientious study and treatment, by physicians, of the ears of their patients, when the latter are attacked by scarlet or typhoid fever, cerebro-spinal meningitis, or obstruction of the Eustachian tubes as the result of measles, diphtheria, tonsillitis, or syphilis; (3) To lead physicians to give the systems of instruction pursued in our various institutions for the deaf and dumb, a certain amount of study, so as to be able to recommend intelligently to patients, their relatives, or friends, the best method for each individual case; and (4) To induce physicians to recommend that there should be appointed by the governor of each State, a commissioner, to collect, examine, and classify the deaf and dumb, so that all who are found to possess any degree of hearing, or any remnant of speech, may be taught articulation by the method of Bell, and that those who are unable to profit by this system, may be taught the language of signs, natural or acquired.

To aid them in their arduous work, I have recommended to some of our teachers of deaf-mutes, that their pupils should use mechanical appliances for improving the hearing power. By speaking or singing different vowels into one of these aids to hearing (tubes or trumpets), we can determine how much hearing the pupil possesses, and, if he be able to distinguish one vowel from another, a continued use of this mechanical aid may ultimately enable him to utilize audition as an auxiliary to vision. I have known very deaf persons, by the aid of this means alone, to have had their hearing so much improved that they could distinguish all ordinary sounds, and, by some effort, enter into conversation. Another important mechanical aid to persons who are deaf from diseases in which the tympanic membrane is lost, in part or in whole, but in which the inner small bone (the stapes) still remains, or in whose ears the bones have become stiff or anchylosed, is the pellet of cotton, moistened and applied near to the bones, or against the stapes, so as to bring the parts in closer contact with external vibrations, and thus cause the sounds to be transmitted to the auditory nerve.

In conclusion, I have added, chiefly from German authorities, the most recent views on the subject of phonation, knowing how much interest this subject is exciting in the minds of both physicians and teachers of the deaf and dumb, at the present day. Some writers have endeavored to prove that the brain molecules of the deaf and dumb differ from those of persons who hear; but, as I have already stated, the brain is rarely affected in those that are deaf, while, on the other hand, in many instances of extensive disease of the brain, not involving the auditory nerve, deafness has not resulted. Occasionally the spine is diseased, or the nerves which, coming from that great centre, give power to the tongue and larynx, thus rendering the deaf-mute unable to articulate, no matter how much instruction he may receive. Section of the spinal accessory, or of the inferior laryngeal nerve, entirely destroys the voice, so that these may be termed the true vocal nerves. If, therefore, the centre of phonation is situated in the spinal cord, it is plain that it cannot be found in the brain; anencephalous children have been known to scream under the influence of external excitation or internal pain. The centre of the memory for words, appears to reside in the brain, and attempts have been made to fix its seat in the anterior lobes, but the observations made on this subject are as yet contradictory. Each centre is independent of the other, for a cry may be easily uttered when articulation is very difficult. *Amnesia*, or the loss of memory for words, therefore, must be distinguished from *aphasia*, or the loss of power to pronounce them; the patient suffering from aphasia can still write his thoughts, while in amnesia he can only express himself by drawing a representation of the object to which he wishes to refer.

ON BATHING, SWIMMING, AND DIVING, AS CAUSES OF AURAL DISEASE.

BY

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Swimming and diving in cold water seem to me to be important and frequent causes of disease of the external auditory canal and anterior surface of the membrana tympani, as also of the middle ear, though bathing in river or sea water is, when wisely and properly regulated, both healthful and pleasant. The evils attending bathing and swimming in cold water are the entrance of this cold fluid, not only into the external meatus, but as far as the membrana tympani, causing inflammation of the lower portion of the auditory canal, and of the anterior surface of the tympanic membrane. But a still greater evil is from sudden deglutition, during diving or swimming, by which—the mouth, nose, and pharynx being filled with cold water, and the mouths of the Eustachian tubes open—a portion of the water passes into the middle ear. This result rarely occurs in expert swimmers or divers, but is most common in beginners, who suddenly, from cold, or the shock of the contact of the water, breathe or swallow in a sobbing manner. I have, however, known it to occur in old and experienced swimmers while plunging headforemost, owing to the intense coldness of the water—the act of deglutition being entirely involuntary.

If the water is not removed by placing the head to one side, and drawing the external ear forcibly outwards, shaking the head at the same time and opening the mouth, it is apt to cause inflammation, with the formation of pus, followed by perforation of the membrana tympani; or the inflammation, if neglected, may pass inwards to the cochlea and labyrinth, and, implicating the brain, may terminate in death.

It is a well-recognized maxim among those who devote special attention to diseases of the ear, that no cold fluid should be allowed to enter even the external auditory canal; still this important fact is not sufficiently recognized by the profession at large. The entrance of warm water into the ear is less objectionable, but even this is not quite free from danger, and has its disadvantages; and the water should in all cases contain a few grains of a saline ingredient, like borax, soda, or common salt, when employed in washing out the ear. The symptoms of water in the middle ear are, in the first stage, an uncomfortable sensation, followed by earache or pain, which after a time becomes agonizing, and is accompanied with great tenderness behind the auricle. In proof that water in the ears is injurious, and causes deafness, I might cite a number of instances, and it is a well-known fact that dogs which are *thrown* into water become deaf.

Many cases of this form of disease, in its chronic stage, come under treatment during all seasons, but acute cases, from swimming and diving, occur during the summer months, and chiefly in boys from eight to six-

teen years of age; a much smaller number occurring in the fall and winter. If the acute form be promptly treated, entire recovery takes place; but should the case not be seen until after the chill, it is always followed by a discharge of shorter or longer duration. In cases not recognized, the symptoms of violent headache, furious delirium, and coma, give the physician the impression that disease of the brain is present, and the case thus improperly treated terminates in death. The morbid condition in the first stage consists in acute inflammation of the extremely delicate mucous membrane lining the middle ear. This inflammation is followed by effusion of fluid, and, after twenty-four hours, by the formation of pus; it is in every instance attended by fever, with swelling and inflammation of the nasopharyngeal space, and great pain. If this fluid or pus be removed by incision into the membrana tympani, followed by the use of the air douche, and injections of hot saline water, the patient recovers, and the ear is saved. The patient is apt to remain deaf for several weeks, and the local application of tincture of iodine, with or without some anodyne, brushed around the back of the auricle, facilitates the removal of the inflammatory thickening. To diminish the discharge of pus, should it continue, we may employ a powder of salicylic acid and starch, blown into the meatus and after a time washed out, and reapplied twice a day, until the discharge shall have ceased and the perforation have become closed.

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THE PRESENT CONDITION OF THE EVIDENCE CONCERNING "DISEASE-GERMS."

BY

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THE object of this paper is to give a sketch of the prominent theories now held as to the nature of disease-poisons. It seems hardly necessary for me to say that the short space of time at my disposal will prevent me from attempting to furnish either a detailed account, or even a review, of the ample material that may be brought to bear upon these matters. I desire, however, to state this clearly at the outset, because I am sure that in the presence of the vast number of observations that have been made, especially during the past few years, nothing short of a very extended treatise could be made to fairly represent the whole subject. Fortunately, my task is limited by the title of my paper, but I find myself at the same time in the difficult position of one who is called upon to define the proper limits of the subject, or, in other words, to mark out the debatable ground.

In doing this, I have thought it desirable to direct the attention of the Section chiefly to a certain number of important topics bearing upon the subject, as in this way we can get a better idea of its present status, and I have naturally included among the topics those with regard to which I have had the most personal experience.

It is a matter of regret that I shall be obliged to omit many facts of historical interest, as well as the names of well-known men, who, like Pasteur, Sanderson, Salisbury, and others, have enriched the field by original research. I shall also have to run the risk of appearing dogmatic, in assuming that certain questions have virtually been settled, or that others do not concern us. I shall thus take it for granted that the minute, vegetable organisms, called bacteria, exist pretty generally in

nature, both in and out of the body; and, following the acceptance of this idea, that the question of spontaneous generation need not be introduced into the present matter in hand.

Almost any one, who for the first time follows the discussions on these subjects, encounters a little difficulty in the word *germ*, which is, perhaps, not a happy one to have come into use as expressing that from which a disease is derived. This, however, is its true meaning, and neither indicates a vegetable nor an animal character, nor even that it is living, though this latter notion is held by Beale.[1] For even should a disease-poison turn out to be a chemical substance, the term "germ" is equally applicable to it, according to the proper definition of the word. In fact, under this threefold character, we are called upon to study these active principles, for about these three possibilities, of a vegetable, an animal, or a chemical, character, are grouped the three prominent theories that have been framed to explain the origin of infective diseases. These theories, or hypotheses, as they might be more properly called, are (1) the vegetable-germ, (2) the bioplasm, and (3) the physico-chemical, theory. The infective diseases are such, in a general sense, as are either propagated by direct contact, or through the medium of the air, various gases, fluids, or common objects. Inquiries have mostly been directed to such of these as are called septic, viz., erysipelas, pyæmia, septicæmia, and puerperal fever, and to others known as contagious, miasmatic, and the like, such as smallpox and typhoid fever.

I shall try to sketch these theories briefly:—

I. The "vegetable-germ theory" is the one that heretofore has attracted more attention and interest than any other. Though we have known it under this name for only a comparatively short time, it is not of recent origin, and has in reality been recognized from very early times in the history of medicine. It was at first, of course, merely a hypothesis of the flimsiest character, though it soon came to be regarded as a possible one, when the microscope showed that both animal and vegetable organisms, of exceedingly minute size, lived and grew in the interior of the body.

More color was given to this theory by the labors of Schwann, Cagniard de Latour, and Kützing,[2] who found that in fermenting matters there was a growth and multiplication of minute organisms, which they regarded as vegetable; when at a later period Pasteur[3] asserted that these particles were conditions essential to fermentation, then it came to be assumed that the phenomena of disease, which in many respects resembled them, might be due to an analogous cause. It was in this way that the doctrine of vegetable germs as the cause of disease came to be regarded as having good *à priori* evidence in its favor. This belief was further strengthened by Schönlein, who, in 1838, published his account of the fungus plant found in Favus, which is now almost universally believed to be caused by the deposit and development of the plant in the diseased part. As further investigations were made, the silk-worm disease, and the potato-rot, or murrain, were also admitted to be caused by growths of a similar nature.

More recently it has been held that a great many of the infective dis-

[1] Disease-Germs, p. 10.
[2] Schützenberger on Fermentations, pp. 36–7.
[3] Bastian; Lancet, April 10, 1875, p. 502.

eases have each a microphyte,[1] or minute, microscopic plant, as its peculiar active principle. These latter alleged discoveries have, however, been received with marked opposition, many excellent authorities refusing to accept these matters as definitely proved.

II. Other hypotheses had, therefore, to be brought forward, and prominent among them was that of Beale,[2] who, admitting that particles of minute size may produce disease, and in fact laying stress upon this point, believes that the particles in question are degraded portions of the animal body. Such particles he tells us have been seen by him, when using a sufficiently powerful lens, and he assures us that they divide and subdivide under suitable conditions, and "as living matters alone divide." Ross[3] has a modification of the same idea, and Hutchinson[4] states, with reference to certain diseases at least, such as gonorrhœa, purulent ophthalmia, erysipelas, and phagedæna, that they are due to the contagion of living cell-material, such as is called protoplasm, or bioplasm. These tenets, with more or less modification, are sustained by those who believe in the "bioplasm theory."

III. Bastian deserves to rank as the ablest exponent of the remaining theory, the "physico-chemical," or "physical." It is called a modification of Liebig's old theory, and, as now enunciated, holds that though minute organisms may act as ferments, they do so by virtue of chemical actions set up by them, while minute particles of the human body have almost an equal capacity for setting up diseased action under suitable circumstances. In either case, bacteria are apt to be engendered as correlative products.[5]

It will be noticed that two of these theories are based upon the belief that the origin of disease is due to minute particles, and that this belief does not militate against the third theory, and is admitted as possible by its advocates. This simplifies the matter before us very considerably, and enables us to state at once that these poisons are *particulate*, or certainly that they are apt to be bound up with particles.[6] I will merely add here that, in my experience, it may be possible that this has always been the case; some of the particles or molecules, may, of course, be almost immeasurably small.

We may get a good idea of the controversial points by considering first the arguments brought forward by those who support the vegetable-germ theory, and then the answers or explanations of its opponents.

(1) I have already stated that after the publication of Pasteur's brilliant experiments in relation to fermentation and putrefaction, they were regarded as affording good, *à priori* evidence of the truth of the doctrine now under consideration. But it must be remembered that, though sustained by the observations of many others, these views were strenuously opposed by Willis, Stahl, Liebig, and others, chiefly of the German school.[7] There is little doubt now that these latter were in a measure correct; in fact, Pasteur[8] has seen fit to modify some of his earlier statements, for he quite recently has said that both alcoholic fermentation and putrefaction may be initiated by the chemical processes

[1] Such as the diphtheria-micrococcus of Letzerich, the cholera-fungus of Hallier, and the measles-fungus of Salisbury.

[2] Disease Germs, pp. 5 and 11.

[3] British Medical Journal, May 27, 1876, p. 22.

[4] Ibid., April 24, 1875, p. 559.

[5] Lancet, April 10, 1875.

[6] There is excellent evidence of this in all the diseases which are alluded to in this paper.

[7] Schützenberger, p. 40.

[8] Lancet, April 10, 1875, p. 508; and Tribune Médicale, Avril 1, 1875, p. 321.

taking place in the tissue-elements of certain fruits and vegetables, independently of the minute organisms supposed to be necessary to the process. Similar statements had previously been made by MM. Le Chartier and Bellamy, who found that, in modified forms of fermentation, independent organisms were generally absent at first, though they often made their appearance afterwards. Among the late contributors to this subject is Dougall,[1] who tells us that he has produced putrefaction without bacteria, and has seen bacteria without putrefaction—in which latter statement he can doubtless find many to accord with him.

Hiller[2] has also made statements to the same effect, for, having injected fresh eggs with a fluid containing bacteria, but not putrid, the eggs remained unaffected, which showed that bacteria might be present without decomposition ensuing. These views have been further supported by Donné and Beauchamp.[3] They are interesting, as showing that there has been, and still is, a diversity of opinion among scientific men as to the *rôle* which organisms play in fermentation and putrefaction. I may here add, also, that, in regard to the question of the ferment or cause of the ammoniacal decomposition of the urine, or fermentation, as it has been called, Pasteur[4] has stated definitely that it has not yet been demonstrated mathematically that the little torula to which he called attention several years ago, is the *cause* of the change, though he asserts that whenever putrid urine is examined the little plant is found.

(2) Another argument, commonly urged by those who believe in the vegetable-germ theory, is as follows: Almost all authorities admit that minute organisms may be the sole and sufficient cause of diseases such as scabies, favus, and the like, while many epidemic diseases among plants and animals are similarly produced, so that it is reasonable to suppose that vegetable organisms may be concerned in the production of infective diseases, the origin and propagation of which are thus explained with comparative ease. In reply, it is to be said that while no one may be willing to deny these facts, and while the silk-worm disease and potato-rot may be due to the vegetable particles that have been described; while various cereals may be affected with peculiar diseases, each exhibiting a peculiar plant; and while the pollen of many grasses may be the real cause of the hay-catarrh, or fever, as maintained by Blackley,[5] yet, that, on the other hand, it is certain that close observation in certain other directions shows the number of diseases due to vegetable growths to be really smaller than was formerly supposed. One has but to compare the long series of skin-diseases which but a short time ago were put down as caused by microphytes, with the modest list of five possible and three probable organisms, which are mentioned by a recent authority in these matters.[6] I refer to the Achorion of Favus, the Tricophyton of Tinea, and the Microsporon of Pityriasis versicolor.

The recent investigations of Lewis and Cunningham point in the same direction. They make it appear probable that in the Fungus-foot of India, the peculiar growth called Mycetoma is not necessary to the disease, as in some instances it has not been found, though the examinations were doubtless conducted with skill and care.[7] In the light of these and similar experiences, it would appear that while we are now and then dis-

[1] British Medical Journal, April 24, 1875, p. 557.
[2] Centralblatt f. der med. Wiss., Dec. 1874.
[3] Schützenberger, p. 225.
[4] Loc. cit.
[5] Experimental Researches on Hay Fever, 1873.
[6] Piffard, Diseases of the Skin.
[7] Lancet. January 22, 1876.

covering new plants or animals that act as disease-agents, we at the same time sometimes find that the number of such alleged causes of disease has been too large.

(3) It is further said that, in a number of the infective diseases, such as smallpox, typhoid fever, etc., there is a constant ratio between the intensity of the disease, in a given part, and the presence of bacteria; so that, where the disease is most active, bacteria are most numerous. This, indeed, is just what we should expect, in support of the vegetable-germ theory, and it is a fact that few of its opponents are prepared to deny. But it has long ago been shown by Schwann and others,[1] and more recently by Tyndall,[2] that the air at times swarms with bacteria; and it is also true that bacteria occur in the body in health, the mucous membrane of the alimentary canal, from the mouth to the anus, having been found peopled by myriads of them in active, independent motion.[3] These facts have been so generally confirmed, that, as it may be remembered, I assumed them at the outset. Now bacteria are also found in the heart of the tissues, which is explained by the fact that they are taken up by the absorbents, and carried about, while they have, in a certain number of instances, been found in the blood of the lower animals, and sometimes in human blood.[4] Now, as long as this remains true, it is for those who maintain this theory to show that there is a manifest difference between the bacteria which they believe to be disease-agents, and those that we know are generally to be found in the tissues; for the mere fact that bacteria are found in extraordinary quantity at any one point, does not by any means indicate disease. In a large number of experiments, performed by Dr. Curtis and myself, it was found that, under certain circumstances not incompatible with ordinarily good health, vast collections of bacteria were to be found upon the tongue. Under these circumstances, it seems to me perfectly proper to believe that bacteria occur in the diseases just named as correlative products, and not as causes, of the disease. I may here venture to state that, as far as diphtheria is concerned, and upon this matter I have made some special study, I have never found any organisms connected with the disease that could in any way be distinguished from those found at other times in the body.[5] Of these experiments, I shall say more presently.

(4) Now, while it is further alleged that, in another class of diseases, such as erysipelas and pyæmia, septicæmia, puerperal fever, and hospital gangrene, there is always a numerical increase of bacteria at the points involved, we may be allowed to state, in this case also, that, the presence of bacteria in the body being allowed, their tendency to accumulate in diseased portions does not necessarily show that they have any relation with the cause of the disease; and when the question is further asked, how may we distinguish the bacteria of such diseases from each other, or from those of putrid infusions, I think that the same difficulty will be found as in the case of the other infective diseases, before mentioned.

(5) An important question is often asked, viz., Can any strictly chemical substance be a fever-producer, or do we know of any substance, whose chemical nature we can express by symbols, that can produce fever? This is a question usually put to those who advocate the physico-chemical

[1] Beale and Sanderson.
[2] Lancet, May 8, 1875.
[3] Bastian, however, seems inclined to believe that these organisms do not come from the air.
[4] Wagstaffe and others.
[5] See Report on the Pathology of Diphtheria, in Report of the Board of Health of the City of New York, for 1877.

theory of disease. It may be impossible to answer it affirmatively, and yet this by no means settles the matter. The minute chemistry of animal poisons is far from being as yet in a satisfactory condition, but we cannot therefore say that such substances may not at some time be found. Weir Mitchell,[1] of Philadelphia, in his most extensive researches on the poison of the rattlesnake, assures us that the poison is an albuminoid body (which he calls *crotaline*), which is not precipitated by boiling, but is by alcohol. Prince Bonaparte seems to have found, also, that the venom of the viper contains a similar, albuminoid substance, which he has called *echidnine*. Hiller[2] also believes that, in glycerine, he has found a material that has the power of extracting certain chemical poisons in putrid and septic matters. We see, therefore, that a certain definiteness has been reached in our estimation of a few active principles, and it is possible that when this field has been more actively worked by the chemist, we may find those other matters which the microscope has failed to analyze, and which yet may react in a definite way, so as to show individual characteristics. It is also reasonable to suppose that chemical combinations of more or less complexity may be formed in the body, under the influence of peculiar processes, or of the presence of virulent matter, and then, in either case, may communicate like qualities to other ordinary matter of the body; for the venom of serpents is certainly formed out of the normal tissue-elements. In the case of chemical compounds, they may increase to an indefinite amount when placed in suitable liquids, where the proper constituents are present.

(6) It is also maintained, by the vegetable-germ theorists, that the extraordinary capacity which disease-poisons have of retaining their vitality, is a strong argument in favor of their being bacteria, which resist various reagents, and exposure to extreme degrees of heat and cold, and which may remain dormant for long periods of time, without losing their capacity for reproduction. It is known, however, that the poison of the rattlesnake resists boiling, and is not rendered less virulent by alcohol; and Fayrer[3] has asserted that this latter quality belongs to the poison of the cobra; but both boiling and alcohol prevent the multiplication of bacteria, according to the testimony of so large a majority of observers that this point has seemed to me sufficiently settled. Whether or not minute particles of human tissue are capable of living long after separation from the body, is still a question before us, and not one that can just yet be summarily dismissed, as some would imply.[4] But if it should prove true that the little particles of living matter soon lose their capacity for reproduction, it does not follow that they have lost their power of acting in such a way as to communicate qualities to, or set up actions in, other particles. This is advocated by one who believes in the physico-chemical theory.

(7) Again, it is frequently urged by those who believe in the vegetable-germ theory, that the bacteria of certain diseases will induce like diseases in other persons or animals, when inoculated. This may be true in the case of individuals, though there is testimony against, as well as for, this assertion.[5]

I may here state that the results of my experiments and of those of Dr. Curtis, in regard to diphtheria, have failed to show that the inoculation of the bacteria of diphtheritic membranes upon *animals* produce any

[1] Smithsonian Contribution, p. 46.
[2] Centralblatt f. Chir., 14, 15, 1876.
[3] Edinburgh Medical Journal, 1868 *et seq.*
[4] Sanderson.
[5] Compare the statements of Wolff, Küssner, and others.

other lesions than those of bacteria artificially reared or obtained from our own mouths. Hiller[1] proposed to himself the following questions: whether (1) bacteria had the property of exciting inflammation, when inoculated? or (2) if in the blood did they produce fever? or (3) multiply? or (4) have the capacity of penetrating living organisms? He experimented on animals, using the bacteria taken from different kinds of decomposing substances. The temperature rose, but not above 2° Fahr.; on the third day, the bacteria at the point of inoculation were shrivelled, and on the eighth day they had disappeared. Even when introduced into the living blood, they soon ceased to be recognized, though in blood exposed to the air they multiplied rapidly. In this connection, the experiment which Hiller made upon himself, the results of which he exhibited at last year's meeting of the German Surgical Society, deserves more than a passing notice.[2] He allowed himself to be punctured in eight places with bacteria, which he had isolated by a method of his own. When these punctures were exhibited at the Congress, six days afterwards, they had nearly healed, no traces of redness, inflammation, or pustule, remaining. He also showed another place where he had been subcutaneously inoculated three days before with a fluid containing bacteria. There had been local œdema, occurring six hours afterwards, but it had disappeared in forty-eight hours, and on the day in which he made his communication to the Society his general condition was excellent, his appetite was good, and he had no fever.

My own experiments on animals agree in a certain number of instances with those of Hiller. When the bacteria were filtered out of Cohn's nutrient fluid,[3] in which they are known to grow with exceeding rapidity, and were inoculated in the thighs of rabbits, the organisms ceased to be visible after the fourteenth day, though there was usually a marked deposit of a pasty material at the point of inoculation. The same thing was observed when inoculations were made with tongue-scrapings, or with diphtheritic membrane; the bacteria soon ceased to multiply, though the time at which the process stopped was variable; they became bloated and distorted, so that it was hard to make them out; after three weeks, they were rarely seen at all, though the focus of disease remained extensive, the animals quite often dying at a later date with symptoms of constitutional infection. Still, in all of these experiments it is not probable that the bacteria were ever completely isolated, for even when Cohn's fluid was used, where the only organic substance was the tartrate, fermentation took place, and some of the products of fermentation, perhaps in the form of minute particles, may have attached themselves to the bacteria which then may have been *carriers* of the poison—itself a chemical product. And yet I have, in repeated instances, seen fluids inoculated upon rabbits, where the microscope showed bacteria in considerable numbers, though no trace of a lesion was observed.

We have now to consider a topic of great importance to us, in further defining our subject, and this is the classification of the organisms that are now called bacteria, to which class the vegetable-germ theorists, almost without exception, refer the agency of disease. Their name does not by any means imply that they are all rod-shaped, though the word *bacterium* means a little rod, but rather that they belong to a class of which

[1] Allg. med. central Ztg., 1, 2, 1874. [2] Archiv f. klin. Chirurgie, 1875.

[3] Tartrate of ammonium, 1 gramme; phosphate of potassium, sulphate of magnesium, each, 5 decigrammes; phosphate of calcium, 5 centigrammes; distilled water, 100 cubic centimetres.

the rod-shaped bodies are common types. Various classifications have been given of them, those of Billroth and Cohn having met with special favor, and each of these being tolerably complete. I will venture to say, however, that no elaborate classification appears to be necessary for these bodies, judging at least from our present knowledge of them, so that I shall describe them by common terms, which can be as well understood by those who are not familiar with these matters as by those who have carefully studied them. Simple as the classification will be, it is perhaps convenient rather than accurate, for while it seems to me to cover the whole ground, it may be questioned whether it does not do more; *i. e.*, give a reality to some forms the existence of which is in doubt.

First we have (*a*) the very minute, round bodies, varying in size from those which are described as having a diameter so small that they can barely be seen by the ordinary high powers of the microscope, up to those which have a diameter of about $\frac{1}{27000}$ of an inch; these are the Micrococci of Cohn, and the Microbacteria of Billroth; then we have (*b*) the little, oblong, or slightly oval, bodies that have a length of about one-third the diameter of a lymphoid corpuscle, and a breadth of about $\frac{1}{27000}$ of an inch; they occur singly, or in chains of two or more elements; thirdly, there are (*c*) the little chains which often appear to be made up of minute spheroids; fourthly (*d*), the globular masses that appear to be made up of spherical bodies (Zoogloea of Cohn, Colonies of Hallier, Gliacoccos of Billroth); and fifthly (*e*), the filamentous bodies which are so often seen to glide about with an undulating or serpentine movement across the field of the microscope (Spirilla of Cohn).

I use the term bacteria for all these forms, and yet, as I have already said, objection may be made to this classification. It is held by some that the round particles known as micrococci, are nothing but either the rod-bacteria, seen end-wise, or merely particles of dead granular matter, or perhaps particles from the living protoplasm of cells. It is probably true that many mistakes of this kind have been made, and yet I should feel disinclined to believe that there are no round bacterial bodies, coming within the range already given, for it has seemed to me that the chains just described are composed of minute spheroids. As for the filamentous bodies known as spirilla, they are also thought by some to be composed of spheres. These bodies when single, or in segments, have two states, one of activity and one of rest; but mere absence of motion does not necessarily indicate that they have ceased to live, for under changed conditions they again resume their motions and multiply. All small spheroids have a motion within a limited area, but it is difficult, if not impossible, to distinguish in them that motion which is called "independent"—which is inherent in them as living particles, and is not caused by external circumstances. It has long been known that the mere fact of motion, in such particles, does not indicate life; for all particles are apt to show a kind of motion, which is greater in very fluid media, and less in dense media. These well-known facts, however, do not seem to be sufficiently prominent in the minds of some who have written on these subjects.

Real motion in such bodies may be certainly recognized, when they move against or across currents, and do not merely show irregular rotation or vibration in a limited area. The rod-bacteria, chains, and long filaments, or spirilla, just described, have clearly this sort of motion, under certain conditions of life. The spirilla often dart across the field of the microscope, stop suddenly, and then dart back, or off in some new direction. Various micro-chemical methods have been devised for assist-

ing the eye in detecting them. Among those that have claimed the most attention, are the methods of Letzerich, Eberth, and Hiller, all of which are more or less useful. Letzerich[1] employs a watery solution of iodine, which imparts to the rods and chains a deep-brown color; but Hiller has shown that this method is unreliable, from the fact that granular iodine is deposited, and that there are no means, *probably*, of distinguishing the iodine particles from the so-called spherical bacteria, or micrococci, etc., if such be present. The mere size of such atoms gives no criterion for determining their character, for, according to Cohn, the distinguished botanist, they vary in size within exceedingly wide limits. My own experience in using this method has inclined me to coincide entirely with Hiller.

Eberth's test consists in boiling the suspected bodies in alcohol, or in caustic alkalies, by which process the rods, chains, etc., are unaffected; but this method is objectionable from the fact that if we wish to get rid of all the oil, which resists these reagents a long while, the suspected tissue is itself apt to be destroyed.

The best method, in my experience, has been that of Hiller.[2] I have found the following modification of it to answer well: Make a ten per cent. watery solution of caustic potassa, in which the tissue to be examined is then immersed for an hour and a half, then washed in distilled water, and finally plunged into a mixture of tincture of iodine and distilled water, 1–25; after remaining in this menstruum for fifteen minutes, the substance assumes a brownish or deep-yellow color. The fatty matters are more or less dissolved, excepting, in some cases, the larger oil drops, but the bacteria retain the color more or less deeply. Such of the round, oily particles as remain after this treatment, can usually be distinguished from bacteria, by the fact that they refract the light strongly, while bacteria have a dull, lack-lustre appearance.

Sometimes we are called upon to decide as to the character of the dark substances that exist in cells, to determine whether they are bacteria or not. I at one time adopted a modification of this plan. In an instance where a rabbit had shown signs of constitutional disturbance after inoculation with putrid matter, I examined some of the blood on the second day after removal from the body, when bacteria were very plentiful, and when most of the white blood-corpuscles had disappeared, while those that remained were enlarged and granular. I added liquor sodæ in excess to a small amount of the blood, allowing it to act for one and a half hours. At that time the white corpuscles had swollen and were generally dissolved, liberating a large number of granules. Ten of these were kept in view, and moved about in all directions, turning and twisting; at one time they arranged themselves in a cluster, and then again separated, and formed a chain. During this time, the bacteria of rod-form had become more and more invisible; while these particular bodies, on the other hand, had become brighter and brighter. During all this time there was little or no change in a number of oil drops that were scattered about the field. Here, then, we have an instance of liquor sodæ having afforded us a means of differential diagnosis, in an optical manner, between bacterial bodies and certain particles that took the chain-form, and that came from the interior of white blood-corpuscles.

One of the most important reasons for putting little reliance on exact measurements as to the size and form of bacteria, is that these charac-

[1] Virchow's Archiv, lxii. 3, 1875.

[2] Virchow's Archiv, lxii., Jan. 1875.

teristics are probably apt to change under varying conditions. Bastian tells us that if we take an ordinary organic infusion, and expose it to a warm temperature, it will show rod-bacteria. If we add a drop of acetic acid, we will get bacteria of larger size; adding a few drops more, the changes take place less rapidly, and the organisms, instead of multiplying as they did before, now grow continuously into filaments. This ground is said to be held by Trécul,[1] of Paris, and is favored by Beale;[2] my own experience is not sufficient to justify me in forming an opinion on this point. I may here add one point, viz., that I have seen no reason to believe that there is anything like a genetical relation between bacteria and the mould or sugar fungus.

I have already stated that my experience agrees with that of Chauveau and Sanderson, that the virulent principle is *particulate*, in certain instances at any rate. It is quite clear that very virulent fluids may be rendered quite harmless by passing them through a clay filter; this I have had repeated opportunities for observing, as far as relates to inoculation with putrid infusions, or with diphtheritic matter; and yet when we come to the question, *are these poisonous particles bacteria?* we know that by successive filtrations, as through paper, where the bacteria of distinctive forms are separated and removed, the fluid may still be poisonous. Granules are often observed in such fluids; *are they spherical bacteria, or the spores of these or others?*

We must remember, however, that rod- and chain-bacteria may be introduced in large numbers into the system without producing any lesion. When they *appear* to have been isolated, as in Cohn's fluid, this, as I have stated, is perhaps really not the case, but some organic compound may have attached itself to the vegetations, and thus have conveyed the septic influence.

Various efforts have at times been made to determine whether or not the poisonous matter is albuminoid. Chauveau, Onimus,[3] and Sanderson,[4] have worked in this direction. Sanderson and Onimus tried dialysis, and Chauveau diffusion. The former, in investigating the cause of the cattle plague, passed the virulent fluids through parchment-paper; the dialysate was wholly harmless, while the liquid that did not pass was poisonous. As the question with Sanderson was whether the contagia were crystalline or colloid, he decided in favor of the latter possibility. Onimus concluded that the matters were albuminoid from experiments conducted in much the same method. But there were in both sets of experiments, possibilities of error, arising from the fact that it was not certain whether albuminoid substances did not actually pass through the membranes, which it is now known that they will do, when certain quantities of the phosphates or carbonates happen to be present.[5] It would, therefore, be erroneous to conclude that the poison remaining behind the filter was necessarily albuminoid, when it was uncertain whether the harmless filtrate did not also contain albumen.

Chauveau appears to have shown, by his experiments, that it is extremely unlikely that the poison in vaccine virus is due to a soluble, albuminoid substance. His method will be briefly noticed. He took vaccine lymph, collected from the arm, and placed it in a test-tube standing in an upright position. The introduction of the lymph into

[1] Lancet, May 15, 1875, p. 684.
[2] Disease Germs, p. 37.
[3] London Medical Record, November 19, 1873.
[4] Twelfth Report of the Medical Officer of the Privy Council, 1869, p. 233.
[5] Graham, Proc. Royal Society, 1861, vol. ii. p. 243. I have not been able to refer to this paper.

the test-tube was effected with such care that the liquid did not touch the sides of the glass in the act of filling. Water, to the depth of a few lines, was then added with similar precautions, and the whole was allowed to stand for twenty-four hours. Although no membrane was used, yet it is said that when proper care was exercised, the liquids did not mix, except in the immediate neighborhood of the surface of junction. All the soluble constituents of the vaccine matter passed upwards into the water. At the end of twenty-four hours, the most superficial layer was removed by dipping into it the end of a fine capillary tube. This supernatant liquid was then examined microscopically, and tested for albumen. Now albumen, though held to have the least diffusibility of chemical compounds, was found in the water, the inference being that no other soluble chemical matter had been left behind. This upper stratum was used upon heifers and children, but without success, while the lower stratum produced the vaccine disease. Chauveau had previously employed a method called *subsidence*, in which, by adding ten volumes of water, he had found that the leucocytes were separated. These did not produce the disease, but the stratum that produced infection contained minute, microscopic granules, and it was therefore concluded that the disease was produced by them. Sanderson corroborated these conclusions in the great majority of his attempts, though he experienced some difficulty in the required manipulations, which need a good deal of delicacy.

Colin[1] asserts that he has been unable to "diffuse," as Chauveau claims to have done, and intimates that the method is faulty, and that no such real diffusion takes place; Chauveau, however, insists that the granules, even when washed with immense quantities of water, retain their virulent quality, and compares them in this respect to the spermatic elements of seminal fluid. The supernatant liquid which is not poisonous, responds to the test for albumen by heat and nitric acid. In farcy, Chauveau finds that the granules are as poisonous as the pus itself. According to Panum's[2] experiments, the virulent substance of putrid infusions is not destroyed by boiling, and is soluble in water, but not in alcohol; which qualities make him surmise that it may arise from the decomposition of albumen, or may perhaps be secreted from the bacteria, though he feels uncertain as to what the nature of the poison really is.

In prosecuting this line of inquiry, I have performed a number of experiments in co-operation with Dr. Edward Curtis, Professor of Materia Medica in the College of Physicans and Surgeons, New York, whose name I have previously mentioned in connection with other topics. The following questions were some of those that presented themselves for solution:—(1) Is the poisonous matter in putrid infusions destroyed by boiling and evaporation to dryness? (2) Is it soluble in alcohol, or not? (3) Is it soluble in water, after boiling in water and in absolute alcohol? (4) If there are granules in the fluid which prove poisonous, will they breed bacteria?

The liquid employed in these experiments was obtained from an infusion of a calf's liver, that had been allowed to become putrid; the liquids and solids used, were inserted into the muscular tissue of rabbits, and examined at varying times thereafter.[3] The following conclusions seemed in my opinion to be warranted:—

[1] La France Médicale, Fév. 26, 1876. [2] Virchow's Archiv, lx. 3 and 4, 1874.

[3] Many of these experiments have already been reported. In fact, this entire paper embodies views which were published in 1875. Medical Record, Dec. 18 and 25, 1875.

(1) Putrid matter, when introduced into the system, is capable of producing a well-marked train of symptoms, which are extraordinarily like those of ordinary *sepsis*.

(2) The poisonous quality does not reside in the absolutely clear liquid, when entirely freed from granules. This was shown by porous clay. The apparatus used in these experiments was devised by Dr. Curtis, and consisted of a porous-clay cylinder or cup, over the bottom of which was drawn a bit of rubber tubing, which was thick, and made to grasp the cylinder firmly; in the opposite end of this tubing was fitted a cork, through which a glass tube passed, so as to connect the chamber beneath the cylinder with the interior of a small glass jar that could be exhausted of air by means of a common exhausting syringe. When the liquid to be filtered was placed in the porous cylinder, and the air was removed from the glass jar, the liquid passed through the cylinder, and stood in beads or drops on its bottom, from which it was collected in small quantity without much difficulty.

(3) The poison is sometimes separated by coarse methods of filtration, such as by common filtering paper. When Cohn's fluid was passed through the equivalent of 25 thicknesses of ordinary filtering paper, it produced no lesion, and yet Cohn's fluid, unfiltered, and ordinary putrid infusions, produced about the same lesion.

(4) Continued boiling and evaporation to dryness does not destroy the poison.

(5) Continued boiling, and evaporation to dryness, and boiling with absolute alcohol, does not destroy the poison.

(6) The dry alcoholic extract, freed from alcohol by evaporation with heat, is poisonous.

(7) My experience with regard to the albuminoid character of the poison, has not made the matter clear to me, for while in these cases the dry alcoholic precipitate, after continued boiling, was poisonous (and it contained the albuminoid substances), yet the liquid alcoholic extract in one case produced a lesion, and in another did not. In my first account of these experiments,[1] the results were stated as above, but in a subsequent series of experiments, the liquid alcoholic extract produced extensive lesions in two cases in which it was tried. In other words, when the albuminoid deposit produced by both boiling and alcohol was separated, and water afterwards added, the virulent matter appeared to reside both in the precipitate and supernatant fluid. These experiments do not, therefore, decide anything as to the albuminoid character of the poison.

(8) It further seemed to be shown of putrid matters which, after being filtered several times, were boiled to dryness, then boiled with absolute alcohol, and again dried and extracted with water, that the ordinary paper filtrate was poisonous.

(9) On examining this watery liquid, it was found to contain granules, and yet, when these granules were placed in *vacuum-tubes*, they in several instances failed to show any development of bacteria under such circumstances as favored the growth of bacteria. The suspected solution was put away in bent tubes, with suitable precautions, and while similarly arranged tubes that had been contaminated with the barest trace of bacteria, were found turbid from the production of these bodies seventy-two hours afterwards, the suspected liquids were perfectly clear, with one exception, where there was a slight pellicle on the surface. It is true

[1] Loc. cit.

that, after a variable time, several tubes became contaminated, and yet others remained clear for months, and one for nearly a year, and as length of nozzle and fineness of neck are necessary conditions for success in the experiment, and as those thus provided held out best, it is fair to suppose that with the others some error was committed in carrying out the experiment, and that thus the bacteria of the air had in some way gained an entrance. That this is possible we have already seen.

It is proper now to stop here, and note that the granules have been very frequently noticed in the poisons of infectious disease; they seem destined to play an important part in the discussions now pending on the subject. There can be no question that in most of the diseases that are assumed to have a definite vegetable form as their immediate cause (for this is certainly the way in which the relation has been stated over and over again by the vegetable-germ theorists), authors have given the most conflicting accounts of the organisms, and we may fairly say that, with the exception of relapsing fever, not a single form has been accepted by any considerable number of germ-theorists as the agent, *par excellence*, of the disease. We have observed that, according to Chauveau, Sanderson, and Beale, certain granules appear to be associated with the origin of disease, and that this fact has been the point of departure of both the vegetable-germ theorists and the bioplasm theorists, while it is not incompatible with the physico-chemical theory. Let us examine these points a little more closely.

It is clear that if the vacuum-tube experiments be of value, they show that the particles observed are not the spores of bacteria. What evidence is there that the matter in question is organic and pertaining to the body? It is quite clear that, to the eye, using the ordinary powers of the microscope, these particles cannot be distinguished from one another with certainty. Beale,[1] however, assures us that by using extremely high powers, such as the $\frac{1}{25}$ or the $\frac{1}{50}$, he has seen them at times dividing, and at others increasing in size, and that he has colored them with carmine. Some of these may, he says, be no more than $\frac{1}{100000}$ of an inch in diameter. In diseased conditions of the body, he believes that such particles may increase enormously. Belief in this doctrine is, of course, inconsistent with the old cell-doctrine, by which this body was regarded as the ultimate morphological element of the system; and yet we may safely say that this old theory is not by any means what Schleiden and Schwann once held.

It does not appear to be necessary that a cell should have an enveloping wall, inclosing a softer material in which is a nucleus. Max Schultze showed that many important cells possessed no cell-wall, and that some might not have a nucleus. Our notions, therefore, have been greatly modified in these respects, and a living cell is now held to be merely a mass of glutinous, viscid matter, which is endowed with peculiar qualities, called vital, such as amœboid motion, molecular motion, and the like. As to the nature of this viscid substance, there is hardly much uniformity of opinion, as yet; some holding it to be formless and structureless, while others believe that it consists of networks, and others again, as Beale, that it is made up of minute spheres, closely packed together, and that these spheres may in turn be made up of other spheres. I hardly think, however, that microscopists are ready to agree upon any staining matter that is fitted to distinguish protoplasm or bioplasm in all cases, and these

[1] Disease Germs, p. 246.

statements of Beale must, therefore, be accepted with caution. Even if the particles be large, it is not certain that carmine will differentiate them. It has, to be sure, a preference for animal tissues, and yet it will also stain various other substances, if they remain long enough in the staining fluids. While, therefore, it is conceivable that the ultimate living elements of the body reside in particles which go to make up cells, we have no sure means of recognizing them, and of deciding positively that they are animal, and not vegetable, or whether or not they belong to that ill-defined class called granular matter. If, however, we believe that such minute portions of the body are the agents of disease, then we have to assume that they have a capacity for life which is remarkable: that, in fact, they can be subjected to extremes of heat and cold, or to the action of various acids and alkalies, and that they can be separated for months, and perhaps years, from the body, without losing their active qualities—all of which does not comport with the opinions commonly held about living animal matter.

The name "physico-chemical," as applied to the theory which is thus designated, is objectionable, as it dates back to the old-fashioned notions of Liebig, when the ideas held on these and kindred subjects were quite different from those of the present day. Liebig[1] held that the ferment was a portion of organic matter, which, itself unstable, was capable of communicating molecular movement (chemical change) to certain substances. Bastian, the exponent of the modified doctrine, holds, as already stated, that "living organisms, though they may act as ferments, act in this capacity merely by virtue of the chemical changes which the carrying away their growth necessitates, and that other chemical changes, taking place during the decay of organic matter, may make fragments of it (in the dead state) almost equally capable of initiating fermentative changes in suitable media."

This statement would appear to show that the friends of the theory are not altogether willing to deny to bacteria and allied forms, an influence in the cause of disease, but that they regard them as perhaps remote factors, the important one being the chemical change exerted by the growth of the microphytes, while, at the same time, dead organic matter may have a similar power. This theory is, therefore, properly a chemical one, and admits of two possibilities; either, on the one hand, that the cause of disease operates through chemical changes under the influence of minute organisms, or on the other hand, that it so operates under the influence of dead portions of the body. The former hypothesis is unlikely, because the virulence of diseases can sometimes be shown to be independent of bacteria; the latter one, however, is by no means impossible, though we know but little in its favor. If we assume that there is a logical connection between the processes of fermentation and disease, and decline to accept the hypothesis that minute organisms have that absolute *rôle* that has been given them, we may believe, with Hiller and others, that the ferment is a chemical substance. This is the view held by Bert with relation to vaccine virus, which he subjected to conditions under which life appeared to be impossible.

Under such an hypothesis, there is formed in the tissues, in certain diseases, a material which is able to resist various reagents (heat and cold, and the like), and which, when transferred to another organism, may occasion the formation of like compounds, just as the sulphate of sodium, instanced by Bastian, when placed in a liquid having the necessary ingredients for

[1] Lancet, April 10, 1875.

forming it, will increase in amount.[1] Now we actually know that the poisons of the cobra and of the rattlesnake, which are elaborated from healthy tissues, are capable of being subjected to conditions that are incompatible with life in its common acceptance. The poison of the cobra may be diluted with water, ammonia, or alcohol, without having its deadly qualities affected, while, according to Weir Mitchell,[2] the poison of the rattlesnake can be boiled or frozen, or subjected to the action of strong sulphuric or muriatic acid, ammonia, chlorine water, iodine, soda or potassa, and yet, as far as the matter has not burned, its virulence be unaffected. These qualities remind one strongly of the poison in putrid matters, where, according to my own and Panum's experiments, neither ordinary boiling in water, nor in absolute alcohol, nor evaporation to dryness, destroys its active qualities.

Let us glance, in conclusion, at the leading opinions at present entertained in regard to those of the infective diseases that have been most thoroughly studied with reference to the materies morbi. I shall refer merely to cholera, vaccinia, the carbuncular diseases of man and animals, typhoid and relapsing fevers, and diphtheria, in regard to which the most specific descriptions have been furnished of the organisms supposed to be concerned in their production.

The investigations which were occasioned by the *cholera* epidemic of 1849, appear to have been the first in which attention was called to certain particles, alleged to be the cause of the disease. These particles were called the cholera-fungus.[3] In 1867, Hallier[4] published a work in which he described the supposed ferment, the cholera micrococcus, a granular mass that by cultivation underwent a great variety of changes in form. Cohn[5] and many others followed with descriptions of the fungus. It is safe to say, however, that there has been no general agreement between the most prominent writers on this subject as to the specific form of the supposed contagium. In the Congressional report of the epidemic of 1873, in the United States, we find it stated[6] that no microscopic changes peculiar to cholera were found in the discharges, though high powers of the microscope were used; this being in agreement with the statement of Macnamara, who used a one-seventieth inch lens. Molecular matter was always seen by these observers, and it is stated in the report that from this molecular matter in the vibrionic stage of decomposition, and not from the vibriones themselves, the dejecta of cholera patients are capable of setting up a morbid action in the intestinal canal of those who receive them.[7]

Among the most recent and complete experiments on this point are those of Lewis and Cunningham.[8] They injected 36 dogs, through the veins, with choleraic dejections. In 15 cases they obtained positive results: that is, death was evidently due to the matter injected. In 17 of the cases this matter had been kept for ten minutes at a temperature of 212° Fahr. In the remaining 19 cases it had not been exposed to heat. In the 17 cases the mortality was 47 per cent.; in the 19 others it was only 36 per cent. It was thus found that a continued temperature of 212° appeared to have no influence in abating the virulence of the matter, for, when it was boiled, the mortality was even a little greater than in the other cases. These observers also believed that bacteria could not

[1] Bastian, loc. cit.
[2] Smithsonian Contribution, p. 46.
[3] Budd and others.
[4] Das Cholera-Contagium.
[5] Biologie der Pflanzen.
[6] Report, etc., p. 36.
[7] Ibid., p. 43.
[8] Journal of Anatomy and Physiology.

be the cause of cholera, and in fact they found similar forms in the lesions produced by inoculation of various other substances, such as solutions of ordinary fæces, or urine.[1]

About ten years ago, it was noticed that *vaccine* virus contained minute spheroids, and they were described in 1867–1868.[2] Cohn called them *Microsphæræ vacciniæ*, and described them as small, colorless cells, sometimes in rosary-like chains of eight or more links, and sometimes clustered together. Sanderson thought that they were "filamentous, not jointed, but branching, and giving off from their extremities microspheres or conidia, much after the fashion of the penicillium." Having been favored by Dr. F. P. Foster, Director of the Vaccine Department of the New York Dispensary, with an opportunity of inoculating calves, I performed the following experiment in relation to this matter. Having prepared a solution of salicylic acid, with sodium phosphate as a solvent, I combined it with equal parts of vaccine virus taken immediately from fresh vesicles, and having allowed the two to remain in contact a minute, they were inoculated in my presence, on a calf, in the usual way. Now this proportion of salicylic acid ($\frac{1}{500}$) had been shown by previous experiment to be able to prevent the development of bacteria for from twelve to twenty-five days, and yet in this instance regular vesicles appeared at the proper time in nearly all the inoculated points. I cannot but think, therefore, that this experiment offers strong evidence against the theory that vegetable germs have to do with the cause of disease.

The name *Carbuncular Diseases*, which the French employ synonymously with the German word Anthrax, has proved to be a convenient form for a rather remarkable group of contagious diseases, which have a certain connection with one another, and which include malignant pustule, splenic fever, and the disease called "mycosis intestinalis." The post-mortem appearances in all these diseases are remarkably similar. Balls of rounded particles are said to be found in the affected parts, and rod-shaped bacteria in the circulating blood; the spleen is enlarged; there are hemorrhagic infarctions of the intestine, causing sloughing of the mucous membrane, exudation and infiltration of serum, and enlargement of the glands near the points involved. In such cases a pustule often appears, and hence the name Malignant Pustule; and yet it may not be necessary to the disease, which often proves fatal without it. Now, in these diseases, Pollender and Davaine[3] observed minute rods, which Davaine called bacteridia, and which he said were always motionless; and he has made such extensive studies of them that his opinions have become well known. He regarded them as necessary causes of the diseases in question, and as essentially different from what he called the bacteridia of decomposition, which were really what we know as the bacteria of putrid matters. His views have, however, not always met with acceptance, and notably have been opposed by Bouley,[4] Gaillard, and others, who assert that the blood may be infectious without the presence of rods. Bodinger, who is now regarded as one of the foremost German authorities, states that he also has produced the disease in question in a similar way, but that in such cases there have always been spherical bacteria present. We may justly ask, in view of what has already been cited, whether spherical bacteria were really present, and whether the particles he describes may not have been other solid particles?

[1] Tribune Médicale, 369, 1875.
[2] Virchow's Archiv, 41, 42, 1867–8.
[3] Davaine in 1850, and Pollender several years previously.
[4] Cyclopædia of Practical Medicine, vol. iii. p. 377.

Among the most recent and best recorded accounts of the vegetable organisms connected with *typhoid fever*, are those of Klein,[1] of London. He declares that he has found peculiar bodies, in this disease, at or near Peyer's patches. These bodies were carried along the lymphatics or bloodvessels of the mucous membrane; in color they were often yellowish-brown, and in size varied from one-fourth to three times the size of a human, red blood-corpuscle. It will be noted that if we accept these views, we are carried away from the subject of bacteria towards that of the other vegetable organisms, called torulæ, which in my opinion have not been shown to have any genetical relation with bacteria. These views of Klein are comparatively new, and as yet I have failed to find any confirmation of them.

In regard to *relapsing fever*, the microphytes are pictured with more vividness than in any other disease. In 1872, Obermeier, of Berlin, gave an account of certain bodies, long and filamentous in form, which were found in the blood during the rise of the fever, disappearing with its fall, and reappearing during the relapse. These observations have been very largely confirmed, but, on the other hand, Laptschinski made frequent searches for these bodies, but never found them, though he did observe an enormous increase in the number of the white blood-corpuscles, which were unusually granular; when the fever decreased, the number of the white blood-corpuscles diminished. Recently, Heydenreich,[2] of Moscow, examined the blood in sixty-four cases, and he asserts that he found the spirillum in every instance, and that it was the same as the Spirochæte plicabilis, of Ehrenberg, which this observer discovered in water. In most cases these bacteria were found at the beginning of the febrile exacerbation, sometimes even preceding it. Before the fall of the temperature, the bacteria generally disappeared. On the other hand, we are told by Motschutkoffsky,[3] of Odessa, that the blood of relapsing-fever patients was readily and successfully inoculated upon healthy human subjects, though not on animals. The blood was only poisonous when it was taken during a febrile exacerbation, but it made no difference whether it contained spirilla or not.

In the matter of *diphtheria*, I may speak with more confidence, as my personal experience in this department of the subject is larger than in any other. The relation of bacteria to diphtheria has been made most prominent by the writings of Oertel, Hueter, and others; and, in conjunction with Dr. Edward Curtis, I have made some extended researches on the subject. Oertel, and others of the German school, plainly ascribe to the micrococcus found in diphtheria, the cause of the disease; at least I can understand them to imply nothing else. In the first instance, it must be said that the real presence of bacteria in diphtheritic membrane is usually a matter of ready proof. They exist in collections on the surface of the membrane, and often in it, as well as in the secretions of the pharynx, trachea, nasal passages, etc. They are to be found in masses, or separate, and usually may be distinguished as rod-shaped. It is true that they often appear as spheres, but this appearance is apt to be deceptive, for, when they are accumulated together, they are often arranged in extremely regular order, and they then give this peculiar appearance. To show that in such cases they are rods, is not a matter of much difficulty: Press the cover against the slide, and the

[1] Report of the Medical Officer of the Privy Council, 1875.
[2] St. Petersburger med. Wochenschrift, 1, 1876.
[3] Centralblatt, and St. Louis Medical and Surgical Journal, May, 1876.

more prominent rods will be pressed down, showing their peculiar shape. Another source of deception is the fact that these bacteria are apt to move about with a gyratory motion, with extremity directed towards the observer. Watching such a particle carefully, it may sometimes be seen to turn and expose its side, and then we may see that it is nothing but the ordinary rod-bacterium. When we have to do with other particles, then indeed it is well to give diagnostic points by which we may distinguish them from ordinary granular matter, such as fragments of tissue, minute oil particles, pigmentary matter, etc. I feel confident, however, from repeated examinations, that no particles have been described by Oertel as micrococci, that can be positively shown to be present in diphtheria alone. On the contrary, both Dr. Curtis and myself have repeatedly examined the accumulations in our own mouths, and have there seen the forms which are described as belonging to diphtheria.

And yet diphtheritic membrane is very poisonous, and fatal when inoculated in the muscles of rabbits, as I have had repeated opportunities of observing. I may here state that Oertel's descriptions are the most elaborate and, perhaps, the most clearly given of any writer's on the experimental pathology of diphtheria; as far, however, as my experience has gone, it has in most instances failed to verify them. In inoculating the corneæ of rabbits, in a number of instances, no lesion followed, beyond an ordinary keratitis, and none of the rabbits died; in fact they all recovered quickly. The fresh diphtheritic membrane, when inoculated, was very fatal, the tendency to death being extremely common about the end of the second day. Of 27 rabbits inoculated with the solid membrane, or an aqueous infusion of it, 20 died, and 12 at about the end of the second day, or between the second and third.

Now the membrane when thoroughly boiled, was equally fatal in two cases, and yet boiling is said by most excellent authorities to destroy bacteria. In fact, this is a matter of general belief. Furthermore, bacteria were shown to decrease rather than increase, when introduced into the system, so that certainly after two or three weeks they had either become distorted, so as to be hardly recognized, or they had altogether disappeared. When very strong solutions of salicylic acid, combined with phosphate of sodium as a solvent, were made up into a paste with diphtheritic membrane, and inoculated in the thighs of rabbits, they produced a fatal disease, though the lesions were not as extensive as when diphtheritic membrane alone was used; and yet, in my experience, salicylic acid in the proportion of $\frac{1}{500}$ has, as before mentioned, prevented the development of bacteria for from twelve to twenty-five days.

I believe that I have now given a tolerably fair account of the most prominent views entertained on the matter of disease-germs, and think that I am warranted in submitting the following conclusions with regard to the present status of the question:—

I. That, as far as inquiry has been made as to the nature of the active principles in infective diseases, it is probable that in a certain number the matter is particulate, or molecular in form.

II. That in regard to the causes of septicæmia, pyæmia, puerperal fever, erysipelas, and hospital gangrene, and those of cholera, vaccine-disease, the carbuncular diseases of men and animals, typhoid and relapsing fevers, and diphtheria, there is not satisfactory proof that they are necessarily connected with minute vegetable organisms.

III. That the real nature of these causes is still uncertain.

VITAL STATISTICS OF BUENOS AYRES.

BY

G. RAWSON, M.D.,

OF BUENOS AYRES.

A BRIEF review of the vital statistics of Buenos Ayres may perhaps awaken some interest in the International Medical Congress now assembled in Philadelphia, not only as adding to the data accumulated from day to day as elements of Sanitary Science, but as presenting circumstances peculiar to that Argentine city itself.

Buenos Ayres, next to Rio de Janeiro the most populous city in South America, has grown with extreme rapidity, having tripled its population within the last twenty-five years, mainly owing to the vast tide of emigration from European shores. But, even in the event of a diminution of the influx of immigrants, and a corresponding reduction of the rate of increase, it is almost certain that Buenos Ayres will, at the close of the present century, have upward of half a million inhabitants. Situated in latitude 35° south, with a genial climate, it is exempt from the extremes of temperature so common in other localities; and, were it not for the modifying influences exercised upon its sanitary condition by the increase of population, and by the circumstances usually attending that increase—when not overruled by strict observance of the admonitions of science—it would be as healthy a city as its name (*good air*) would seem to imply.

But the death-rate in Buenos Ayres attains proportions by no means satisfactory; and the recent epidemics, particularly the yellow-fever scourge of 1871, show that much has to be done before the city can regain that salubrity reasonably to be expected from its situation and soil, and from the prevailing winds and other climatic conditions with which it is favored by Nature. Epidemics are warnings to mankind—warnings all the louder in proportion to the severity of the visitation; and Buenos Ayres has learned from her recent sufferings the lesson to be desired. Works, both above and beneath the surface, are now in prosecution for the sanitary improvement of the city, by means of a system of drainage and underground disinfection which will cost twenty millions of dollars (eight millions have already been expended), and which, when completed, will be one of the most efficient in the world.

Besides, the population of the city is, to a great extent, composed of foreign elements permanently incorporated therein and constantly increasing. In this respect Buenos Ayres bears a striking analogy to many cities in the United States, and, like these, presents a series of phenomena peculiar to this species of social evolution, as revealed in its vital statistics. The analogy here referred to I shall endeavor to point out in the course of the present essay.

Population of Buenos Ayres.—It is necessary, first of all, to determine the population of Buenos Ayres, and its component elements; but the accurate accomplishment of this is difficult, owing to the long intervals between the censuses, and the rapid increase of the number of inhabi-

tants. The mode of development in cities of the New World is in general so irregular that, in measuring their growth, it will not suffice merely to determine the difference between the numbers of births and deaths. The increase resulting from that tardy process is insignificant when compared with that derived from immigration, and with that which is due to the irresistible attraction which populous centres exercise at all times upon neighboring towns. This species of attraction is, from causes peculiar to the Argentine Republic, more powerful in Buenos Ayres than in other American cities of similar growth. In 1871, for example, the number of victims of the disastrous yellow-fever epidemic exceeded that of the births in the same year by 13,206: but the equilibrium of the population was more than re-established by the influx of emigrants from Europe, and from the surrounding provinces and republics. By comparing the total number of births during the period from 1858 to 1872 with the total number of deaths during the same period, including, of course, the victims of the cholera of 1867 and 1868 and those of the yellow fever of 1871, we observe an excess in the deaths, of 1778. Hence, if Buenos Ayres had depended solely upon its own resources for its growth, its population, instead of increasing, would have diminished. But the ravages of this frightful mortality were more than compensated for by immigration, and the remarkable growth of the city pursued its course without any apparent interruption.

The progress becomes more and more marked, dating from 1852. In the absence of official data, it is scarcely possible to estimate the population of Buenos Ayres in that year, which was marked by a political event of transcendent importance—the downfall of the dictatorship by which the nation had been oppressed and depopulated for the space of twenty years. Numerous emigrants returning to their homes after a prolonged proscription; the establishment of liberty, political and civil; the opening the navigable rivers to the vessels of all nations; the discovery of riches susceptible of being developed advantageously in the Argentine Republic by the hand of man; and the facilities offered by both the government and the people to foreigners desiring to take up their residence there, induced a stream of European immigrants, which continued uninterruptedly for a number of years. Dating from the beginning of the influx of foreigners, the rapid growth of the population became evident, and about that time the greatest increase took place. Nevertheless, no census of the city was taken until 1855, nor was a second effected until 1869, the year of the general census of the republic. According to the terms of the constitution, the general census will in future be taken every ten years.

The two censuses referred to are, therefore, the only available data on which to base my computations; those for other years must, of necessity, be only approximate. In the census of 1855, the total population of the city was set down at 91,548, and in the general census of 1869, at 177,787. Hence, the mean annual rate of increase during the period embraced between these two extremes would be 4.8 per cent., always supposing the progression to have been uniform. Now, by applying the same system of reckoning to the previous years, that is to say, from 1852, and to the successive years down to 1875, the population of Buenos Ayres would be as in the annexed table in the years therein expressed:—

1851, by calculation	76,000	inhabitants.
1855, according to census	91,548	"
1869, " "	177,787	"
1875, by calculation	230,000	"

On reviewing the foregoing statement of the increase of the population, two objections are apparent, and I must now endeavor to explain them. They are: first, the epidemic of 1871, which would appear to have checked for the time being the numerical advancement of the population; and, second, the perceptibly decreased immigration in the last two years, 1874 and 1875.

Buenos Ayres is not only the chief port, but also, by reason of its position, the natural centre of all movement in the republic. It is the point of disembarkment for the immigrant on his arrival, and of re-embarkment for those who return to Europe. Passengers, whether immigrants or not, coming from Montevideo, likewise land at Buenos Ayres; and there they go on shipboard again when leaving to go back. There, too, is the rendezvous of all craft navigating the tributary streams of the Rio de la Plata. Hence, the movement of passengers throughout the year may at all times be readily ascertained, and the balance to the credit or debit of Buenos Ayres accurately determined by comparison of the arrivals and departures. The communications by land are extremely limited, and confined to the rural districts of Buenos Ayres and the neighboring provinces. It is generally admitted that more than two-thirds of all immigrants to the republic, come whence they may, remain in Buenos Ayres city, the remainder being distributed through the surrounding country. The following table shows the movement of passengers for the port of Buenos Ayres in the years 1864–72:—

Years.	Arrivals.	Departures.	Balance remaining.
1864	29,307	16,745	12,562
1865	30,556	24,434	6,132
1866	40,132	20,658	19,474
1867	42,729	21,154	21,575
1868	56,354	25,342	31,012
1869	73,045	29,990	43,055
1870	81,166	33,450	47,716
1871	49,741	28,468	21,273
1872	70,991	36,756	34,235

As seen in the foregoing table, in 1870, the year immediately following the general census, the balance in favor of the population reached the highest point so far known, as far as the arrival and departure of passengers were concerned. That year was an auspicious one for the city. Immediately after the termination of the Paraguayan war, a vast accumulation of capital was effected, credit facilities were opened up, encouragement was offered to such as desired to embark in new industrial enterprises, and important public works were undertaken, such, for instance, as city railroad lines: and the result of all these favorable circumstances was the attraction to, and permanent establishment in, Buenos Ayres, of a vast number of people from foreign countries and from the other provinces of the republic. The influx of strangers at that time amounted to two-thirds of the balance of 47,716 mentioned in the table, that is to say, over 30,000 new inhabitants, which is equivalent to three and one-fourth times the usual rate of increase. Such an excessive augmentation of numbers, at a time when the city was not suitably prepared to receive them, or to afford them necessary accommodation, gave rise to an accumulation altogether incompatible with the general good health, and contributed, beyond all doubt, to the awful severity of the epidemic of 1871, by which more than 12,000 foreigners were carried off.

The yellow fever committed its ravages in the midst of an overcrowded city. But, notwithstanding the great mortality, there still remained,

with a surplus for the following year, a mass of inhabitants greater than would have been obtained by calculation, adopting the established annual rate of increase, and to which should be added the number of the immigrants of 1871 who settled in the city. These remarks may serve to meet the first objection above alluded to.

As for the second, suffice it to state that, in 1872, the tide of immigration again began to flow, and foreigners arrived in such numbers as to leave a balance of 34,235; and that in 1873 immigration reached the highest figure ever attained in South America, leaving a balance much more favorable still than that of the year immediately preceding; I deeply regret that I have not at hand the official returns to offer in support of this statement. In 1874 and 1875, by local causes easily explained, and others of a general character which have produced and still produce such grave perturbation in commercial and industrial circles throughout the world, immigration to Buenos Ayres was considerably diminished, while emigration was increased in a proportionate degree. There has, nevertheless, been no instance as yet, even at the worst, of the departures having exceeded, or even equalled, the arrivals; a balance, however small, still exists in favor of the latter. Now, taking into consideration the excessive accumulation which occurred in the two years immediately preceding the crisis, the same reasoning may apply to 1874 and 1875 as we have already applied to 1871, with the favorable difference in the case of the two former years that they were marked by no such catastrophe as the epidemic of the latter, to exercise a depressing influence upon the population. Hence, the adoption here of the mean annual rate which served us in estimating the number of inhabitants for the earlier periods, would seem perfectly justifiable; and we may, without any fear of exaggeration, set down the population of Buenos Ayres in 1875 at 230,000.

This population is spread over an area of 1620 hectares (or 6 square miles approximately), or a mean of 70 square metres (= $83\frac{2}{3}$ square yards) to each individual. Of course, the distribution is not always uniform. There are many districts much more densely populated than others, and the tenement-houses, though disseminated through all the districts, are in themselves centres of accumulation, pernicious alike to the physical and moral well-being of the community. The streets are for the most part narrow, barely eleven metres in width; and the public squares are few and of inconsiderable dimensions. Within the last few years, a vast system of horse-railways has been introduced, with an aggregate length of seventy miles, by means of which cheap mode of conveyance a certain degree of expansion is afforded to the inhabitants. It may here be observed that, with the exception of Philadelphia, Buenos Ayres has, in proportion to its population, a greater extent of horse-car lines than any other city in the world.

From this short history of the development of the population of Buenos Ayres, it may be presumed that it is largely composed of foreign elements. Indeed, the census of 1869 shows that, in that year, foreigners constituted almost one-half of the total number of inhabitants.

Total population		177,787
Argentines	89,666	
Foreigners	88,121	
		177,787

Doubtless the proportion of foreigners has increased in a marked degree since that time, and, in the absence of precise data, I calculate that their number in 1875 exceeded that of the natives as follows:—

Total population	. . . .		230,000
Argentines	. . . . .	105,000	
Foreigners	. . . . .	125,000	
			230,000

But, in order to give an idea of this foreign mixture, and commence the study of the vital statistics upon a more solid basis, I shall here transcribe a page from the census returns of 1869, with all the details and particulars to which we may have occasion to refer.

Table showing the Population of the City of Buenos Ayres, in 1869, *Arranged according to Nationality, Age, and Sex.*

Nationality.	To 1 year		2 to 5		6 to 10		11 to 15		16 to 20		21 to 30	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Independent America:—												
Argentines	3,704	3,565	6,672	7,136	7,070	7,668	5,568	6,858	3,022	5,811	4,262	8,240
Bolivians	......	......	......	1	......	2	1	1	9	3	26	13
Brazilians	3	1	15	26	32	25	34	24	54	46	123	60
Chilians	......	......	1	1	5	4	9	13	46	30	96	32
North Americans	......	1	2	6	7	6	15	8	47	13	239	20
Uruguayans	48	25	150	129	266	221	414	325	620	449	1,003	770
Paraguayaus	1	2	15	11	43	37	68	25	64	17	117	33
Peruvians	1	......	......	......	......	......	2	......	4	3	13	2
From other parts of America	......	......	......	2	1	1	3	2	4	4	15	15
Europe:—												
Austrians	......	......	......	2	3	1	3	4	46	2	241	17
Germans	2	1	21	17	29	27	27	32	88	72	596	234
Belgiaus	......	......	2	1	3	......	4	2	4	8	38	16
Spaniards	15	16	87	90	151	158	471	305	1,604	496	3,706	1,035
French	26	12	97	109	168	172	302	242	919	585	2,810	1,524
English	9	2	28	20	45	46	71	67	192	132	809	368
Italians	82	75	559	524	954	818	1,881	1,026	2.670	1,629	9,490	4,063
Portuguese	1	1	2	......	5	4	6	3	39	4	274	20
Swiss	2	5	11	11	20	13	35	23	112	60	417	114
From other European countries	2	2	2	7	6	5	29	7	190	38	538	63
Africans	......	1	2	1	4	......	4	3	9	5	48	13
Asiatics	......	......	......	......	......	1	......	......	2	......	3	......
Not classified	......	2	......	......	......	......	2	1	8	......	6	......
Totals	3,896	3,711	7,666	8,094	8,812	9,209	8,949	8,971	9,753	9,407	24,870	16,652

Nationality.	31 to 40		41 to 50		51 to 60		61 to 70		71 to 80		81 to 90	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Independent America:—												
Argentines	2,909	5,543	2,043	3,712	1,343	2,089	612	987	223	425	43	110
Bolivians	11	2	4	......	8	......	1	3	......	2	......	......
Brazilians	93	37	54	23	32	15	13	2	2	......	......	3
Chilians	65	23	51	21	21	14	11	3	4	1	2	2
North Americans	128	11	57	7	21	2	8	3	2	......	......	......
Urnguayans	423	361	164	225	94	142	49	56	14	25	3	7
Paraguayans	78	11	32	7	12	10	3	3	4	......	......	......
Peruvians	7	6	4	9	7	2	3	3	2	......	......	......
From other parts of America	13	7	5	2	4	2	1	1	1	......	......	2
Europe:—												
Austrians	109	11	44	1	11	3	7	2	2	......	......	......
Germans	431	160	175	79	53	28	33	19	5	4	1	......
Belgians	39	14	17	4	5	2	4	......	......	......	......	......
Spaniards	2,478	718	1,237	410	496	179	167	76	59	25	12	4
Fronch	2,229	1,051	1,320	671	490	251	193	113	60	27	9	6
English	453	166	257	125	101	59	64	31	23	7	1	3
Italians	7,428	2,612	3,852	1,387	1,343	636	458	230	127	56	29	15
Portugueso	141	11	105	10	80	7	38	3	14	3	5	......
Swiss	262	72	118	36	33	12	13	2	6	2	1	......
From other European countries	298	62	146	26	46	29	13	6	9	4	2	......
Africans	34	14	39	24	40	39	27	45	31	40	20	24
Asiatics	2	......	3	2	1	1	1	1	......	3	......	1
Not classified	4	......	......	......	......	......	......	1	......	......	......	......
Totals	17,635	10,892	9,727	6,781	4,241	3,522	1,719	1,590	588	624	128	177

Nationality.	91 to 100		101 and upward		Age unknown		Recapitulation		
	M.	F.	M.	F.	M.	F.	M.	F.	Total.
Independent America:—									
Argentines	10	29	3	2	2	......	37,486	52,175	89,661
Bolivians	......	......	......	......	......	......	60	27	87
Brazilians	......	......	......	......	......	......	455	262	717
Chilians	......	......	1	......	......	......	312	144	456
North Americans	......	......	......	......	......	......	526	77	603
Uruguayans	......	2	1	1	......	......	3,249	2,738	5,987
Paraguayans	......	......	......	......	......	......	437	156	593
Peruvians	......	......	......	......	......	......	43	25	68
From other parts of America	......	......	......	......	......	......	47	38	85
Europe:—									
Austrians	......	......	......	......	33	......	499	43	542
Germans	......	......	......	......	......	......	1,461	578	2,039
Belgians	......	......	......	......	......	......	116	47	163
Spaniards	2	......	1	......	......	......	10,486	3,512	13,998
French	......	......	2	......	......	14	8,625	4,777	13,402
English	......	......	......	......	1	1	2,054	1,027	3,081
Italians	4	2	1	......	5	1	28,883	13,074	41,957
Portuguese	1	1	......	......	......	......	711	67	778
Swiss	......	......	......	......	......	......	1,030	350	1,380
From other European countries	2	......	2	......	33	......	1,315	347	1,662
Africans	2	7	3	3	......	......	263	219	482
Asiatics	......	......	......	......	......	......	12	9	21
Not classified	......	......	......	......	1	......	21	4	25
Totals	21	41	14	6	75	16	98,091	79,696	177,787

Attention is at once attracted by the numerical relation of the predominant elements in the native and foreign populations respectively. It should be borne in mind that, among immigrants to Buenos Ayres, the male element is far in excess of the female, and that for reasons as obvious as they are natural. The pioneers of emigration are always robust men in the prime of life; and not until prolonged experience has proved the certainty of happy results, do the women and families set out to share the fortunes of the fathers and brothers who have left their native homes. Immigration to the United States has now assumed a character of permanency and stability induced by satisfactory experiments extending over more than half a century; nor is it to be wondered at that immigrants arrive on the shores of the Union in a state of almost complete family organization, or, at least, that 45 per cent. of them are females of all ages. And it is a noteworthy fact that the distribution of female immigrants throughout the country is, according to the census of 1870, in direct ratio to the age of the States. In Maine, New York, and Massachusetts, for instance, the female foreign population is equal to, or often in excess of, the male; while in the new States and in the Territories, the number of females in the foreign population is small as compared to that of the males, save in the single Territory of Utah, where, for well-known reasons, the reverse is the rule.

The population of Buenos Ayres in 1869 comprised:—

Males of all ages and nationalities	98,091
Females " " "	79,696
Total	177,787

The Argentine branch of the population, of all ages, comprised, in the same year: males, 37,486; females, 52,175; or an excess of 14,689 females. In the foreign branch, including all ages and nationalities, there were: males, 60,605; females, 27,521; or an excess of 33,084 males, which not only compensated for the deficiency of males in the Argentine population, but constituted an excess of 18,395 males in the total number of inhabitants.

In regard to age, children under five years numbered 23,367, of whom 2290 had been born in foreign countries. In New York, the number of children of that age, so interesting in the study of vital statistics, is equal to 11.8 per cent. of the whole population, while in Buenos Ayres it constitutes only 13.1 per cent. From the age of five to fifteen, the proportion of foreign-born children gradually increases, which is likewise observed to be the case in New York, as shown by the report of the Commissioners of Emigration for 1875, in which children not over twelve years of age represented 21 per cent. of the 84,000 immigrants who arrived at that port in the year mentioned. The period ranging from sixteen to sixty years, is that in which the numerical superiority of the foreign population is most marked in Buenos Ayres; and the same phenomenon is observed in New York, though without the strange proportions characteristic of the former city. This is the period of life in which full physical and moral development are attained—plenitude of power for labor, for reproduction, and even for organic resistance of the permanent influences which menace health and existence. In this triple point of view, the foreign population is certainly superior wherever it presents, as it does in Buenos Ayres, such an evident majority of individuals in the age of vigor and strength.

Marriages.—The following table shows the number of marriages that took place in each of the seven years from 1867 to 1873. I have taken care to compare the numbers with the population in the respective years, calculating the latter according to the established rate of increase, in order to ascertain the number of persons per thousand married in each year:—

Years.	Marriages.	Number of persons married per 1000.
1867	1530	19.0
1868	1703	20.2
1869	1858	20.9
1870	1916	20.5
1871	1896	19.4
1872	2193	21.4
1873	2291	21.3
Mean number of persons married per 1000 during 7 years		20.39

From this table it appears that the number of marriages increased gradually, save in the year 1871, in which there was a decrease of 20 as compared with the figures of the year immediately preceding, due to the perturbation caused by the great epidemic. In the succeeding years the increase was again visible, as was also that of the population. It is likewise to be observed that the proportion per thousand varied very little, and may be set down at an average of 20, though the mean for the seven years was 20.39. Figures in statistics assume greater importance when compared with others of like nature. Here, as in other branches of the present report, I prefer to make the comparison with the city of New York, not only because I have ready access to the official documents relating to the vital statistics of that populous city, but also on account of a certain analogy which I find it to bear to Buenos Ayres in the department which now engages my attention. The number of marriages in New York in each of the same seven years, as given in the reports of vital statistics published by the Board of Health, and for which I am indebted to the courtesy of Dr. Nagle, will be seen in the subjoined table. I should add that, in computing the rate per 1000 of persons who

married, I have estimated the population before and after the census of 1870, according to the rate of increase—say 2.1 per cent.—adopted by the official authorities:—

Years.	Marriages.	Persons married per 1000.
1867	7144	16.14
1868	6926	15.33
1869	8695	18.44
1870	7985	14.71
1871	8646	18.07
1872	9008	18.52
1873	8887	17.72
Average		16.99

This mean annual rate may be regarded as somewhat below the truth for New York, inasmuch as the Registrar-General, in the course of his reports, complains of the culpable negligence of some of the persons required by law to have marriages registered in the proper office. At all events, the data I have adopted as the basis of my calculations are of official origin, and they show the increase in the number of marriages to be not uniform, as likewise the proportion per 1000. This variability or lack of uniformity I am at a loss to account for, nor am I aware to what causes, social or economical, it should be attributed. I shall add, by way of comparison, a list of the marriages in nine American cities in the year 1872, observing that the population of the city of New York has been increased in the proportion corresponding to two years after the census.

1872.

Cities.	Marriages.	Number per 1000 of persons married.
New York	9008	18.52
Boston	3762	28.38
Philadelphia	6496	19.28
Richmond	567	18.90
Providence	943	26.46
Pittsburg	1143	26.56
Albany	613	16.14
Newark	1241	23.62
Jersey City	677	15.74
Buenos Ayres	2193	21.04

In view of the heterogenous composition of the population, it is important in a statistical point of view to inquire into the nationality of the consorts. The results of observation in this respect are, in Buenos Ayres, as follows:—

Marriages according to the Nationality of the Consorts.

Year		Argentines.		Foreigners.
1869	Males	444	Males	1414
	Females	719	Females	1139
		1163		2553
1870	Males	519	Males	1397
	Females	770	Females	1146
		1289		2543
1871	Males	478	Males	1418
	Females	737	Females	1159
		1215		2577

1872	Males	485	Males	1708
	Females	722	Females	1471
		1207		3179
1873	Males	418	Males	1873
	Females	758	Females	1533
		1176		3406

In these five years, the number of foreigners who married is seen to exceed by far that of the Argentines. And if, for the sake of greater accuracy, we take the year 1869, bearing in mind that the foreign and the Argentine elements were then almost equal, the different nationality of the persons who married can only be explained by special comparison of the foreign element in relation to age and sex. Thus, the number of Argentines who contracted marriage in the year referred to was 1163, and that of the foreigners of both sexes, 2553—that is to say, about 2.19 of the latter to 1 of the former. It will also be noticed that the number of Argentine males was in still greater disproportion to that of the foreign males, namely 444 to 1414, or as 1 to 3.18. Still another comparison with one of the foreign nationalities: The Italian population is set down in the census at 41,957, against 89,661 Argentines. Now, the number of Italians who married in the course of that year reached 1304, and that of the Argentines 1163, of both sexes, while the Italian males amounted to 704, and the Argentines to only 444. A similar disproportion is noticed in New York in a series of years. Let us take, for example, the year 1873:—

Natives.		Foreigners.	
Males	2688	Males	6,183
Females	3530	Females	5,341
	6218		11,524

Here the natives stand in the proportion of 1 to 1.8 of both sexes, the proportion of native males being even smaller—1 to 2.3. The same observation may be made in New York with reference to the German nationality, as that I have just made with respect to the Italians in Buenos Ayres. The German population, according to the census, amounted to 151,216, the native having been 523,198. Meantime, while but 6218 native Americans of both sexes contracted matrimony, the number of Germans who married was 6340. And, in order to render the analogy still more marked, let us mention that the American males who entered into wedlock numbered only 2688, and the Germans 3416.

Births.—The same uniformity of progression appears in the births. It may also be stated with certainty that the number of births follows the increase of the population, preserving with it a uniform proportion, which proves that the registration has been carefully attended to, and that my estimate of the population before and after the census is neither excessive nor deficient. Although I have the registers from 1855 down to 1873, I only adopt in the following table those relating to the five years commencing with that of the census, and consequently including the unhappy year 1871:—

Years.	Population.	Births.	Rate per 1000.
1869	177,787	6944	39.3
1870	186,320	7561	40.5
1871	195,262	7549	38.6
1872	204,634	8078	39.4
1873	214,456	8559	39.9
Mean annual rate per 1000			39.5

Continuing the comparison with the vital movement in New York, I transcribe from the official documents the number of births during the same five years, in relation to the population calculated according to its annual growth:—

Years.	Births.	Rate per 1000.
1869	13,947	15.17
1870	14,524	15.41
1871	20,821	21.64
1872	22,068	22.45
1873	22,683	22.61
Mean annual rate per 1000		19.45

The different rates in the first two years as compared to the others may perhaps, in the absence of a visible specific cause, be attributed to a defective system for the registry of births. But, even taking the proportion of 22.61 per 1000, which is the maximum, it is far from the mean annual rate in Buenos Ayres. The accompanying table will show the proportion of births in a few cities of Europe and America, and will serve to demonstrate how variable that proportion is in different nations, though not on account of difference of race, but from other and complex causes which must, sooner or later, come within the range of sanitary science:—

Cities.	Births in 1873.	Rate per 1000.
Brussels	6,200	33.51
London	121,100	36.05
Bordeaux	5,036	25.95
Berlin	36,281	38.19
Dublin	9,031	28.70
Milan	9,091	33.52
Mexico	9,273	41.21
Boston	9,688	35.07
Brooklyn	5,027	11.54
Philadelphia	17,811	23.74
" (1874)	19,387	24.07
Chicago	9,718	24.29

In some American cities the proportion of births is so low, as observed by the Board of Health of Philadelphia in its reports, as only to be accounted for by defective registration. If such be the case, the necessity of reform is evident in this very important department of statistics.

The matrimonial statistics with respect to the nationality of the consorts, would seem to promise a like proportion in the births with respect to the nationality of the parents. I shall confine my examples in the following tables to births of children whose parents were either both Argentines or both foreigners, omitting cases of parents of mixed nationalities, and cases without any specification.

	Births.		
Years.	Parents both Argentines.	Parents both foreigners.	Proportion.
1869	1512	3314	1 to 2.0
1870	1403	3881	1 " 2.7
1871	1564	4019	1 " 2.4
1872	1580	4451	1 " 2.8
1873	1543	4738	1 " 3.0

The proportion of children of foreign parents to children of Argentine parents, increased from 2 to 1 in 1869 to 3 to 1 in 1873, it being worthy of remark that the augmentation coincided with the most extensive immigration. Here, again, I meet the same analogies as before between Buenos Ayres and New York, as set forth in the subjoined table, in which

are included only births of children of American parents and children of foreign parents.

Years.	Births. American parents.	Births. Foreign parents.	Proportion.
1869	2457	9,080	1 to 3.7
1870	2553	9,282	1 " 3.6
1871	2631	14,144	1 " 3.8
1872	3721	14,829	1 " 3.9
1873	3827	15,353	1 " 4.0

In New York, the excess of births of children of foreign parents, as compared with those of children of American parents, is more remarkable, inasmuch as it reached the proportion of 4 to 1 in 1873. This extraordinary proportion is far in excess of that suggested by the number of marriages according to nationalities, and indicates that the fecundity of these different classes of persons is affected by other causes, as proved by the two following facts: (1) That the predominance of males over females is greater among children of foreign than among those of American parents, and (2) That, according to the curious observations contained in the tables of statistics published by the New York Board of Health from 1870 to 1873, among American mothers whose children were registered in those years,

35 had had up to that time	12 children each.
17 " " " "	13 " "
9 " " " "	14 " "
5 " " " "	15 " "
2 " " " "	16 " "
1 " " " "	18 " "
69	

And among foreign mothers there were counted

215 with	12 children each.
119 "	13 " "
57 "	14 " "
17 "	15 " "
12 "	16 " "
11 "	17 " "
4 "	18 " "
1 "	19 " "
436	

making a total of 436 fecund foreign mothers, and only 69 American, the fecundity of the latter appearing, too, in a less degree than that of the former.

Mortality.—I have now reached the most painful portion of my review. In regard to the sanitary condition of Buenos Ayres, I must state facts plainly as I apprehend them to exist, not merely because science should be in possession of the whole truth, but also because the truth may serve a useful end in stimulating the praiseworthy efforts now being made to remedy the evils which menace the public health in that city.

Annual reports of the vital statistics of Buenos Ayres have only been published within the last few years, and always so late as to have lost much of their interest when they have reached the eyes of the few persons who consult them. The public is for the most part ignorant of the number of deaths that have occurred during the year, and is altogether unaware of the proportion existing between the mortality and the population, so that the belief still prevails that the city is very healthy. From time to time such epidemics have appeared as smallpox, scarlatina, or

measles, often carrying away large numbers of people; but such visitations have been regarded as temporary exacerbations of mortality, which, once past, would give place to returning health and all its attendant blessings. In 1858, a yellow-fever epidemic, confined within the narrow limits of a single ward of the city, killed from three to four hundred victims. As the same disease had raged to a fearful extent the previous year at the neighboring city of Montevideo, its appearance in Buenos Ayres produced a profound impression of terror, and caused a large number of the inhabitants to flee to the rural districts; but the epidemic having proved of short duration, and its effects somewhat limited, it was believed that the sanitary condition of the city was so perfect as to prevent the spreading of the horrible malady, and a general complaint arose against the quarantine department for want of vigilance in allowing the introduction of an exotic disease. In spite of the frequent returns of smallpox, scarlatina, and typhoid fever, and the mortality caused by the *tetanus infantum* (popularly called the "seven days' sickness"), and other diseases common among children, the idea of the salubrity of the city still prevailed in the public mind.

In 1867, Asiatic cholera made its first appearance, and the following year it returned, extending its ravages through the surrounding country and most of the provinces of the interior. Much greater, indeed, would have been the alarm in Buenos Ayres, if timely publicity had been given to the disastrous effects of the warnings from Nature, who in no instance allows any violation of her laws to pass with impunity. Much greater, I say, would have been the alarm, for the people would then have been made aware that the number of deaths in 1867 had reached 8029, or 49.9 per 1000 of the entire population, and 38.9 per 1000 in 1868. As it was, however, the evil was productive of some good results: works were undertaken for supplying the city with running water, and the cleaning of the streets, and other sanitary details coming within the province of the municipal police, were more regularly and efficiently performed.

The year 1869 was a peculiarly favorable one for determining the real sanitary condition of the city, there being no epidemic just then in Buenos Ayres, and the general health being, to all appearance, perfectly satisfactory. The census of the population was taken in the year just mentioned, and that was the juncture fixed upon for inquiring into the real nature of the city as a habitable centre, and examining the status of its account current between life and death, always bearing in mind that, after the disappearance of severe epidemics, the relative death-rate decreases in a sensible degree. Now, had a report of the vital statistics then been drawn up and published, it would have exhibited a mortality of 5982 for the year last referred to, or 33 per 1000 of the inhabitants who had just been numbered, and would have shown that, with such a usual rate, Buenos Ayres could not appear to advantage beside other civilized cities, in which latter—even including the most populous of Europe and America—the mean annual death-rate is, with very few and marked exceptions, much lower. Then would the existence have become evident of permanent causes, the removal of which was indispensable in order to the improvement of the public health. It would also have been discovered that those causes tended to heighten the severity of epidemics, and that something more was necessary than merely defending the entrance to the city against the invasion of pestilence (even could such defence, which is rarely the case, be rendered efficacious), namely, the purification of the city itself, in order to render it salubrious under all

circumstances, and to weaken the virulence of disease in the event of extraordinary visitations.

In 1870, peace and prosperity reigned undisturbed in the city, the general health seemed good, immigrants, in hitherto unprecedented numbers, were flocking in, labor was abundant and well remunerated, and capital easily obtained, and commerce and industry were more active than ever before. Buenos Ayres was all contentment, and looked with evident satisfaction upon its visible growth; and the very possibility of another epidemic was forgotten by the people, who supposed the entrance to their city well guarded by vigilant sentinels. Indeed, the death-rate for that year was 1.5 per 1000 lower than that of the year immediately preceding, though still far above the usual rate in other cities between which and Buenos Ayres comparison was allowable. The situation remained unchanged until the early days of 1871, when some cases of yellow fever were reported in the southern extremity of the town. How had the enemy entered, and who was to blame for the neglect? Investigation was idle; the yellow fever, the terrible yellow fever, was in their midst, and the frightened inhabitants anxiously occupied themselves with the thought: what is likely to be the severity, and what the extent, of the visitation? All, families and individuals, who could do so, left the city in search of a refuge from death that stared them in the face. The pestilence, in the mean time, spread apace, and gained in intensity as it spread. It attained its maximum severity in April, and thenceforward gradually subsided until the end of May or beginning of June, when the last cases occurred. The epidemic had extended throughout the entire city. Its ravages were truly awful; 106.5 of every 1000 inhabitants died in that year, including in the population those who were saved by withdrawing to the rural districts—some 60,000 persons. Such mortality was beyond all imagination: one out of every nine inhabitants is a death-rate unprecedented in the civilized world in the nineteenth century; nor is it possible to describe the feelings of anguish and terror which it left in the breasts of the survivors. It then became deplorably evident that the hygienic condition of Buenos Ayres was unfavorable in the extreme, and that the prompt investigation and removal of the causes of the evil, at any sacrifice, were matters of the utmost urgency. Under the promptings and counsel of science and experience, works of sanitary improvement were at once commenced, and on their completion we shall be in possession of that salubrity which is so much to be desired, and which is always the reward of man's efforts to secure it. The lesson has been a severe one.

It is only within the past few years that the beneficial results of Sanitary Sciences have been experienced, even in Europe itself. To know that many diseases are avoidable, inasmuch as their determining causes are known and may be suppressed, is the first step, and comes within the province of science. The will and the means necessary for the removal of those causes are to be applied by the people, through their municipal or political organization. The world has been very tardy in learning to know itself in this respect, and is still slow in reaching the ultimate consequences. It is not, therefore, to be wondered at that Buenos Ayres should have been so ignorant, and still more neglectful than ignorant, of the interests of the public health. Its very rapid growth warrants the presumption that its sociological evolution may possibly partake of a somewhat tumultuous character. Political agitations on the one hand, apt on occasions to assume convulsive forms; inward satisfaction at

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conscious advancement on the other, with the accumulation of stimuli developed in a feverish and progressive society, and the complete concentration of power, individual and collective, in pursuit of the more material and tangible ends of human energy, have been so many influences tending to divert, and which have in reality diverted, the minds of men from those other interests, more radical and enduring if we will, but less peremptory than the first, because their sphere is not measured by common vision, and because a catastrophe is at times required to bring them to the public attention.

Although the foregoing reflections do not strictly form a part of the principal object of the present review, they bear so close a relation to it that I cannot forbear including them, they having been suggested to me in the course of my statistical inquiries. It also appears to me proper to mention, in this place, the chief causes of the increased mortality, though they differ in no respect from those which—in all cities where the laws of hygiene are imperfectly observed—contribute to augment the number of deaths.

In Buenos Ayres, as in all Spanish towns, the streets are narrow, and they present, together with the few, small, public squares, an area of very inconsiderable extent. It is to be remembered that the proportion of the municipal area to each inhabitant, as mentioned in a preceding page —70 metres—is the average proportion, there being many parts of the city sparsely populated; and it is likewise to be borne in mind that in most houses considerable space is devoted to extensive court-yards, etc. As the population increases, these relative advantages grow less, and they will become altogether null at no far distant day, unless prudent forethought lead to the enlarging of the squares, and the converting them into health-giving parks, and the widening some of the streets into vast and spacious avenues.

In the course of the past twenty-four years, about two-thirds of the city have been rebuilt, but without any attempt having been made to improve that opportunity by adopting a methodical system of widening, save only in the new streets of the suburbs. A grave error was committed in using the filth of the town for the purpose of filling up and levelling some of the streets, which were immediately paved over. The filth thus employed consisted of a heterogeneous mass principally made up of the refuse from the dwellings, that is to say, animal and vegetable matter united with dust and other substances, comprising house and street sweepings. Such a sediment is destined to be decomposed by putrid fermentation, and gives place to the generation of mephitic gases, which escape through the porous surface stratum, and, mingling with the air breathed by the inhabitants, constitute an inexhaustible source of poison for the atmosphere. Indeed, it has been observed that the people dwelling in the wards here described, have been relatively the principal sufferers during epidemics, and that some portions of these districts are rarely exempt from typhoid fever, especially in the spring and summer seasons.

The water formerly used in Buenos Ayres, both for drinking and general household purposes, was derived from three sources; the rich had rain-water, preserved in impermeable tanks or cisterns, and the remainder of the population took well-water and the river-water sold in the streets, which was commonly brought from the portion of the river La Plata contiguous to the town, and was certainly contaminated by the

fluvial population of the port, and by the liquid animal matter flowing down from the slaughter-houses situated on the borders of the Riachuelo, some two miles south of the centre of the city. With the exception of the cistern water, for the most part good, the water chiefly used was necessarily bad, as may be presumed from the nature of the sources from which it was derived. Since 1868, steps have been taken for supplying the town with running water proceeding from a more suitable portion of the river, towards the north of the city; but the supply thus obtained is extremely limited, nor is the source from which it is taken altogether free from objection yet. The extensive works at present in process of execution will accomplish a radical improvement in this respect.

The system of privies was primitive in the extreme. The receptacles of the excrementitious matters were common cesspools, usually sunk below the level of the subterranean water. In the soil, eminently porous in that region, a deposit of putrescible matter was maintained for years, dissolved more and more by the subterranean water, whose level has a mean alternate rise and fall of about six feet from the dry to the wet season. Hence, the process of absorption was rapid; and not only the gases generated by fermentation, but the liquids in which the decomposed organic matter was held in solution impregnated the soil, vitiated the water of the wells, and sent to the surface mephitic emanations incompatible with the good health of the people who breathed an atmosphere thus poisoned. In 1868 a new system of impermeable privies was instituted; but it has not yet become sufficiently general in its application, and since 1871 the plan has been put into practice of emptying and cleaning the privies by a pneumatic mechanism similar to that so successfully applied in France. The works of drainage and sewerage, so actively prosecuted at the present time, are destined to radically remove this infection, which experience has demonstrated to be one of the most fruitful causes of disease and death.

Before closing this tedious enumeration, I must add that there existed two other foci of infection in Buenos Ayres. One of these was an immense heap of refuse, made up of animal and vegetable matter in full course of decomposition, which was accumulated some three miles southwest from the centre of the city, and from which the noxious gases were carried to the town by the atmospheric currents, and mainly by the wind called *el pampero*, blowing in that direction from the pampas, and reputed as the most healthful in the region. The quantity of filth gathered in that place was enormous; in 1873, three hundred tons of the pestiferous mixture were carried off daily, and in 1875, three hundred and ninety tons. Since 1873, the evil effects of the accumulation have been sensibly modified by the simple and most efficient means adopted of burning the unclean matter.

Two miles southward from the centre of the town runs the Riachuelo, which empties into the Rio de la Plata. Here the loading and unloading of the coasting and other light craft are mainly effected; and on the banks of the Riachuelo stood for many years the slaughter-houses and fat-boiling establishments, which represented the two principal industries of the province, and in which as many as half a million of cattle were slaughtered annually, and more than two millions of sheep and mares, and the various parts of their carcasses were prepared for commerce. All the liquid and much of the solid refuse matter proceeding from these establishments were thrown into the Riachuelo. The mass of infection thus

collected may easily be imagined, as may the consequent contamination of the waters by the fermentation of the organic matter, from which arose stifling gases to corrupt the air shortly to be breathed by the inhabitants of the town. So extensive were the interests connected with the ancient and remunerative industry allowed by our legislators to be carried on in the manner described, that, notwithstanding oft-repeated warnings of the grave objections offered by the nuisance, all the terror and desolation of 1871 were necessary to bring about its abrupt suppression. Many proprietors, no doubt, suffered materially by the measure; but the beneficial results to the public health were incalculable. The diminution of the typhoid fever since the removal of the *saladeros*, etc., affords an evident proof of the wisdom of the step.

Is it, then, astonishing that, under so many unfavorable circumstances, mortality should have been so great in Buenos Ayres? If the people had been informed of the number of deaths from year to year, and the relation between that number and the number of inhabitants, they would have seen that, during the years 1854–1872, the death-rate was never less than 31 per 1000, without counting the great epidemics; they would have understood that the severity of the cholera and yellow fever which decimated their ranks was intimately connected with causes within their midst, calculated to encourage the development of contagious maladies; and they would have investigated the nature of those causes, would have found them where all modern societies have found them, and would have energetically set about their removal. They would thus have avoided much grief and many losses of greater moment than the expenditure would have involved. There is no doubt that, if the public health be persistently and intelligently cared for, and past and future experience in this respect be profited by, the rate of mortality will in a few years descend to the level of that of London, the metropolis of the world, as it is called on account of its gigantic proportions. The climate of London is not better than that of Buenos Ayres; nor are the waters of the Lea or the Thames comparable, either for purity or abundance, to the sweet and crystalline streams of the Paraná and Uruguay, which unite to form the Plata; nor is the food used in England more healthy or nutritious than that used in the Platine city; nor is the soil of the former more extensive or more fertile than that which the latter can offer to the present and to future generations. If, in our endeavors to improve our own condition, we use the same enlightened determination which England uses, and which secures for her the respect and admiration of all beholders, we shall accomplish what she has accomplished, namely, the reduction of the death-rate in her large cities from 50 per 1000, near the close of the last century, to 22 per 1000, which has been the mean rate during the last few years.

Let us now see what statistics say in their austere language. Taking a series of fourteen years, from 1861 to 1875, exclusive of 1874, for which I have as yet no returns, but inclusive of 1867 and 1868 with their great cholera epidemic, and 1871 with its yellow fever, the mean annual mortality is found to be 38.9 per 1000. If we leave out the years marked by epidemics, the mean annual rate of the eleven remaining years will be 31.3. But I prefer to present these data in tabular form, for the sake of clearness, and in order that the maximum and minimum mortality in the years referred to may be readily perceived.

Years.	Estimated population.	Mortality.	Rate per 1000.
1861	121,280	3,410	28.1
1862	127,101	4,313	33.8
1863	133,200	4,539	34.0
1864	139,593	4,378	31.3
1865	146,292	5,857[1]	40.0
1866	153,313	5,111	33.3
1867	160,671	8,029	49.9
1868	168,382	6,564	38.9
1869	177,787	5,982	33.6
1870	186,320	5,886	31.5
1871	195,262	20,748	106.2
1872	204,634	5,671	27.7
1873	214,453	5,891	27.4
1875	230,000	6,751	29.3
Mean annual rate			38.9
Mean annual rate, leaving out the years marked by epidemics			31.3

In the foregoing table are included three years of dreadful epidemics. It is beyond doubt that, although the importation of contagious maladies is almost always accidental, the extent to which they spread and the severity of their character are always to be imputed to the sanitary condition of the population, and to the same account is the increased mortality to be charged. The year 1865 likewise figures in the table with a death-rate of 40 per 1000, owing to the fact that the soldiers wounded in the first battles of the Paraguayan war, fought on Argentine territory invaded by the enemy, were brought to Buenos Ayres, and considerably increased the number of deaths registered in the city. Deaths occasioned by war should also appear in vital statistics: they are losses of life; and, sooth to say, the cause to which they are due is among the most easily avoidable. With these observations, the mean annual rate for the period we have chosen may readily be estimated. If we leave out the epidemic years and the first year of the Paraguayan war, the mean annual rate of the ten remaining years will be 31 per 1000.

Let us see the state of things in New York during the same period. As will be observed in the subjoined table, I have adopted the figures of the census returns of 1860 and 1870, and computed the population of the intervening years by applying the mean annual rate of increase corresponding to 1.5 per centum. For the years following 1870, I have, for reasons already hinted in this paper, adopted 2.1 per cent., the mean annual rate of increase admitted by the Board of Health. The State census of 1865 has always been regarded as too low; and such must be the case, for, by comparing the population as set down in that census with the mortality for that year, the death-rate would be 35.4, which is inadmissible, as being out of proportion with the rates of the previous and succeeding years.

[1] Commencement of the Paraguayan war.

Years.	Population.	Deaths.	Rate per 1000.
1861	825,873	24,525	28.4
1862	838,260	23,150	27.6
1863	850,833	26,617	31.2
1864	863,625	25,792	29.9
1865	876,579	25,767	29.3
1866	889,726	26,815	30.1
1867	903,071	23,159	25.6
1868	916,641	24,889	27.1
1869	930,337	25,167	27.0
1870	942.292	27,175	28.8
1871	962,079	26.976	28.0
1872	982,282	32,647	33.2
1873	1,002,909	29,084	28.9
1874	1,023,969	28,727	28.0
1875	1,045,467	30,709	29.3
Mean annual rate of mortality per 1000			28.8

In comparing the two tables, I have only to observe that the death-rate for the years 1872 and 1873, in Buenos Ayres, is lower than that for the same years and the year 1874 in New York, while that for 1875 is precisely the same in both cities; that the number of still-born infants is included in the total of deaths in Buenos Ayres, but not in that of the deaths in New York, according to the usual custom in the United States and England; and finally, that, of the total number of deaths figuring opposite the year 1875 in the Buenos Ayres table, 1041 were caused by smallpox.

Comparative Table of the Rate of Mortality in some American and European Cities in the Year 1873.

Cities.	Rate.	Cities.	Rate.
New York	28.9	London	22.8
Philadelphia	19.6	Liverpool	25.8
Chicago	23.8	Edinburgh	21.9
Boston	28.4	Paris	23.0
Cincinnati	22.8	Bordeaux	26.7
Buffalo	13.7	Berlin	27.8
St. Louis	19.4	Vienna	35.2
New Orleans	37.5	Valparaiso	50.0
Cleveland	19.2	Buenos Ayres	27.4

The statistics of mortality in Buenos Ayres are taken with commendable numerical accuracy; but that alone is not sufficient when those statistics are to form a part of the general vital statistics, for in the tables of these a methodical and scientific classification is required. The classification in Buenos Ayres is defective. The deaths are registered according to the medical certificates, in which is expressed the cause of death; but the diagnosis is not submitted to the criterion of any competent authority; nor are any fixed rules observed in the preparation of the tables. The municipal employés, in their turn, but so long after date as to preclude the possibility of adequate revision and correction, copy down in alphabetical order the diseases mentioned in the physicians' certificates; so that statistics thus compiled leave much to be wished for, in a scientific point of view. The establishment of a uniform system of classification for all countries would be desirable, as much enhancing the advantages to be derived from comparative statistical studies. The classification proposed by Dr. Farr, and approved by the International Statistical Congress in Paris, in 1855, is the one commonly followed in the United States and England; but it has not been adopted in France and

many other nations, nor is it uniformly observed even in the United States. In the reports of the Chicago Board of Health, which I have before me, I find the diseases arranged in alphabetical order, without any regard to their special character.

The publication of the complete statistics of Buenos Ayres would, therefore, be of no practical interest; and so I shall select the latest of a series of reports in my possession—that for 1875—and transcribe therefrom such portions as are necessary. Of the 6751 deaths registered during that year, the prevailing causes were as follows:—

Smallpox	1041
Phthisis	858
Tetanus infantum	445
Typhoid fever	140
Meningitis	355
Pneumonia	382
Organic affections of the circulatory system	389
Diphtheria	101
Diarrhœa	296

The mortality according to sexes was as follows:—

Males	3841
Females	2889
Not specified	21
Total	6751

According to ages:—

Up to 5 years (including 189 still-born)	3521
Up to 100 years	3072
Not specified	158
Total	6751

According to nationalities:—

Argentines	5102
Foreigners	1649
Total	6751

Smallpox stands for 15.4 per cent. of the mortality of the year. Few cities present so large a number; and this is precisely the disease the ravages of which may be most effectually prevented. If vaccination were made obligatory in all cases and by all means, and due care taken to be sure of the efficacy of the virus by frequent renewal, the deaths from smallpox might be reduced to one-tenth of the number figuring in the table; particularly when the hygienic condition of the city should have been improved by the overground and underground sanitary works now in progress. The appearance of smallpox is no longer an accident, but an event of ordinary occurrence; the extent of its ravages is variable, but the disease never fails to come, as proved by the following table:—

In 1869 the number of deaths by smallpox was	183
" 1870 " " " "	195
" 1871 " " " "	1656
" 1872 " " " "	836
" 1873 " " " "	76
" 1874 " " " "	525
" 1875 " " " "	1041
Total	4512

Giving a mean annual rate of 644 during the seven years mentioned. Such figures as these need no comment, nor even comparison with those of American and European cities or regions.

The next item of importance is that of 858 deaths from pulmonary consumption, representing 12.7 per cent. of the total mortality. The death-rate caused by phthisis has sensibly augmented in six years, as will be seen in the following table:—

Years.	Deaths from phthisis.	Rate per cent. of the total mortality.
1869	370	6.1
1870	274	4.6
1871	495	2.4[1]
1872	597	10.5
1873	755	12.8
1875	858	12.7

This progressive increase of tuberculosis, at a time when cases of zymotic disease are tending to diminish in number, save only smallpox, which is due to a specific cause, can only be accounted for by an increase of dampness in the soil underlying the city, in combination with the other causes of insalubrity already mentioned, and which have as yet been but slightly modified. Dr. Buchanan observed the effects of the drainage-works in twenty-five cities in England, and reported that in all of them the general mortality had decreased, particularly that determined by typhoid fever and diarrhœa; but that in those which had been supplied with thorough sewerage, without an equally effective system of underground drainage, neither pulmonary consumption nor catarrhal affections had undergone any diminution. The immediate effect of subterranean drainage is to dry the ground, thus allowing the air to penetrate into the latter and take the place of the water throughout the thickness of dried earth, and there hasten the oxygenation of the infectious substances held in solution, and thereby establish a healthy condition of the soil. Besides, this drying process exercises a powerful influence upon the atmospheric strata immediately contiguous to the ground and forming the air breathed by the inhabitants. It has been observed as an invariable rule, easily explained by physical laws, that fogs disappear or are diminished in a remarkable degree in cities and elsewhere where the ground has been dried by drainage. From the two foregoing facts it may be deduced that the increase of tuberculosis corresponds to the increase of underground humidity in conjunction with other depressing influences; and that if we desire to put a limit to this grave malady, all the more to be feared from its hereditary character, it is necessary that a most perfect system of underground drainage should form a part of the works now in process of execution, not only to prevent the invasion of new masses of water, but to dry up that now contained in the ground, and so remove the existing infection.

Buenos Ayres is not, indeed, the city which offers the greatest mortality from pulmonary consumption. This disease has always been proportionately more destructive in New York. The statistics of fifty-five years demonstrate that, down to 1853, it caused from 16 to 21 per cent. of the total mortality of that city; but that since that time it has diminished in severity, as may be seen by the subjoined table, corresponding to that above given for Buenos Ayres:—

[1] The year of the great yellow-fever epidemic.

Years.	Deaths from phthisis.	Rate per cent. of the total mortality.
1869	3364	13.37
1870	4030	14.82
1871	4186	15.52
1872	4274	13.09
1873	4134	14.21
1874	4034	14.04
1875	4172	13.78

Nor is New York the least favored city in this respect in the United States or Europe, as may be seen from the following table:—

Cities.	Rate per cent. of deaths from phthisis.	Cities.	Rate per cent. of deaths from phthisis.
Boston	15.17	St. Louis	8.78
Philadelphia	14.28	Baltimore	14.55
Portland	25.03	London	11.70
Cincinnati	11.64	Paris	18.90
Cleveland	7.76	Rome	11.00
Chicago	6.68	Naples	15.24

Next in order of numerical importance, on our list of causes of mortality, stands *tetanus infantum* or *trismus nascentium*, the number of victims to which was 445 in the year referred to, or 6.5 per cent. of the total mortality. The peculiar tendency of the soil of Buenos Ayres to determine such a considerable number of cases of *tetanus infantum*, vulgarly called the "seven days' sickness," and in England the "nine days' sickness," is a circumstance deserving experimental and scientific inquiry. This species of tetanus is regarded as traumatic, and as proceeding from the section of the umbilical cord: it may set in during the process of cicatrization. That this is not the only determining cause, is proved by the existence of the disease in the endemic form in the city and province of Buenos Ayres, and, to a certain degree, in the other littoral provinces. That it is dependent upon the impurity of the surrounding atmosphere and the want of care bestowed upon new-born infants, is demonstrated by its almost total disappearance among families comfortably situated in clean and well-ventilated houses; while the chief ravages of the affection occur among the poor, particularly in houses overcrowded with people who either know not how, or are unable, to give their infants that care which is required by their tender years. At all events, the mortality from *tetanus infantum* has been on the decrease for a number of years, owing to the improved condition of the general hygienic police, for I have no doubt of the existence of a zymotic element in the production of the disease.

Years.	Deaths from tetanus infantum.	Rate per cent. of the total mortality.
1869	630	10.5
1870	689	11.7
1871	470	2.2[1]
1872	558	9.8
1873	620	10.5
1875	445	6.5

It is not here possible to establish a comparison with other cities, the tetanus of new-born infants either not being mentioned at all in their statistical reports, or only figuring therein for an extremely small proportion of the total mortality. One exception I should, however, make:

[1] Year of the yellow fever.

the city of Havana, where 388 of the 8390 deaths which occurred in 1875, were caused by *tetanus infantum*, being 4.6 per cent. of the whole mortality.

Typhoid fever was represented in 1875 by 128 deaths. The number of deaths by this disease was 600 in 1869; but it has been gradually decreasing since the suppression of the *saladeros* in 1871, and the destruction of the filth in 1872 and 1873. This, like the other zymotic diseases, is subordinate to the infection of the soil and air, and will prevail in inverse ratio to the progress of the sanitary works now in course of execution.

I shall devote no special remarks to the other causes of death enumerated in the table, partly because they are common diseases, and again because the sanitary improvements will exercise a certain influence upon them. After a brief examination of the subject of mortality among children in Buenos Ayres, I shall proceed to consider the relation in which the various nationalities appear before the law of death.

The number of children under five years of age who died in 1875, was 3521, including still-born infants, or 52.1 per cent. of the total mortality for the twelvemonth. More than one-half of the deaths in Buenos Ayres are of children under the age of five, which proportion is at once extremely large and afflicting, especially as it seems to increase from year to year, as set forth in the annexed table:—

Years.	No. of deaths among children.	Rate per cent. of the total mortality.
1869	2534	42.2
1870	2690	45.7
1871	3591	17.3
1872	2649	45.7
1873	2891	49.0
1875	3521	52.1

Let us compare this table with the following one for New York during the same number of years:—

Years.	No. of deaths among children.	Rate per cent. of the total mortality.
1870	12,971	48.0
1871	13,333	49.0
1872	16,188	49.5
1873	14,182	48.7
1874	13,956	48.9
1875	14,839	48.3

From the comparison it would appear that the relative mortality among children was greater in New York than in Buenos Ayres. But in both cities it is very much greater than in localities the hygienic condition of which is more favorable to general health; while, on the contrary, wherever that general condition is less favorable, infant mortality represents a still higher proportion.

Where a full-grown adult may be able to brave morbific influences with impunity, a tender infant will succumb. The mephitism of cities, resulting at once from accumulation, imperfect circulation of air, defective light, and the gases from decayed organic matter which pervade the atmosphere, menaces the health of all; notwithstanding which, the great majority of adults survives, thanks to fully developed vigor and a confirmed habit of organism, inured, as it were, to these adverse circumstances. But the infant, just emerged from the maternal cloister, ignorant of the circumstances in the midst of which it lives, unaccustomed to the struggle, breathing that impure air from twenty-six to thirty

times each minute, with extremely permeable membranes, and the rapid processes of assimilation required by the progressive development of its organs; the infant, in a word, feeble, inert, and defencelessly exposed to the poisonous air, without knowledge of the evil, and even helpless to avail itself of its locomotory mechanism to enable it to avoid danger under the guidance of instinct, droops, and is prostrated by disease and death. And all this without taking into account the baleful effects of hereditary transmission, by which those tender organisms are unfitted for life, poisoned as they are before birth by virulent infections, which at best doom to a miserable existence the beings who receive them with the blood of their progenitors.

These simple reflections, dictated by common sense, serve to illustrate the incontestable fact that, under whatever hygienic conditions, even the most satisfactory, life is surrounded by most dangers when in its dawn, and that deaths will be more numerons during that period than at any other time with which it may be compared; but the same reflections serve also to illustrate another phenomenon less generally known, namely, that, in proportion to the development of toxic influences in a community, not only will the general mortality be enhanced, but the relative mortality among children will be increased in a still higher degree. This unchanging fact, proved by statistics and never contradicted by experience, points to the conclusion that the proportion of the infant mortality to the total mortality in each community may be taken as a reliable human necrometer, and that from it may be deduced the sanitary condition of that community, even without an absolute knowledge of the population at a given moment.

Supposing, for instance, that New York should succeed in reducing its mortality from 29.3 per 1000, as it was in 1875, to the London rate of 21 per 1000 (which, it seems to me, would be perfectly attainable in a few years, if the remedies pointed out by science were resolutely applied), the aggregate mortality would be 21,954, instead of 30,709, and the infant mortality, which reached 14,839, would not only be reduced, in proportion to the total, to 10,604, but (the rate per cent., instead of 48.3, being only 40, as it is approximately in very healthy cities) would not rise above 8781, thereby saving more than six thousand children annually who are now sacrificed by the fatal circumstances under which they are born to await death. And, if the same reckoning were applied to Buenos Ayres, the mortality in 1875 would have been 4830, instead of 6751, and the corresponding infant mortality would have reached but 1932, and 1589 infant lives would have been spared.

The vital statistics of Buenos Ayres show that, of 6751 who died in that city in 1875, 5102 were Argentines, or 75.5 per cent. of the total number who perished. With the exception of 1871, in which year the number of foreigners who died was greater than that of natives, the latter stand in the death-returns for from 70 to 75 per cent. Indeed, the exception mentioned is to be explained by the fact that 60,000 persons, almost exclusively Argentines, fled and took refuge in the rural districts, while the foreigners (workingmen with their families) remained, for the most part, in the infected town, and bore the brunt of the pestilence. For the sake of clearness, I give the subjoined table, in which are set forth the mortality among Argentines and foreigners, and the death-rate of the former in relation to the total mortality in the city during a period of six years:—

Years.	Argentines.	Foreigners.	Percentage of Argentine deaths.
1869	4203	1,779	70.2
1870	4587	1,299	77.9
1871	8082	12,666	38.9
1872	4121	1,550	72.6
1873	4319	1,572	73.3
1875	5102	1,649	75.5

If we examine the figures corresponding to 1869, which, taken as they are from the census-returns of that year, present the advantage of being accurate, and bear in mind that the native and foreign elements were then almost equal (89,661 Argentines, 88,126 foreigners), we shall observe that the mortality among the former was at the rate of 46.9 per 1000, and that among the latter 20.1. And, if we take into consideration that the proportion of the foreign to the native element has increased from year to year, the marked inequality in the relative proportions of mortality in the two groups will become at once perceptible. The gravity of such an alarming state of affairs as shown by statistics is somewhat attenuated by the fact that, among the number of deaths of Argentines, is included the total mortality of children up to five years of age. Deducting the latter from the death-returns of the same year, 1869, which, as has been seen, is likewise the least unfavorable for the Argentines, and counting as adults (in as far as probabilities of life are concerned) all individuals of upward of five years of age, we shall have:—

Argentines	4203
Children	2534
	1669
Foreigners	1779

that is is to say, a slightly greater mortality among foreign adults. It is, however, to be observed that, of the 23,367 children (up to 5 years) shown by the census, 2290 were born out of the republic. The portion of these among the children who died in that year, and who were naturally registered acording to their respective nationalities, forms a part of the 1779 foreigners of the death-returns, and hence cannot be deducted from the total of the Argentine infant mortality. If 9.8 per cent. of the infant population, up to five years, is made up of foreign children, it is fair to suppose that at least 9 per cent. of the infant mortality belonged to that category, in whicn case the approximate result would be:—

Total of deaths among children	2534
Nine per cent. of foreign children	228
Number of Argentine children	2306

which deducted from the total Argentine mortality, would leave, instead of 1669, a remainder of 1897, a number much higher than that of the deaths among foreigners.

Before proceeding any further with the review of the sad details suggested by the foregoing results, let us compare these with those of the corresponding table for New York, as follows:—

Years.	Mortality.		Percentage of Americans.
	Americans.	Foreigners.	
1868	16,805	8,084	67.5
1870	17,471	9,714	64.2
1871	17,470	9,566	64.7
1872	21,448	11,199	65.6
1873	19,135	9,949	65.7

Taking the year of the census, in which the population was composed of 523,198 Americans and 419,094 foreigners, the mortality among the former was 33.3 per 1000, and among the latter 23.1 per 1000. If the children be also deducted, for the same reason as in the case of Buenos Ayres, the balance of the adult mortality is in favor of the native population, in the proportion of 4671 deaths of adult Americans and 9714 of foreigners, regarding as adults, as usual in my calculations, all the inhabitants of more than five years of age. It is necessary here to make two observations concerning these comparative results. The first has reference to the difference as regards ages, between Buenos Ayres and New York, whence it appears that the inhabitants under five years of age constitute, in the first city, 13.1 per cent. of the total population, as may be seen by the census returns, while in the second they represent but 11.8 per cent., and, consequently, that the mass of inhabitants among whom death makes its most abundant harvest is relatively larger in Buenos Ayres. The second is contained in the following words of Dr. Elisha Harris, recorder of vital statistics of the New York Board of Health: "We next notice that more than half of the total population which is over five years of age is of foreign birth, and that families, the heads of which are of foreign birth, constitute the chief portion of the tenement population, as shown by the returns of census-takers in the different wards." . . . "Vigorous as the better portion of foreign residents are, the rate of mortality in their children is excessive." These statements go to show that the mortality among foreigners of over five years of age, who live under such depressing circumstances, as also among their young children, must be greater than that among the native population, by whom other advantages are enjoyed.

There is in all this something more radical and serious than mere numbers. In order to attenuate the gravity of the vital statistics, the argument of the mortality among children is at all times adduced, and their number deducted from that of the deaths among natives, thereby proving that, from the age of five years upward, the vital energy to resist disease and death is no greater in foreigners than in natives. I do not wish to discuss this point, respecting which much might be said in view of the lessons of history and of the biological sciences; but I must say something upon the subject of the element with which it is asserted that the balance against the native population is closed. Considered in its relation to social mechanics, a child is assuredly not a power, but a resistance; not a force, but a burden; and, according to this incomplete and fallacious theory, the death of a child would only be a loss in the breasts of those who mourned him, and not for the producing capacity of society, especially if replaced by a vigorous adult, come whence he might. Such a doctrine, condemned by our natural feelings, by justice, and by philanthropy, is also in opposition to the dictates of sound reason and statistics. Socially, the child is the moral bond and the hope of the family; politically, he is the citizen of the future. The citizen who has been born and grows up in the land of his fathers, not only assimilates the materials of his organism in the continued process of rapid renovation, but takes in, in that atmosphere, the constituent elements of his disposition, and thus becomes by progressive education, by the contemplation of vicissitudes and of the struggles of manhood, and by the innate love of all belonging to him, the surest guaranty of the firmness of the institutions under whose influence and in whose mould he has been formed. It is, then, the duty of well-constituted societies to bestow

attention upon their children in view of the double interest just referred to, and, if for nothing else, for the purpose of preserving the largest number possible for the battles of collective life.

If the mortality among Argentines in Buenos Ayres, and among Americans in New York, should become enhanced, or even retain its present proportions, it would be difficult to foretell how far the respective nationalities may become weakened as a ruling power in society, and how, little by little, that loss of influence may be felt in the customs of the people and the integrity of the institutions. In my opinion this danger is neither imaginary nor remote, and the only means I can discover by which it may be averted (and I have long studied the subject) is, by the energetic improvement of the sanitary conditions of states, cities, and towns; the reduction of mortality to its lowest possible expression (18 per 1000, for instance); and the perfecting of the systems of education, popular, moral, and physical. From what we have already seen concerning the susceptibility of favorable or adverse influences in infancy, if mortality be lessened, those most benefited thereby will be the children; while society, by the preservation of a larger number of these, will secure the organic advantages which I have mentioned.

Statistics have also a voice in the question. The city of New York, during the seven years ending in 1873, had a mean annual death-rate of 29 per 1000, while the rate of births for that period was very low. Taking the figures as they stand in the register, the number of deaths and births was as follows:—

Deaths during the seven years	189,385
Births " " " "	119,226
Difference against the population	70,159

which is equivalent to a loss of more than 10,000 per annum. So that if such losses were not compensated, and even exceeded, by immigration, the population would go on gradually decreasing. I should here mention that, according to official reports, the registry of births is so defective as to warrant the assumption that not more than 75 per cent. of the real number is recorded. Even accepting this hypothetical suggestion, and supposing no omissions to have occurred in the death-registers, we should have:—

Deaths	189,385
Births	158,968
	30,417

which would still leave a considerable loss for the population. And, in order to avail ourselves of these figures, let us suppose the mortality in New York to be reduced to 21 per 1000; in which case we should have the following results:—

Deaths	140,692
Births	158,968
	18,276

leaving a balance of 18,276 in favor of the population, in place of a real loss, as it has been sustaining; and this without taking into account the positive fact, based on experience, that the number of births increases with the improvement of the sanitary condition of the city. It is likewise to be borne in mind, respecting children, that mortality among them decreases much more rapidly with an improved healthy condition

of the city than among adults; so that, if, of the hypothetical number of deaths (140,692), 40 per cent. had been of children, the mortality of the latter would have been 56,276, instead of the 93,677 who died during the seven years, thereby constituting an economy of 37,401 infant lives, or as many fathers or mothers of families in the course of a few years.

During the same seven years, Buenos Ayres had 51,207 births and 58,771 deaths, including the three epidemics (1867, 1868, and 1871), or a loss of 7564 inhabitants. By reducing the annual death-rate to 21 per 1000, the mortality would have been 27,455 in the seven years, and the difference in favor of the population 23,752. I am not examining chimeras, but realities; death and life are recorded in official statistics, and the improvements which I hold as possible are graven in imperishable monuments, in the form of sanitary works, in those American and European communities which have struggled gloriously with death, and have conquered it.

Wherever there is death there is sickness. It is generally admitted, and to a certain extent proved by statistics, that the sickness-rate of a city or town is approximately two sick persons throughout the year to each death per annum. Hence, to ascertain the number of days of sickness suffered by the population, taken collectively, the permanent factor 730 (365 × 2) is multiplied by the number of deaths during the year: so that when the mortality is considerable, the number of sick persons is also considerable in like proportion—which is perfectly reasonable, inasmuch as sickness and death proceed from the same causes acting, in a given locality, upon each and all of the inhabitants. In 1875 the mortality of New York was 30,709; and, if, as has been admitted, there were two sick persons for each death constantly throughout every day in the year, the number above given should be multiplied by 730, and the product, 22,417,590, would be the number of days of sickness suffered by the population collectively, which is equal to 21.4 days for each inhabitant.

One day of sickness signifies one day's labor lost for those at an age for working and producing. The number of these is easily computed when we know that, according to the census of 1870, there were in the city 350,556 persons engaged in all occupations, that is to say, 37.1 per cent. of the total population; and, by applying this percentage to the population in 1875, we ascertain that as many as 387,769 persons were working, each one of whom lost by sickness 21.4 days of work in the course of the year, and all of them collectively 8,298,566 days. The remuneration for work varying considerably, according to the nature of the occupation and individual capacity, it is not easy to adopt a uniform standard; so I shall take the lowest rate said, by the Commissioners of Emigration, to have prevailed in 1875 for persons working by the day, namely, $1 to $1.50, preferring the first, in order to avoid all appearance of exaggeration. The value of the labor lost would amount, at the rate of $1 per day, to $8,298,566. The expenses occasioned by sickness are incident to the whole population; and the number of days' sickness, for population collectively, as above reckoned, was 22,417,590. The approximate amount of such expenses may be ascertained by means of the data furnished by the hospitals, and according to which each patient costs about $2 per day. Now, reducing those expenses by one-half (say to $1 instead of $2), and including therein the outlay for professional attendance, medicines, regimen, and nursing, for the inhabitants of all classes, the aggregate cost would be $22,417,590, corresponding to the number of days of sickness.

I shall not present in the pecuniary form the depressing influence which morbific causes exercise upon the capacity, physical and moral, of those individuals who, by reason of their vital energy, can resist the effects of such causes and preserve their health. To eliminate and prevail over a pathological agent, implies a waste of organic force in the process, and so much less force in the physical and moral aptness applied to production. *Mens sana in corpore sano.* It is enough to mention this last branch of the subject, the pecuniary value of which cannot be estimated.

Those who die during the working age have an independent value as capital irrecoverably lost. Not only the days of sickness, or the days of the year, are here lost, but the very instrument of production itself for the future. In the United States, an adult immigrant is estimated as representing a capital of $1000 incorporated in the national wealth: now 8580 adults, approximately, died in 1875, in the city of New York, figuring consequently for an aggregate of $8,580,000.

With these numbers, every figure of which I have carefully calculated according to data, either official or of acknowledged scientific authority, the losses in the account of the public health for 1875 may be posted in the ledger of the city of New York, as follows:—

Work lost	$8,298.566
Loss, expenses incurred by sickness . . .	22,417,590
Loss, adult lives	8,580,000
	$39,296,156

These losses cannot be avoided, but they may be considerably diminished. If the mortality had been 21 per 1000, instead of 29 per 1000, each of the items of the account would have been reduced in the same proportion; the aggregate would only have been $28,452,216, and there would have been an economy of $10,838,840.

That amount of money saved would be equal to the interest of a capital of $150,000,000 at seven per cent. And let it be asked of the distinguished Dr. Chandler, President of the Board of Health, or of the most skilled and experienced Sanitary Engineers, either of this country or of Europe, if that capital, or one-half thereof, intelligently invested in sanitary improvements in the city of New York, would not be sufficient to place that city on the level of London in point of salubrity. I am as certain that the reply would be in the affirmative as I am that never could money be invested in a more remunerative speculation.

As for the causes of the evil, and its remedy, there is a set of books which contains their explanation, and which is worth a whole library. The Reports of the New York Board of Health enumerate those causes, accompanied by eloquent suggestions of remedies, from year to year, supported by the authority of science and study. If the reading of those Reports were rendered popular, the influences which make an imperial metropolis an unhealthy city, in the eyes of the laws of hygiene, would be recognized; people would then know in what mortiferous conditions live one-half of the population, and how those conditions may be materially improved, and what are the existing defects in the system of drainage and sewerage, and the danger of those defects being enhanced, to the still greater detriment of the public health; the relations of meteorological phenomena to the mortality of the city would be discovered; and for each and every one of those evil influences would be found a certain remedy, suggested by experience both at home and abroad, or

the means of attenuating such influences as were not immediately dependent upon the hand of man. The recommendations of the board of health, once put into action, with the amplifications required by successive developments, would be sufficient to reduce, within the space of five years, the mortality of New York from 29 per 1000 to 21 per 1000, and thus secure all the benefits, physical and moral, to be gained by such a triumph.

It is for the interest of all, both poor and rich, natives and foreigners, to aid in the realization of sanitary improvements, which are peremptorily urgent for the present and necessary for coming generations. And it must be remembered that the cost of their execution is enhanced by every day's delay. One of those omnipotent movements of opinion which in a strong-minded people are commonly attended by prodigious results, would lead to the immediate and complete solution of the grand problem. Nor does this subject concern alone Buenos Ayres, in that far-distant region of America, nor New York, in this part of the continent: it is a subject which affects the interests of whole nationalities. New York, in 1790, had 33,000 inhabitants; eighty years later, with a million of souls, it is the third city of the Christian world, and is marching onward to become the first, at the close of the second centennial of the United States. Philadelphia competes with Berlin and Vienna; on the banks of the Mississippi is rising up St. Louis, another city which would monopolize the admiration of all who beheld it, were it not that on the margin of Lake Michigan, Chicago rears her head in pride before the astonished gaze of natives and foreigners—Chicago, the new born city, which now contains half a million of human beings. What, in the course of time, will become of these grand centres of civilization, and of many others as yet without a name? The problem of their destiny is in their own hands, and Sanitary Science should be the chief light to guide them.

The tendency to centralization, with its advantages and inconveniences, is inherent in human nature, and not to be controlled. In 1860, there were in the United States 18 per cent. of the total population in cities of 8000 inhabitants and upwards; and in 1875 the aggregate population of the cities of 8000 and upwards amounted to 10,116,000, or 24 per cent. of the total population, this last being estimated at 42,000,000. And this in spite of the agricultural interests which should tend to disseminate the population over the entire length and breadth of the land; spite of the rivers and lakes, canals and railways, the aggregate length of which would reach three times round the terrestrial globe; spite of all these facilities for cheap and rapid transit for people and industrial products, whithersoever called by the necessity of the market.

In the Argentine Republic, this instinctive force of centralization is still more marked than in the United States, and that for reasons, economical and geographical, which are readily perceived. The growth of the littoral towns is proportionately much more rapid than that of the towns in the interior, and particularly that of Buenos Ayres, where the agglomeration is altogether incommensurate with that observed elsewhere in the country. This is not a suitable time to inquire into the sociological and political consequences of this natural inequality; but, in treating of the life of cities and of the statistics of Buenos Ayres, to the latter are applicable such considerations as that species of agroupment may suggest. As for the causes of the excessive mortality of Buenos Ayres, they have now been briefly enumerated, as have also the measures undertaken for their removal.

For the rest, in the sanitary condition of cities, whatever be their population, it is incumbent, not only upon them, in as far as their circumscribed capacity may extend, to assume the responsibility of the present evil and of its future aggravation, but upon each and all of the inhabitants: all have to take part in the continued struggle in defence of life, as well individually as in municipalities, legislatures, and congresses. I know not whether the illustrious Cobden and Peel concerned themselves little or much with matters of public hygiene, but I do know that the liberal commercial legislation established by them in England, has served to improve the condition of the poor of that nation, by rendering their existence less burdensome through the lower prices of articles of food, and has contributed effectually to lessen the number of deaths and of those diseases which are induced by want.

In closing this paper, I feel it my duty to apologize for the liberty which I have taken of instituting comparisons with the vital movement of New York, without the authority of prolonged personal observation. On one hand, I should say that the Argentines ever look to this part of the world for example and stimulus; and on the other, that I have noticed such close anthropological analogies between what I have seen during my short stay in New York and that which has long been the object of my study in Buenos Ayres, that I have not been able to resist the desire of establishing a few comparisons, even at the risk of arriving at defective or inaccurate conclusions.

THE GENERAL SUBJECT OF QUARANTINE, WITH PARTICULAR REFERENCE TO CHOLERA AND YELLOW FEVER.

BY

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The earliest recorded measures instituted to prevent the spread of disease were adopted by Moses, who made regulations concerning the leprosy, in which he not only provided for the isolation of the infected people, but held the suspected for observation and cleansing. In the fourteenth century, certain restrictions were placed upon vessels arriving at Venice and Genoa, in Italy, to prevent the introduction of the plague, but it was not until the fifteenth century that quarantine was generally established.

The early system of quarantine was not based upon scientific facts or experience, but appears to have been an outgrowth of that peculiar period (noted for ignorance and superstition) known as the "dark ages." The name "quarantine" fairly indicates the hypothetical extravagance which held sway at its birth. As out of the *forty days'* fast in the wilderness grew *Lent*, the period of abstinence instituted for the purification of the soul, so the process of purifying ships and passengers from so called contagion was made to cover forty days, and this process was termed quarantine.[1] In the measures blindly enforced to protect seaports from infection, not only was commerce crippled, and property needlessly sacrificed, but the comforts and rights of the suspected were entirely ignored. They were not only compelled to undergo a tedious, and worse than useless, period of observation, but, by the very process of sequestration, they were exposed to tenfold greater danger from fomites that might chance to be in the ship. Many of the senseless and inhuman practices which grew up under the name of quarantine, have by degrees, and from time to time, been discontinued, and yet—so hard is it to cut loose from the influence of tradition—not a little of the fog and delusion associated with the term "quarantine" in the past continues to the present day, especially in the countries where quarantine was first established.

As the visits of the plague became less frequent and less severe, yellow fever and cholera commenced their migrations. These diseases gave a new interest to quarantine, which had previously been confined in its operations to the old world, but by these diseases was extended to the new. To the quarantine of ships, and of those who travelled by them, was added the "*cordon sanitaire*" against cholera, in the hope of preventing its march by land. This last measure has been enforced in Europe with the utmost rigor, and, though utterly futile in its results, appears

[1] Charles Caldwell, M.D., on Quarantine, 1837.

not to have been entirely abandoned in some localities even during the prevalence of the last recorded epidemic of this destroyer.[1]

In America, quarantine was first established in 1758, at New York, against yellow fever. Though I shall have occasion, further on, to speak of the administration of quarantine at New York, it serves to mark the progress and application of experience in this direction to note here some of the changes which have taken place at that port. In 1811, we find that vessels arriving from infected districts were detained thirty days at quarantine, and passengers and crews twenty days after the occurrence of the last case of yellow fever. In 1830, healthy persons arriving in vessels from infected localities were permitted to proceed to the city, but without their baggage. Notwithstanding that many pernicious restrictions still stand upon the statute-books of the State, the enforcement of them is left to the medical officer, whose opinion is more and more recognized. It is now found practicable not to detain either healthy passengers or infected vessels. This arises from the generally accepted principle, that healthy individuals do not convey the morbific poison of cholera or of yellow fever, but that the poison finds lodgment in confined clothing, and especially in the vessels themselves, under favoring conditions. To detain an infected vessel has been found to increase the virulence of the poison. Accordingly, the ship's cargo is promptly transferred to lighters in the bay, freely exposed to light and air, and the vessel, after thorough disinfection, is allowed to proceed. In strong contrast with the foregoing, is the practice pursued at Pensacola, on the Gulf Coast. In May, 1874, upon the arrival of yellow fever at quarantine, the City Council of Pensacola passed an ordinance which is here given in full:—

"Saturday, May 30, 1874.—Board met, pursuant to call from Mayor. The object of the call having been stated, on motion it was resolved that all vessels arriving at this port from infected ports shall be compelled to remain in quarantine until frost; or said vessel shall be allowed to depart from this port at any time during quarantine, at the option of the commander or captain of said vessel; but in no case shall any such vessel be permitted to load, take in, or discharge, cargo, within the limits of the quarantine station during the existence of quarantine; nor shall any person being on board any such vessel be allowed to visit the city during quarantine.

"Resolved further, that the Mayor be requested, and he is hereby fully authorized and empowered, to employ a good and sufficient armed guard, with a good and sufficient boat, to patrol during the night on an established line between the quarantine station and this city; and that the Mayor be requested to instruct said guard to prevent all communication whatever, by boat or otherwise, between said station and this city, and to use sufficient force to attain that end, should it be necessary to fire into the person or persons attempting to violate said quarantine.

"Attest: M. P. de Rioboo, *Clerk*."

The results of these widely different methods may be stated in a few words: During each of the past five years, and more, a number of infected vessels arrived at the New York quarantine station. Many cases of yellow fever were treated in Quarantine Hospital, but in no instance did the disease spread to the city, or affect a single attendant on the sick. At Pensacola, in 1874, the infected ships were anchored near the quarantine

[1] Dr. Milroy, in an article in the British and Foreign Medico-Chirurgical Review, No. lxxxiii., states that "in 1867 military cordons were drawn around the infected districts in Montenegro, and persons who dared to cross them were shot. The horrors of famine were thus added to those of pestilence."

station. Communication from the infected vessels and from quarantine was had by night with the land; and when, under the fostering conditions for propagation, the yellow-fever poison had gained sufficient virulence, the disease spread to the city and the naval station near by, the quarantine medical officer himself falling a victim to the system which he had blindly endeavored to carry out.

In passing to a brief notice of quarantine regulations at two other ports of the United States, it is proper to mention the fact that the control of quarantine in this country is not assumed by the general government, but that it is established and enforced by each seaboard State, or municipality, for itself. The only exception occurred during the war of the rebellion, when the commanding general of the Union armies, upon the recommendation of the Surgeon General of the Army, directed (1) that all vessels arriving from ports infected with cholera, but having had no case during their passage, should be quarantined for fifteen days and thoroughly fumigated, and (2) that all vessels having had cholera on board during their passage, should be quarantined for fifteen days after the termination of the last case, and thoroughly fumigated.

The quarantine regulations at the port of New Orleans, in 1822, authorized the health officer, in his discretion, to continue quarantine indefinitely, but provided that infected vessels should be detained not less than fifteen days after the recovery of the sick, and purification of the vessel. Healthy vessels from healthy ports were detained five days, and "vessels from any port of Europe, arriving between the first of May and the first of November, bringing more than twenty passengers, were not, however healthy, permitted to come within three leagues of the city, until after the first of November in any year."[1] A committee on quarantine for the State House of Representatives of Louisiana, reported that "during the last year [1822], notwithstanding the *strictest compliance* with these laws, our expectations were frustrated at the very moment when we thought we could indulge the hope of the most complete success." Under the present laws in force at the New Orleans quarantine, vessels hailing from ports infected with yellow fever are allowed to pass to the city after ten days from the date of leaving an infected locality, and in this period is included the time consumed in the passage. The Spanish bark Valparaiso, which introduced yellow fever into the United States in 1873, arrived at the quarantine station below the city of New Orleans on the 24th of June, eight days out from Havana. She completed the two remaining days of quarantine required by law, and having been twice fumigated with chlorine, and carbolic acid having been used in her pumps, she proceeded to the city, where the mate was attacked on board on the fourth of July, and died. Eight days subsequently, the mate of a steamboat, laid up for repairs at the same wharf, was also attacked, and died. The first case of the cholera epidemic of 1873, in the United States, occurred at New Orleans in the month of February, when quarantine is not enforced. The man who first died of the disease had resided two months in New Orleans, and at the time of the attack was engaged in discharging cargo from a Liverpool vessel.

Under revised quarantine laws adopted in South Carolina in 1869, healthy vessels arriving at the port of Charleston, in that State, from infected ports, are detained from fifteen to twenty days, and thoroughly

[1] History of Quarantine in Louisiana, from 1821 to 1846. By B. Dowler, M.D., New York Journal of Medicine, 1846, p. 160.

cleansed. If no case of disease be developed, the vessel is allowed to proceed to the city. If, however, cases of disease have occurred on the passage, the vessel is subjected to a quarantine of thirty[1] days, or longer; all sick are promptly removed to the lazaretto, and the vessel is disinfected as before. Under the careful enforcement of these rules, no case of yellow fever has entered the city, notwithstanding that the disease has been brought to quarantine every summer, except in 1871. In the latter year, when no recognized case arrived by ships, the disease broke out in the city.

Turning from the diversity of opinion, practice, and results of quarantine, in this country, we observe a most rigid quarantine enforced by countries bordering on the Mediterranean, while England has, during late years, maintained the free *pratique* of her ports. Because England, from her climatic conditions, comparative isolation, and wise measures of internal sanitation, is able to maintain this advanced position, it does not follow that the seaports of the continent can with impunity conform to her example. In India, the endemic home of cholera, quarantine has proved unavailing, and thence, from the standpoint of some observers in that country, it is argued that measures to contravene the progress of cholera are useless. Were quarantine to be established in the West Indies against yellow fever, it would require no prophetic foresight to predict the result. The conventions of Paris, Constantinople, and Vienna have happily done much to harmonize the conflict of opinion which has prevailed in reference to the particular system which should be applied in quarantine. This system, however, must necessarily be modified by the geographical position and climate of the countries concerned.

To ascertain, as nearly as possible, what precautions are necessary, and what restrictions superfluous, in the administration of quarantine, the next step will be to consider the characteristics of cholera and of yellow-fever propagation, in order to determine as nearly as may be the prophylactic value of *Port Sanitation*, a term which most definitely expresses the practical measures indicated by experience.

Cholera.—Amongst the many views respecting cholera, it is generally admitted that the endemic home of the disease is India, especially the valley and delta of the Ganges, and that it is caused by a specific poison capable of reproduction or multiplication. Whether the endemic existence of cholera in India is due to peculiarities of air, temperature, soil, or water, or to the filth of uncleanly masses of people, is not determined; but it is undoubtedly true that each and all of these influence the propagation of the poison, and the spread of the disease outside of India. Some of the apparently well-established facts respecting cholera in India are: (1) That it only prevails epidemically at intervals, usually of several years; (2) that since its first migration around the world, it has not passed the border of its endemic home to prevail epidemically in other countries, except at times when the disease has shown unusual virulence and diffusion in India; (3) that the number of persons attacked in the same town or city, during different epidemics, varies considerably, even where the surrounding conditions are apparently equally favorable to the spread of the disease; (4) that it is possible to flee from it,[2] showing that a locality or place is the centre or focus of infection; (5) that

[1] Dr. Lebby, the Health Officer at Charleston, has recently informed me that, at the last session of the legislature of South Carolina, the period of quarantine was reduced to fifteen days.

[2] The mountains of Lebanon have always proved a safe retreat for the inhabitants of the cholera-stricken cities of Syria.

recent improvements in the sanitary condition of jails in India have rendered them almost exempt from visitation by cholera, many escaping entirely, and very few of those attacked suffering with severity;[1] and (6) that attendants upon the sick, in cholera hospitals removed from infected localities, do not suffer more than any other class, and usually enjoy entire immunity.

Dr. Cunningham maintains that the spread of cholera does not depend in any degree upon the use of drinking water polluted by cholera evacuations. Over against this statement is to be placed the fact that the mortality rate from cholera has greatly diminished in Calcutta,[2] since the introduction into that city of water less liable to contamination than formerly.[3] The fact that those attacked by cholera, and those who escape, in any given locality, drink from the same wells or other source of water supply, is not proof positive that the cholera poison may not be conveyed through the medium of drinking water, while the condition of individual susceptibility remains as an undisputed factor in the development of the disease. Many undoubtedly receive the poison who do not suffer from it. Proof does not exist of cholera arising from impure water, uncontaminated with cholera dejections, but the cholera poison is unquestionably intensified in its action, or the susceptibility of individuals increased, or both, by the use of drinking water containing organic impurities. Experience outside of India[4] abundantly proves that the cholera-principle is portable; that it is carried by ships, and in clothing and baggage, when excluded from the light and air. The spread of epidemic cholera is characterized by localization in particular places, ships, or houses, which then become centres of infection. The activity of these *foci* of infection, as well as the intensity or fatality of the disease, appear to depend upon certain causes associated with the aggregation of human beings, and the attendant evils of impure air, impure water, and filth. When these are present, a mild case of cholera serves to kindle an epidemic as readily as a spark from the hunter's flint may kindle a fire in the dry, rank grass of a Western prairie; and the progress, controlling influences, and results, of both sparks, are not dissimilar. Individuals, infected with the disease, who travel during the period of incubation, may, under favoring conditions, set up new *foci* of infection at the places where they are attacked. These "favoring conditions" are both individual and local, and, when either are absent, the cholera germ may slumber, or may lose its reproductive power altogether. The non-receptivity of certain localities, and of individuals, has long been recognized, and is undoubtedly due to good sanitary conditions.

There is strong reason for believing that cholera dejections, which appear to be harmless in their fresh state, become actively poisonous

[1] Report of the Cholera Epidemic of 1872, in Northern India; by J. M. Cunningham, M.D., Sanitary Commissioner with the Government in India.

[2] British and Foreign Medico-Chirurgical Review, July, 1872, p. 56.

[3] Prof. von Pettenkofer states that whole towns in Germany, which were ravaged by cholera during former visitations, have been entirely free from the disease during late epidemics, and that this result has been brought about by thorough and efficient drainage, and by purification of the water supply.

[4] Both Dr. Cunningham and Dr. Bryson maintain that cholera is not spread by traffic or human intercourse. The difficulties in the way of proving or disproving the portability of the cholera-principle in a country where the disease originates, are too apparent to give any weight to this negation. The causes or vehicles which spread a disease, not contagious in the sense in which smallpox and typhus are contagious, can only be determined, if determined at all, outside of the endemic home of the disease.

with the commencement of fermentation or decomposition. Acid disinfectants interrupt or prevent altogether the fermenting process, thereby destroying the poison. In like manner, the acid secretions of a healthy stomach are believed to neutralize or render innoxious a certain amount of the cholera-principle. The almost universal testimony is that good sanitary conditions lessen the influence of the attack, restrain its dissemination, and frequently ward off the disease entirely. Dr. Harris has remarked, in referring to cholera in New York, in 1866, "interpret it as we may, the events in the experience of that visitation of cholera demonstrated the practicability of dealing with the transportable and exotic, or germinal, factor of the pestilence as an enemy to be held in restraint, and its propagating attribute to be destroyed—a pestilence to be *stamped out* by definite hygienic measures."[1] There is abundant proof that the specific poison of the first cholera epidemic in the United States, and of succeeding epidemics, down to the last one, was introduced by ships from Europe, and the testimony of European observers furnishes conclusive evidence that the disease was imported into that country from India. The last epidemic of cholera in this country has differed from previous epidemics, in that direct connection between the initial cases and an imported germ has not been established,[2] but the fact remains that nearly two thousand emigrants from cholera-infected districts of Europe arrived in New Orleans during the month of the outbreak and that immediately preceding.[3] It may be stated, as a general fact, that each period of epidemic prevalence of cholera in America has been associated with a similar period in Europe.

The morbific principle of cholera has from time to time shown a disposition to permanent lodgment by surviving the winter seasons of Europe[4] and America. In the epidemic of 1854, in this country, the disease spread to the Pacific coast, and was several times re-imported from Central America to the port of New York; but the generative principle of each epidemic has finally died out, and each succeeding epidemic has depended upon fresh importations from India.[5] Emigrants from infected districts have always been a prolific cause of intensifying and spreading the disease. "It is a matter of common experience to find that at times, when the disease is only of occasional or sporadic occurrence in Calcutta, emigrant ships leaving the port are scenes of severe outbreaks."[6] The readiness with which a vessel takes on the characteristics of a crowded community or city, and becomes a centre of infection, will be more readily appreciated, and the measures to prevent the spread of cholera by ships more wisely directed and applied, when it is considered that a ship is but a town afloat. The ill-ventilated hold and

[1] Reports and Papers of the American Public-Health Association, vol. i. p. 346.

[2] There occurred, in 1873, three later outbreaks at widely remote points in the United States, which were due to poison imported in personal baggage packed in the infected districts of Sweden, Holland, and Russia. Surgeon Ely McClellan, U.S.A., in his Report on the Cholera Epidemic of 1873, in the United States.

[3] Dr. McClellan, op. cit.

[4] Drs. Pelikan and Arkhangelsky have shown that cholera was present in Eastern Europe from 1847 to 1856, and again from 1865 to 1873, though evincing a feeble activity during much of the time.

[5] Prof. von Pettenkofer maintains that "it is a fact independent of all theory, which has been observed in every epidemic of cholera, in every place, and on every occasion, that the attacks in each individual house terminate on the average within twelve or fourteen days." Cholera, How to Prevent and Resist it. By Dr. Max von Pettenkofer. Translated by Dr. Hime, 1875.

[6] Calcutta Review, 1869.

foul bilges are the counterpart of the filthy sewers and cesspools of a filthy city.

Yellow Fever.—Yellow fever is a disease produced by an invisible poison, capable of self-multiplication outside of the human organism. This something—the germ or miasm which has hitherto eluded microscopical demonstration—is a product of the tropics. It shows a disposition not to spread in high, salubrious lands, but exhibits a preference for low regions, especially the filthy portions of cities. Filth and high temperature are as truly its concomitants as cleanliness and cold are opposed to its lodgment or propagation. In this country, yellow fever has prevailed in most of the Atlantic and Gulf cities, and in many of the towns along the Mississippi River.[1] In some instances it has been carried inland with people fleeing from infected localities, but it has never shown a disposition to spread epidemically at points remote from the continuous water-roads of commerce. The cities of the great lakes have always been exempt from the disease.

In considering the morbific cause of the disease, we are necessarily restricted to an examination of its characteristics or comportment, exhibited in different places and under different conditions. We are, however, met at the outset with the difficulty and uncertainty of distinguishing between yellow fever and the pernicious malarial fevers of the Gulf coast. The cases that end fatally furnish reasonable certainty of the diagnosis given, but the milder cases that recover scarcely escape from doubt.

This uncertainty in determining the nature of mild cases, divides the profession on the question of the endemic lodgment of the disease on the Gulf coast, and serves to complicate the question of quarantine. Whether the disease was endemic in the intertropical islands of America, before it was brought from Africa, is uncertain; but there is no doubt now of its permanent lodgment in the West India Islands. The fact that the island of Key West, off the southern extremity of Florida, is in daily communication with Havana, makes it extremely difficult to determine the question of importation at that particular point. The weight of evidence, however, is against the assumption of an indigenous origin of the disease in the border lands of the Gulf, but it is probably true that the poison has there been preserved through the winter in fomites, though it is believed that the United States is not affected epidemically except by the imported germ. The germ is transmissible. It is capable of being transported in the personal effects of passengers and sailors, but the damp, filthy holds and bilge water of ships are its favorite lurking places, where, if confined, it multiplies and increases in virulence to such an extent as not only to affect those on board, but even to exert its pernicious influence in the direction of the currents of air to a considerable distance. A vessel anchored at a wharf in Havana, or other infected place, appears to become, for the time being, a part of the infected region; and in sailing to a distant port, the ship transports the poison as surely as could be done by transplanting a section of the infected island of Cuba itself, were this possible.

Yellow fever is not communicated from the sick to the well, the sick being dangerous only as possible carriers of the poison-germ or miasm; hence all persons from an infected district may safely be considered as

[1] Greensville Dowell, M.D., of Galveston, states in his forthcoming work on the History of Yellow Fever Epidemics, etc., in the State of Texas, that "yellow fever has spread to 228 cities and towns, and in 28 States in the United States, appearing 741 times, and causing 65,311 deaths."

harmless when the period of incubation, which lasts from two to six days, has passed. This simplifies the question of quarantine, and indicates the direction of preventive measures to the vessel and cargo, or to the locality, if the poison have found lodgment on shore.

We have come now to the question of practice, and to the consideration of the methods which should be pursued to secure the greatest protection to the public health against cholera and yellow fever, with the least restriction upon commerce. We may differ on the questions of the nature and modes of propagation of cholera and yellow fever, but it is believed that all meet on the common ground of prophylactic measures, and will accept the proposition of *General Sanitation*—applied to the endemic abodes of cholera and yellow fever, to ships, and to exposed countries—as the surest means of stamping out these diseases. The general question of preventive measures, is one intimately associated with that of higher civilization. Every improvement in India, in matters of drainage, in securing pure drinking water, and in personal cleanliness of the people, will tend to lessen the ravages of cholera there, and to diminish the chances of its spread abroad. In the West Indies, the harbor of Havana is most frequented by commercial people, and is the most prolific source of yellow fever.

In a recent personal letter to myself, Dr. Peters has expressed the opinion in reference to Havana, which he had just visited, that "an international public sentiment should be created against the filthy and careless ways of the authorities, which cause so much suffering and death among the mercantile and public navies of the whole world. The harbor is small and landlocked; all the drainage and sewage of the city go into it, and very little gets out of it." Dr. Peters suggests that a canal could be made at the head of the harbor, to the ocean, allowing the tide-water to sweep through the whole harbor, and thus remove its unhealthfulness. A vessel exposed to the influences of an infected harbor may not be able to escape, but the danger can be greatly lessened by enforcing strict cleanliness of the men and vessel. The bilge should be changed every day with sea-water, until the pumps bring clear water. The men should be compelled to bathe and change their flannels daily, when their work is done, and should not be allowed to sleep on the open deck, or in the lower part of the vessel. The same precautions should be continued through the voyage from a yellow-fever port. Ventilation should be freely encouraged, and no confined air allowed in any part of the vessel, if it be possible to prevent it. There is an example of a ship trading between New York and Havana, on which these precautions have been strictly enforced for a period of twelve years, and not a single case of yellow fever has occurred on board.[1] The proprietors of a line of steam-vessels, trading and carrying passengers between West India ports and New Orleans, employed an experienced person on each vessel, during last year, to supervise its sanitary condition and to disinfect it after the discharge of each cargo, with a similar result.[2]

I have already described the manner in which a ship becomes infected with cholera, and, as cleanliness is the great end to be attained, and maintained, on shipboard, the general preventive measures against this disease are not unlike those suggested against yellow fever. If, however, one or more persons are seized with cholera on board a vessel, they

[1] Communicated by Dr. Vanderpoel. [2] Communicated by Dr. C. B. White.

should be separated from the well passengers and crew; their stools should be disinfected with a solution of sulphate of iron, and at once thrown into the sea; and all soiled clothing should be burned or thrown overboard. But if cases occur after the first four or five days out from port, it is safe to conclude that the vessel is the source of infection. Any suspected baggage should then be disinfected, and, for ten or twelve days, all water used on board should be boiled before being served out.[1] Experience teaches that a cholera-infected house, or ship, ceases to impart the disease after about twelve days, unless a new crop of the poison be introduced. An outbreak of cholera on shipboard can certainly be stamped out, and it is possible to do so during the passage of a steamship from Europe to America.

The foregoing suggestions can be carried out by any master who possesses sufficient intelligence to navigate a vessel, and it is believed that by imparting such definite information to seafaring men, commerce will be relieved from most of the restrictions reasonably imposed by quarantine. The *unreasonable* hindrances imposed by quarantine can only be corrected by cutting loose from senseless traditions and theories. It is unreasonable, as it is impracticable, to apply a theoretically uniform quarantine to all places, without reference to climate, the relations of surrounding countries, or the natural history of the disease to be combated. The question should be settled, separately and practically for each country or place, by taking into account the liability to infection of the port of destination, the period of incubation of the disease, the length of time consumed in the passage, and the measures enforced by the vessel *en route*. True quarantine measures should commence as soon as the vessel has left an infected port, and, if commercial people find that their well directed efforts are recognized by the port health-authorities, a strong incentive to rigid cleanliness will be thereby at once established, infectious diseases will be measurably restricted in their course, and the problem of quarantine will be made comparatively easy of solution. The practice of medical inspection and sanitation, as adopted and now carried out by Dr. Vanderpoel, at the port of New York, expresses the practical application of our present knowledge of cholera and yellow fever, as found applicable at that port.

If, upon arrival from an infected port, it be found that a vessel is clean, and that no case of sickness has occurred on board during her voyage, any detention of passengers and crew beyond the time required to thoroughly air and disinfect their baggage and dunnage, is believed to be unnecessary, provided that the period of incubation of the disease has passed during the voyage. If cases of *yellow fever* have occurred during the passage, the passengers and crew are removed from the vessel; the sick, if any, placed in hospital, and the well detained in a comfortable, healthy place, until five or six days after the occurrence of the last case, when they are allowed to proceed to the city. Meantime the vessel is at once subjected to a thorough airing and fumigation, and the cargo transferred to open lighters in the bay, by men kept for the purpose. During the unloading, the vessel is daily fumigated, and, when the transfer is completed, every available part is cleansed with water, and, after a thorough disinfection and fumigation, the vessel is returned to commerce. It is not found practicable to lay down arbitrary rules in regard to vessels on which *cholera* has occurred. Each vessel is judged by the particular features

[1] Plan recommended by Macnamara.

which belong to it. If prompt and energetic measures be enforced at the first appearance of the disease, its spread can be cut short, as before stated, and when this has occurred on vessels coming from Europe, it has been found practicable to allow the vessels, passengers, and crew to proceed to the city with only such delay as has been required to definitely ascertain the facts, and to perform disinfection. A vessel arriving, however, with cholera on board, the same precautionary measures are carried out as are enforced on the arrival of ships with yellow fever, and eight days after the occurrence of the last case, the passengers are allowed to leave. While the foregoing measures are necessary at New York, England wisely confines her supervision of cholera, from without, to the removal and care of the sick, and the cleansing of the vessel. It would be futile for England to quarantine against ports of the continent, with which she is in daily communication. Mr. Simon has truly said that "contraband of quarantine, like ordinary smuggling, is developed so soon as the inducements for it are considerable." Yellow fever is less liable to find lodgment in England than in Spain and Portugal, or in New York.

As it is the custom for many emigrants, bound for America from the continent of Europe, to embark at Liverpool or other healthy ports, when cholera prevails on the continent, this country is exposed to the introduction of the disease through the personal effects of such emigrants, sailing on healthy vessels from healthy ports. In a recent report to Congress, I called attention to the necessity for "prompt and authoritative information to threatened ports [of the United States] of the shipment of passengers or goods from a cholera-infected district," and suggested that the consular officers of the government should be instructed to place themselves in communication with the health-authorities of their respective localities, and to advise promptly, by cable, of the outbreak of cholera, and the sailing and destination of any vessels carrying passengers and goods from infected districts. By this plan (which the Honorable Secretary of State deems it practicable to carry into effect, if Congress provide the necessary means) the thorough disinfection of infected articles would be insured, and the danger from this source of infection would be lessened.

In considering the value and practice of disinfection, the fact should be kept in view that disinfectants are not as much needed as is cleanliness; where the latter prevails, the former are not required. It is when there is not time to get rid of filth, or when the poison has found lodgment, that disinfectants are applicable, and then their application should be thorough. The value of the several disinfectants is recognized, but their use on ships depends chiefly upon the manner of application. Ordinary fumigation cannot be relied upon to destroy the poison of an infected vessel, without removing the cargo, as has been exemplified in many instances, notably in the case of the bark Valparaiso, before cited. Dr. Perry[1] has devised an apparatus, which he used successfully at the New Orleans quarantine, in 1874, whereby either sulphurous-acid gas, carbolic-acid vapor, or heated air, can be forced into a ship through flexible rubber pipes, by means of blowers propelled by steam, all hatches and other openings in the ship having been first securely closed. Whether

[1] Effectual External Sanitary Regulations without Delay to Commerce. By A. W. Perry, M.D., of New Orleans. American Public-Health Association's Reports and Papers, vol. i. p. 437.

this process, in which are employed from thirty to one hundred pounds of sulphur, for a single vessel, will always prove effectual without removing the ship's cargo, remains to be determined by further trial.

It must be admitted that the germs of infectious diseases may elude the most vigilant sanitary supervision of shipping which can be devised; hence the importance of municipal co-operation, to the end that all exposed cities may, through wisely directed sanitary measures, be rendered less susceptible of infection. The value of sanitary supervision of ocean-travel and traffic, and of preventive measures at home, is more and more recognized by civilized nations, and the enlightened and vigorous prosecution of these means gives hope of rendering exposed countries less liable to infection; but the repeated battles against cholera and yellow fever should awaken an *international* spirit, and a determination to carry the war upon these diseases to their endemic homes, and to there instruct the people, and if need be compel them, to employ rational means of prevention.

From what has preceded, the following conclusions appear to be justified:—

I. The supervision of ocean-travel ought to be directed to securing good sanitary conditions for vessels at all times, out of as well as in port.

II. A system of Port-Sanitation should be adopted and administered for each country or place, separately, and should be modified in particular cases by taking into account the liability of the port to infection, the period of incubation of the disease, the length of time consumed in the voyage, and the measures enforced by the vessel *en route*.

III. In some countries, the detention of passengers and crews of ships hailing from infected ports is warranted, but for such time only as is necessary to complete the period of incubation of cholera or of yellow fever, counting from the date of departure from an infected port, or of landing from an infected vessel; in no instance should passengers or sailors be held for observation on board an infected vessel, and such vessel should not be detained beyond the period required for inspection, and for thorough disinfection and cleansing.

IV. Recognizing the fact that the morbific causes of infectious diseases may sometimes elude the most vigilant sanitary supervision of shipping, the importance of wisely directed internal sanitary measures can scarcely be overestimated.

V. As far as America is concerned, it is desirable that prompt and authoritative information should be had of the shipment of passengers or goods from districts infected with cholera or yellow fever, thereby insuring the thorough disinfection of infected articles.

VI. The endemic homes of cholera and yellow fever are the fields which give the greatest promise of satisfactory results to well-directed and energetic sanitary measures, and to this end an international sentiment should be awakened, so strong as to compel the careless and offending people to employ rational means of prevention.

Discussion on Dr. Woodworth's Paper.

After the reading of the preceding paper, Dr. A. S. Baldwin, of Jacksonville, Florida, said:—In the port where I have lived for the last thirty years, there has been no visitation of yellow fever as an epidemic. Isolated cases

have presented themselves, but there has been no spread. In one instance, the disease was brought in the contents of a valise. A gentleman, a resident of that section of country, had been visiting in Havana, and in the course of his rambles had passed through the Yellow-Fever Hospital. In a valise, which he had opened two or three times on the way, the disease was carried, and affected a nephew and niece who met him on his return. The niece recovered, but the nephew died, and a *post-mortem* examination showed the liver to be of a boxwood color. No other case of yellow fever followed from that outbreak. In 1857, a disease broke out in Jacksonville, supposed to be yellow fever, but it could not be traced to any foreign cause, and it differed somewhat in its symptoms. During the summer a marsh had been cut off, and a railroad depot erected thereon. This was in June. On the 20th of August, an individual living near the marsh was affected with a fever; on the 20th of September, the disease was still existing in that neighborhood, and had occasioned much alarm, 100 deaths having resulted. But the fever was as unlike the cases of yellow fever which have been brought to Jacksonville, as typhoid fever is unlike typhus.

Dr. Henry Hartshorne, of Haverford College, Pennsylvania, said:—If we believe that yellow fever is not personally communicable, there should be no detention of the sick at quarantine. In regard to cholera, the fact mentioned in the paper that persons escape by fleeing from the disease, is a proof that it is not more communicable than yellow fever. Efforts to disinfect the stools of patients have notoriously failed to arrest the disease, while local sanitary regulations have in this country prevented its spread. Quarantine and detention have caused many deaths on infected vessels. What is wanted, is to do away with personal detention at quarantine, and to see that sanitation is enforced at sea and in port. A comparison has shown that rigid restrictions have failed in Pensacola, while, in New York, less rigid restrictions have been attended with better results.

Dr. S. Oakley Vanderpoel, of New York, said:—There are three distinct schools of belief, as to quarantine: One relies wholly upon a certain amount of detention, and with another, everything depends on cleanliness, following out the English hygienic system by which quarantine has been done away with. I think that the modern school is the best, which employs a combination of moderate detention and sanitation. I am willing to go as far as any one in praise of local sanitation, but there is a necessity for the detention of vessels coming from infected ports. I say too that yellow fever is not contagious, from person to person, but I think that persons should be detained until the period of incubation of the disease has passed. In New York there is no *quarantine*, that is to say, no forty days' detention, as the name implies; there may be a delay of a couple of days, but the period of incubation must be allowed to elapse before passengers' baggage and clothing are permitted to go into the city. There have been several instances in France in which disease has been carried away to the country in clothing. A Savannah steamer came into New York Harbor last Sunday morning. The crew and passengers were perfectly well, and the vessel had not been lying in that portion of the town in which the epidemic was raging, but I held the vessel twenty-four hours, to make up the five days out. I do not call that a quarantine. Quarantine has killed more persons than it has saved. The epidemic in New York, in 1856, was caused by the Health Officers placing a vessel with its cargo off Long Island. Exposed to the sun, and lying there for twenty days, it became a pest-ship, and soon from 150 to 200 vessels were strung along the Long Island coast, all infected. Clothing and offal were thrown overboard from these, and people living within two or three miles of the coast suffered. In relation to cholera, I believe that if the dejections can be disposed of immediately, the risk of contagion can be prevented. There is a period of from one and a half to two hours, in which the dejections are inert; after that, fermentation sets in, and they become contagious. As far as I have been able to study the subject,

cholera can be traced along the lines of travel. Even with the same atmospheric conditions, five or ten miles from the line of travel, the disease is not found. There are seasons in which epidemics are intense, and others in which, if proper sanitary precautions be taken, cholera, though breaking out with great virulence, will last only a week or ten days.

Dr. J. G. Kerr, of San Francisco, said:—In the cities of China there are no municipal governments, and no arrangements are made for cleaning the streets. The filth which accumulates remains in the streets until carried off by natural processes, or carted off by scavengers. Some streets are filled the whole year with filth. I witnessed two epidemics of cholera in China, but they did not seem to be influenced by the filth, nor confined to the filthy sections. Why is this, when so much stress is placed in America and in Europe on cleanliness? My own opinion is, that it is due to the absence of underground sewerage. In the cities of China there is no system of underground drainage, and the gases which form are not confined. Epidemics there are not worse than here. The poisons which propagate these are fostered by the arrangement of drainage here, and fermentation in sewers is as much to blame as anything else. In China, where no attention is paid to drainage, there is no confinement of gases, and no concentration of influences which produce disease. My observations were confirmed by a physician of the British Army, who, during the Chinese war of 1856–1857, visited cities where the accumulations of filth were obnoxious to strangers.

Dr. Hartshorne said:—It can be easily seen that scattered filth is not as poisonous as when it is concentrated, but the fact still remains that filth is one of the factors of disease. If it be true that the period of incubation of cholera is short, yet I have known cases in which cholera has appeared when a vessel has been two weeks at sea. The morbific agent which produces cholera has met a vessel out at sea, and though the ship has been detained in quarantine, yet in all that time the disease has not passed to the cabin-passengers, showing that it is connected with local conditions and migratory causes.

Dr. Vanderpoel said:—I agree with Dr. Hartshorne that cholera has appeared in vessels hailing from perfectly healthy ports, but inquiry has shown that the steerage-passengers, among whom the disease has broken out, did not belong to the port from which they started. For instance, most emigrants to this country start from Liverpool, yet they belong to Poland and other places. These outbreaks on vessels have thus been traced to sections in which the disease has been raging. In the clothing of these emigrants, there is undoubtedly a large quantity of soiled linen, which, as is well known, carries the disease. The fact of the vessel coming from a healthy port does not show that every one on board belongs to the healthy port. I can account for what Dr. Hartshorne has related, by the baggage being closed until the vessel has got out to sea; case after case has been traced to trunks. That is one reason why the disease does not go among the cabin-passengers; steerage-passengers generally keep their baggage about them, while cabin-passengers have their trunks stored in the hold, and there they remain until the end of the voyage. Besides, the cabin-passengers are more cleanly, which is another point in favor of Dr. Hartshorne's idea of sanitation.

Dr. Ezra M. Hunt, of Metuchen, New Jersey, said:—I remember a case in which a lady visited a hospital, and three weeks afterwards wore the same silk dress which she had worn while at the hospital. Scarlet fever broke out immediately afterwards. As to filth, we have never yet had it proved to originate cholera. We understand that filth is a nest for disease, some of which it does, and some of which it does not, originate; and this is a good reason why we should get rid of it. The fact that it does not originate cholera and yellow fever, does not weaken the importance of not letting their contagia get into the filth. To prevent the spread of these diseases, we must stop baggage and disinfect vessels, and stop passengers too, if necessary, but not keep them on the ships.

A UNIVERSAL PHARMACOPŒIA.

BY

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The idea of an International or Universal Pharmacopœia, though by no means new, has within a few years past come before the medical and pharmaceutical professions with more than ordinary prominence, and much has been written on the subject. The rate of progress in the art of medicine in its utility to mankind, depends largely upon accumulated experience; experience is the result of research and observation; and the value of an observation is in direct proportion to its accuracy and completeness. That classification and comparison of observations, the result of which is experience in the art, depends largely for its value upon the uniformity of the conditions under which the observations are made, and hence the present strong tendency to control and to formulate the conditions under which experience accumulates, so that a larger proportion of it may take the rank of absolute knowledge. For example, we know, as the result of observation which has passed through the stage of experience to become absolute knowledge, that opium relieves pain. This reaction between opium and pain involves two distinct series of phenomena, or is conditioned from two separate bases: First, the cause, nature, and quantity of pain, and this concerns the pathologist; and, second, the cause, nature, and quantity of the opium appropriate to the reaction, and this concerns the therapeutist. Pain is universal. Opium has, through knowledge, become almost universal. To formulate and condition the opium for this reaction upon the best results of general experience in the past, so that future experience may accumulate under uniform conditions of the medicinal agent—and may accumulate universally to the same single end, namely, advanced knowledge for universal good—is the object sought for in a Universal Pharmacopœia.

Some such train of reasoning as this, having for its basis, at least, the establishment of a universal standard of strength, composition, and quality, for medicinal agents, so that by accuracy in the agents the results of their use all over the world may be compared with accuracy, and thus more rapidly convert empirical experience into knowledge, has been generally advanced in support of the design. And it may be said that the same line of argument, and the same necessities which made National Pharmacopœias first useful, and then indispensable, must, with the advance of civilization, apply with equal force to produce an International Pharmacopœia. Thus the advantages to be expected from a Universal Pharmacopœia are so great and so easily shown, and the arguments for it are so forcible, that up to this time all writers upon the subject, as far as seen, in all nations, have been on one side of the question. And yet, although ranged on the same side of the question, I have, by thoughtful consideration of the subject, gradually reached the conclusion that the design is premature, and for the present age impracticable, for the following principal reasons:—

(1) The difficulties of constructing a Universal Pharmacopœia, which would be universal in any true sense, would probably be at present insurmountable, because, as in other international designs of a similar character, the present national standards are so very different that they could not be made to harmonize without radical changes, which the standard of medical education of the age is not high enough to admit; and because each prominent nation would at once seek to make its own pharmacopœia the basis to which others should be changed, as the condition upon which it would share in the undertaking, just as is done at present in the attempts to unify the coinage of different nations. And if a Universal Pharmacopœia should be constructed, with one or more prominent nations omitted from active co-operation, it would be but a sham, and would in the end be productive of more harm than good, since anything of the kind which might not be thorough and sound, would not stand, nor be authoritative, and would but add to the present confusion. The complete idea of a Universal Pharmacopœia is, of course, that it should take the place of all national pharmacopœias, and render these unnecessary, so that they should be abandoned. But short of this, there seems to be a popular idea which aims to embrace in the design only the making of the preparations, or the compound medicaments whether chemical or galenical, of a uniform strength and character, leaving the national pharmacopœias to be as necessary as ever, but to be in accord in regard to the strength and quality of the agents of certain classes of remedies which have been long and commonly used in different nations by the same or similar names. For example, colocynth is used under the same name in all the national pharmacopœias, but its preparations and compounds in the different pharmacopœias, under the same or similar names, contain it in very different proportions. To have such preparations and compounds of the same strength, and to have them designated by the same name everywhere—and this only—leaving the national pharmacopœias as they are with such exceptions of common accord, seems to be the less complete idea of a Universal Pharmacopœia aimed at by some writers. But this really involves no less difficulties than the complete idea, nor are the difficulties of a different character, nor more easily overcome. For the primary substance itself must be uniformly defined or described, or otherwise it may vary so much in different countries and markets as to make its preparations and compounds as different and as variable from that cause as they would be from different proportions.. And all primary substances and chemicals would require the same definition and description in order to secure their uniformity in use. Hence the difficulties in making a Universal Pharmacopœia seem to be insurmountable in the present age. But let it be assumed that a proper commission or council could be had, and that the great amount of time and labor needful had been given to the work, and that it had been harmoniously completed to the entire satisfaction of all nations; even then, unlike coinage, and weights and measures, it could not be considered as either established or completed, since in order to carry out the idea, and preserve its utility, it would have to be revised at short intervals, and the commission would have to be reconstructed from time to time, or would have to be permanent.

(2) To have a Universal Pharmacopœia, involves the idea of universal applicability. Could it be made universally applicable, or universally useful? In civilized countries the civilization is not uniform in degree, and between nations there is a still wider difference in this respect. All the normals of different nations differ, that is, geographic position,

climate, race, food, clothing, manners, habits, etc., make nations differ so much that their health and their diseases are not the same either in cause, course, or result, and are therefore not subject to uniformity in management or treatment.

(3) Again, the standard and degree of medical education is anything but uniform throughout the world, and therefore a uniform code for its use might not be generally applicable, any more than a universal grammar would be applicable to popular use in all languages.

(4) Finally, all modern progress in the medical art points unmistakably to singleness and simplicity in medicinal agents, and in their application. The older and more complex formulas are rapidly passing out of use, and single substances of more or less known physiological power, and of definite constitution, are as rapidly taking their place in the only true and permanent interest of the art; because upon such medication only can much accuracy of observation be claimed. Then, just in proportion to the progress in this direction, does a Universal Pharmacopœia become less necessary and less useful. And by means of this progress, the idea of a Universal Pharmacopœia becomes year by year more easily carried out, because simple substances of definite constitution and established character and power are perhaps alone applicable to universal uses, and because such are easily defined, described, and controlled.

These apparent difficulties in the way of a Universal Pharmacopœia are presented with hesitation, because the offering them may be construed into opposition to a design for improving the means whereby precision of observation and of result is to be looked for. The suggestions are made, however, in no such spirit, but simply to present a few points which may serve to awaken thoughtful attention to that side of the question which has not hitherto been sufficiently considered.

THE RELATIONS OF THE PHARMACIST TO THE MEDICAL PROFESSION.

BY

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OF METUCHEN, N. J.

The relation of Pharmacy to Practical Medicine is not merely collateral, but to the extremest degree radical and essential. All the commandments that form the decalogue of the physician find their summation in two avowed purposes. One is thoroughly to make out the disease, and the other is thoroughly to make out the remedy. Anatomy, physiology, and pathology, mean little to us professionally and practically, unless thereby we establish standards for the normal and abnormal, and by comparisons and observations are able to arrive at a diagnosis. Diagnosis means but little unless it defines, and differentiates, and knows, in order to guide to correct treatment. But treatment is as impossible without accurate pharmacy, as is diagnosis without accurate physiology. Indeed, the term diagnosis is as applicable to Materia Medica as to Disease. It is just as indispensable to know through and through a medicine, as it is to know thoroughly a disease. If there is failure at all either in the one or in the other, we have, as far as ignorance can accomplish it, an absolute failure resulting, just as if both were alike defective. A polished steel bar on one side, elaborately finished and laid, and a tarred cable-rope on the other, never yet made a railroad track. None the better will there be efficiency in conducting onward this great science of ours, with elaborated diagnosis on one side, and a smeared pharmaceutics on the other.

I am quite aware that in what may be called popular medical feeling, diagnosis of a disease outranks diagnosis of a remedy. There are a glow and an enthusiasm about anatomy, histology, physiology, and pathology, because of the subjects themselves, and of the accuracy of classification of which they admit, but which has not as yet its parallel in the corresponding department. A human being is so much grander a thing than a poppy-seed, that it is much more attractive to pry into the cells and functions of the one than into those of the other. The old story of the Frenchman, who in his hospital rounds thrilled his pupils by his accurate demarcations between healthy and diseased action, is still pertinent. When asked the remedy, he turned away with a shrug, as if that was a matter in which he had no concern. Until with equal identification and elaboration he had indicated this, he might be anatomist, histologist, physiologist, and pathologist; but he could not be a physician.

The anatomy, histology, physiology, and pathology of the Materia Medica must be studied individually and accurately, just as we study the subject Man. If, in a certain sense, the one is to be investigated as if the question of treatment would never occur, so the other may be studied as if the drugs concerned were never to be employed as remedies. Such at least may be the tendency of the naturalist, in either department. But

he must know that his studies, as thus pursued, are merely preliminary or collateral. His relations to medical practice (when he stops here) are only those of the botanist, geologist, chemist, or comparative anatomist. Our practice, our profession, our art, has as its sole object our ability to make such an adjustment between the individual, corporeal man, and the substances found in nature outside of him, as will promote his healthfulness. This, and this only, is the practice of medicine.

Therapeutics, since it is the application of remedies to the cure of disease, is almost a definition of the art of medicine. Dunglison defines a therapeutist as "a practitioner of medicine." Watts is only a little more general when he says, "Medicine is justly distributed into prophylactic, or the art of preserving health, and therapeutic, or the art of restoring it." Let it then be axiomatic with us that none but a therapeutist is a physician. The knowledge of the remedy in all its accuracy and in all its effects, is as essential as the knowledge of disease. It is the conjunction that marks the inflowing tidal-wave of medical knowledge, and that bears with it the skilful practitioner. So fundamental and essential is the concurrence of these conditions of medical art, and so palpable the proposition even upon its mere bare statement, that we can scarcely account for the existing contrast and discrepancy between the prominence given to the study of disease, and that tendered to the study of remedies. Handfield Jones wrote to me some time since, in a private letter, "a more perfect knowledge of the action of remedies is the great desideratum of the day."

A recent prize essay of the New York State Medical Society (1862) had this as its motto: "Physiology has become a science by well-conducted experiments that have illustrated facts. Can therapeutics claim to be a science while so many of its votaries are content with assertions, without experimental facts to support them?" We must at least affirm that unless therapeutics be a science, physiology cannot make the practice of medicine an art. This necessitates as a basic condition, not only of success but of all experimental investigation, a certified reliability in the articles by which our therapy is tested and applied. What we need just now, is the most accurate assemblage and classification of experimental and experiential facts as to medicines themselves, and the greatest safeguards as to purity and uniformity, in order that with scientific exactness we may trace and record their actions. Accurate study and adjustment here are very difficult to secure, as is almost everything of value in our art. But only thus can that exactness of result be obtained which will make real treatment not only possible but actual.

I have said thus much as to the general significance of therapeutics, and its essential foundations, in order to magnify and emphasize this fundamental condition of the progress of rational and practical medicine.

Our definiteness must begin with definiteness as to the disease which we have to treat, and definiteness as to the medicines which we propose to use. To this end, uniformity of preparation and reliability of dispensation must accompany a definite knowledge of what we have a right to expect from the quantity of each article named in our prescriptions. How absurd that we should seek for and watch the action of a preparation, and expect and record salutary results from its use, unless the preparation itself be of an indisputable character. Thus it needs no argument to prove that the relation of what is called Pharmacy to the Practice of Medicine is vital. It has so to do with the essence of the thing, it is so much the centre and substance of our professional science

and art, that it is an accessory before the fact. It will not do to assign it to a territory adjacent to that of the medical profession. Such border land is always doubtful land. As well might I recognize my right foot or my right hand as an acquaintance: my only real prospect of material success is to recognize and have them a part of my integral self. Nay, the case is even stronger: it is wearing one of the vital organs outside of the body, to put the framers and formers of our remedies totally without the pale of our profession. Their department must have fundamentally the surveillance of the medical profession, and this can only be secured by its being within the adjudication of the faculty. Other assurances of competency may be occasional and accidental, but only this can be reliable, when the two branches of science are so closely connected. When seeking aid in our diagnosis, we consistently go to him who, while belonging to our profession, may have made of one department a specialty. When he puts himself outside the pale of the profession, or is so put by circumstances, whatever his boasted attainments, we recognize that it is not for our interests, and still less for the interests of medical science, that we should avail ourselves of his assistance. Yet for aid in the department of therapeutics, that other arm of the profession, a very great part of our dependence is upon those who are outside. The relation is a purely commercial one, and tempts to demoralization. No profession can ever put so much of its practice, of its essentials, so totally outside of itself, and yet conserve its best interests as a science or an art. The effect always will be to enfeeble the practitioner as to his knowledge of remedies, so that he will not even know how to adjudge competency, or to verify the assurance of reliability and accuracy, while another effect will be to make the dispenser a mere tradesman.

The first step in remedying this evil must be to hold pharmacy to accountability and reliability, by making it a part of the profession of medicine. It must be a specialty, like every other medical specialty, having its foundation in a thorough medical education. It must, like ophthalmology and gynæcology, be made a specialty, only because it may not be practicable for each practitioner to attend to every department of medicine, and in order that by giving more time for study greater accuracy may thereby be attained.

It is a noticeable fact, in the history of medicine, that this was just the position that pharmacy formerly occupied. It was made a specialty not that it might be a trade, adjunct and accessory to, yet outside of, medical art, but because it was so fundamental and essential as to require concentrated study within the field of our calling. Even the English method at the present day shows how thoroughly the Apothecary's business was identified with the medical profession. It was so fundamental and integral as to give name to the practitioner.

The results of the separation which has occurred are so patent as to be undeniable, and so degrading as to be notorious. The average American dispenser is a dry-grocer, who avails himself of the patronage of the medical profession in order the more to thrive as a merchant of drugs, dye-stuffs, prescriptions, wares, patent medicines, fancy articles, liquors, and manifold incidentals too numerous for cataloguing. The price-current, for instance, of a prominent New York drug house, in over two hundred pages, has only about forty devoted to drugs, and only one-half of these are used medicinally. Not a few bear testimony that their largest transactions are in patent nostrums, in which many of them have a proprietary interest. The retailers differ very much as to their stock in

trade according to locality; and in that part of their business which is pushed into prominence we know of but few who follow what from the medical, professional stand-point would be considered in accordance with the rules of ethics. Under the elegant college-building of one of the best of American medical schools, the pharmacist posts his own cure for neuralgia, and prepares his own specifics for diarrhœa, and is at the same time well patronized by the profession with prescriptions.

As the word from which "apothecary" is derived, means a repository or storehouse, and as "drug" is a general name for commodities employed for the purposes of medicine, dyeing, tanning, and other uses, these dealers may be excused for their employment of the terms, and may become general storekeepers. We do not object that honorable men should pursue as a legitimate business the sale of this combination of commodities, which the law permits. Our claim is that the preparation of medicines, and the dispensing of the same by physician's prescriptions, should not by the physician be tolerated under such an arrangement. Medical practice is demoralized by such association, and an incubus is thus put upon the real advancement of therapeutical knowledge among practitioners.

The only question before the wholesale dealer, is how to carry on his trade to the best pecuniary advantage. If he acquire any pharmaceutical knowledge at all, it is not in the interests of legitimate practice, but that he may associate this with the other branches of the trade. We know of a leading house which pays a practising physician to acquaint it with the tastes of the profession, and to inform it of what is most likely to please. The laxness of the retailers, if not quite so outspoken, is often no less real. Prescribing pharmacists abound. The prescriptions of physicians are used with freedom, and are repeated indefinitely without the knowledge of the prescriber. If in some stores the patent-medicine notices are not so prominent, the article is nevertheless kept in stock, and large sales of it are made.

We are aware that there are three classes of pharmacists: Those of the first, and by far the most numerous class, do all in their power to generalize their business, having not the least regard for the physician, except as a customer, and availing themselves with equal suavity of Jacob Townsend and of Austin Flint. Those of the second class desire the patronage of the profession more than they do a general business; they have a faint perception of the higher sphere of therapeutics, and so foster a prescription business, while feeling themselves necessitated to keep all things required in the general trade. We theoretically add a third class, of those who deal only in pharmaceutical preparations, and who dispense medicines within such bounds as a profession can recognize. But we add this class simply because many of the best of our pharmacists wish that it existed, and that they belonged to it. It is quite plain that while deploring the present state of things they do not see any way to improve it.

At the last meeting of the American Pharmaceutical Association, Professor Maisch spoke of the immense traffic in patent medicines. The Association professed to be opposed to the sale of these articles, but it is added "the efforts of pharmacists in this direction would amount to nothing, as long as patent medicines are called for by the public." Nothing could more plainly show the present status of the business. We might just as well say that the physician is compelled to prescribe patent medicines because they are popular with the public. It is merely the forced admission that the pharmacist does each day that which is antagonistic to the principle of individual prescription for each case.

And we do not notice any gain in the adaptation of the drug business to professional ethics. A recent New York authority[1] says that in that city "the sign, 'Physician's prescriptions accurately prepared,' is flanked on all sides by cure-all pills, wonderful lotions, and infallible pain-killers." This is even more literally true in all the smaller cities and villages through the land. I have purposely examined some of the leading so-called pharmacies between New York and Philadelphia, and have found the evil magnified beyond expression. I do not suppose that it will ever be possible to break up these bric-a-brac shops, or prevent the mongrel sale. Public demand will cause a place of supply. Stores will ever continue to exist, at which, under the name of "apothecary," stationery and innocent toys, perfumery and cosmetics, soda water and liquors, blacking and garden-seeds, soaps and paints, window glass and putty, and all varieties of patent medicines will continue to be vended. Let the drug-grocer go on, and be recognized as such in his business, but let not such be the place to which we resort for the compounding of our prescriptions. If we give recognition to such establishments, we must not be critical because these bazaars are the points of departure from regularity in medical practice. Quasi-elixirs and counter-doctors' prescriptions are the inevitable results of such afflictions. Worse still, the public is educated into low views of our art, and among physicians themselves the relations of therapeutics to practice are distorted. Manifold preparations are smuggled into professional use, which would not get there under proper medical supervision, and such pharmacy is in turn the progeny and the progenitor of lax therapeutics.

We are not to expect that any radical reform will originate with the apothecaries themselves. Their relations to our profession being merely of a business kind, there are not sufficient motives for change amid the various mercantile interests pointing in other directions. We are told of late, on the best of authority,[2] that there is danger that the United States Pharmacopœia itself will go over "to the commercial interests of pharmacy." Prof. Remington, in his address before the Philadelphia College of Pharmacy, March 14, 1876, in view of the present degraded tendencies of pharmaceutists, says: "Do you regard your avocation as a profession, or as a mere business calling? . . . What shall it be? Profession or trade, pharmacy or black art, pharmacopœia or price current?" The very character of the business decides that question for us, and decides too its reflex influence on medical science and practice. Definite and damaging results must flow to us from such untoward channels of communication with the populace. It is not surprising that the vender of patent medicines should himself become a compounder and prescriber, and that those whom we send for prescriptions should be beguiled by the beautiful signs, or influenced by the advertisements of nostrums, with which they meet.

For all this condition of things I hold the medical profession, in a great degree, responsible. I do not complain of the American druggist that, placed by the consent and really by the action of the medical profession outside its guild, he should accept the situation and assume his relations to that profession to be merely commercial. I will not blame the College of Pharmacy of the city of New York,[3] whose business it is to educate successful venders of drugs, that its Code of Ethics says that the "necessities of the times" warrant the keeping of patent medicines, although

[1] Medical Record. [2] E. R. Squibb, Ibid., July, 1876, p. 691.
[3] Ibid., Oct. 30, 1875.

it declares their sale to be "not a legitimate part of our business custom." But I do complain that our profession, by complicity with this system, aids and abets a calling which, by its very construction and methods of practice, compromises that profession, militates against scientific medicine, lowers our own estimate of therapeutics, and tends to remove the care of our curative forces outside the domain of our professional jurisdiction. The medical profession in New York City regards it as so improper for a licensed practitioner to carry on the business of a retail druggist, that it excludes such an one from medical societies, and will not place his name in its register. If a physician cannot keep a modern pharmacy, *as certainly he cannot* consistently with proper professional feeling, I do not quite see how he can suitably patronize one. It would be well if it were practicable for every physician to refuse to send prescriptions to be compounded by any save those duly licensed in medicine, and who, having chosen this as their specialty, were bound in ethical propriety not to prescribe, to sell and to compound only in recognized pharmacy, and to hold themselves in the same relative position as a part of our profession, as do others who pursue a particular branch.

If, to-day, ten pharmacists in such cities as New York and Philadelphia could be brought into this direct association with the profession, it would be a gain for pharmacy and therapeutics of the most reformatory and indisputable character. Such a relation, existing between one or more of our leading manufacturers and the medical profession, is an elevation of the whole domain of therapeutics, not because of their personal attainments merely, but because they are thus held thoroughly responsible to those high therapeutic demands which are alike in the real interests of each department. When we bring the same conditions to bear on the retail dispenser, we shall have our greatest security for the purity and adaptability of medicines. All hope of reform from outside is Utopian, because the organic relation is wrong, and must first of all be righted.

A method of this kind needs only to be initiated by such a medical school as that of Harvard, by such a State Society as that of New York, or by such a school of pharmacy as that of Philadelphia, in order to make of it an ultimate success. Enough of the better class of physicians would rally to the support of those of their own number who should thus thoroughly identify themselves with the interests of the profession in one of its specialties. Many a pharmacist, sighing for deliverance from his present mongrel trade, would gladly accept his place in our profession, and in this branch of it would contribute to its welfare. All chemical and microscopical examinations, and various similar aids which the general practitioner now seeks and pays for, might naturally be committed to this branch of the profession, as well as much that is now appropriated by the instrument-maker. Place the dispensary in its natural relation, in charge of a member of the medical profession by choice assigned to that department, and society would be shielded from exposure to irresponsible remedies, and from the temptation to self-medication to which, by the present system, it is constantly and cordially invited.

The pharmacist, within the fold where he legitimately belonged, would have the status demanded by therapeutics, and would thereby become directly interested with other physicians in the advance of our art. There would be hosts of apothecaries on the outskirts, just as now there are hosts of irregular practitioners on the outskirts of the legitimate profession, but they would recognize themselves as belonging to the open

frontier and the outside common. The line of demarcation would be so pronounced, and the professional recognition so honorable, that the real pharmacist and the real physician would alike rejoice. Therapeutics, adjusting the relation between diagnosis and cure, has the right to claim such a union. It already takes every one of us by the hand, and shows us that it is not meet that we should divorce from us that which forms an integral part of our profession, and which is incident to our very life-success. Instead of leaving the pharmacist outside, let us invite him within the boundaries of legitimate practice, and let him become imbued with the *esprit de corps* which belongs to a noble profession.

I am importunate in this regard, because I feel that the present method of supply, or the medium of communication between ourselves and our patients, as far as remedial agency is concerned, is essentially defective. If it were a mere criticism as to feasibility of method, or as to expediency of convenient or financial management, it could be passed by; but the whole success of treatment, and therefore the welfare of both patient and practitioner, are alike involved. The medicine or thing prescribed is the result and means of conveyance of expressed skill—the deliberate verdict on a case more or less critical. It is entitled to the most assuring safeguard which the profession itself can furnish. This safeguard cannot be furnished by adjunct relations. We cannot afford to leave the work of preparing our remedies to the manipulation of an outside corps. Identify pharmacy as fully with the profession as any other legitimately followed medical specialty, and we at once throw around it a *cordon* of protection and give an assurance of reform. Such vastness of uncertainty as is at present distributed from the drug-shops of our land, and very much under our prescribing, must be abolished; and this can only be done by reinstating pharmacy as part and parcel of our calling.

This is peculiarly essential to medical progress, because for the next quarter of a century the most crucial questions of practical medicine will centre in therapeutics. The two great preliminaries for our progress are (1) to have physicians record clinical and experimental results most exactly, and to tabulate in extended classifications the symptoms of disease, and (2) so to acquaint us with the physiological effects of medicines, and their effects in the various kinds and stages of disease, as to enable us in like manner to tabulate remedies. To do this, we must be assured of the precise quality, quantity, and precision of combination, of articles used, so that sources of error shall be indisputably eliminated. All of us who have examined and compared various specimens of medicines, know how much this means, and how much uncertainty is introduced into all chemical records by unreliable preparations. Never shall we be able to exclude this disturbing factor, confusing all statistics, until the prevalent system of dispensing medicines is changed, and until the pharmacist takes his place as a member of our profession, glad to be held strictly responsible to its ethics, and finding it to his interest to practise his specialty in the interests of every other department.

The following conclusions are offered as being warranted by what has gone before:—

I. The interests of society and of the medical profession render it desirable that the furnishing of medicines should be surrounded with greater safeguards.

II. There are reasons why pharmacy should be regarded as a specialty within the general bounds of the medical profession.

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THE MICROSCOPIC STUDY OF THE BRAIN.

BY

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The microscopic examination of the tissues of the brain has added so much that is new to the departments of anatomy, physiology, and pathology, that each demands more time for its consideration than can be given to it now. The microscopic pathology of the brain, as found in insanity, having engaged my attention more particularly, I shall devote myself wholly to this department of the subject.

Changes in the Meninges.—Insanity, in its various forms, is found to involve all the cerebral tissues. Of the membranes investing the cerebral mass, the dura mater is the least liable to general histological change, general paresis being the only disease in which it is found to be uniformly altered. The alterations consist of thickening of the tissue, the increase in thickness depending upon the formation of a new membrane, apparently due to a disease of the epithelium lining the dura mater; new vessels are formed in the membrane, generally tortuous and irregular. In the more advanced stages of the disease, minute hemorrhagic points are found scattered over the under surface, depending upon rupture of some of the most tender capillaries. This state of the membrane must be sought for particularly, as no great departure from the normal standard is observed without the aid of a lens, the most characteristic change being the presence of a delicate, pink tinge, not found ordinarily in the normal state. Transverse sections of the tissue, carefully prepared, will show this condition very perfectly; or the new membrane may be torn from the dura mater, though it cannot then be so well studied. The large hemorrhagic clots found sometimes over the brain surface, in general paresis, occur between the dura mater and this new membrane, the latter

separating the clot from the subjacent tissues, and forming a delicate film which, being stained by the blood, is often overlooked, and is removed when the membrane is torn away from the clot. Not unfrequently the dura mater is found thickened in localized patches in other forms of mental alienation, the increase of thickness appearing then to depend upon an inflammatory condition which does not differ essentially in appearance from that found in ordinary inflammation.

The arachnoid and pia mater are nearly always found diseased in all forms of insanity, the most prominent changes being found in the vascular system, and consisting of deviations in the course, size, and structure of the capillaries. The membranes are usually semi-opaque, and in some cases thickened, and in thickening of the arachnoid, granules and the products of inflammation are found upon its external surface. The deviations in the course of the vessels, consist of increased tortuosity; the deviations in size, of aneurismal enlargement; and the deviations in structure, of hypertrophy and degenerative processes.

In removing the pia mater from the convolutions, before the brain has been placed in any hardening agent, portions of the external layer of gray matter often adhere to the membrane, and patches of gray matter, varying in size from that of a pin's head to the entire width of a convolution, may be removed with it. In the parts so removed, it is common to find changes of structure, having their origin in abnormal deposits on the vessels, in minute hemorrhages, dependent upon rupture of vessels, or in passive exudation, preparing the way for inflammatory changes, and for softening and destruction of adjacent nerve-tissue.

The pia mater is composed of two layers, and in diseased states these layers are easily demonstrated. Occasionally we find a hemorrhagic point between the layers, in which case the layer on each side becomes thickened by inflammatory changes; the thickening of the membrane arrests the circulation in the contiguous parts, sometimes completely shutting off the blood-supply to that part of the convolution which is covered by the membrane, and, the twigs conveying the blood to the external layers of gray matter being first closed, causes local softening of the gray matter. The change here alluded to is often found in cases of general paresis, although it is also found in other forms of mental disease of an acute type. When the hemorrhage occurs between the pia mater and the convolution, secondary changes do not appear to follow so frequently. Occasionally, changes occur in the pia mater which do not depend on hemorrhagic points; the simple thickening of the membrane from inflammatory processes may cut off the blood-supply, and necro-biosis of the tissue beneath then results.

New material is sometimes found on the external layer of the pia mater, increasing the thickness of the membrane three or fourfold, and being always the result of inflammatory processes. It is rare to find extensive lesions in either the arachnoid or pia mater, without both membranes being involved, though the pia mater is probably the first affected; this is especially noticeable when pus has been formed, the pia mater being then seriously diseased and thickened by deposits of plastic material upon its surface.

Bloodvessels.—The most constant lesions observed in the cerebral tissue of those who die insane, are connected with the circulatory apparatus; and the first of these to be described, because probably laying the foundation for other lesions, is engorgement of the arterioles and capillaries. It will be remembered that the coats of the cerebral vessels are in their

normal state more inelastic than those of other vessels, and that their lumen is considerably larger than that of vessels of the same size elsewhere. In the earlier stages of insanity, there is often fulness of the cerebral vessels, resulting either from rapidity of the heart's action, which forcibly dilates the capillaries for a considerable length of time, or from stasis of blood, depending upon disease of the vaso-motor apparatus. If this fulness is relieved in a short time, the capillary resumes its ordinary calibre, but the second distension is more easily effected than the first, and the vessel is then less liable to resume its normal size. The process, if frequently repeated, leaves the capillary in a state of permanent distension. The vessel is then irregular in form, being larger above a bifurcation than below, and presents the appearance of having been stuffed. If the engorgement has been of long standing, changes in the coats of the vessel and in the adjoining brain-tissue will be seen. The change which occurs in the vessels is a fatty degeneration of the muscular coats, or of the cells constituting the walls of the finer capillaries.

The first change noticed in the coats of the larger vessels is the collection of numbers of fine granules, apparently lying on the wall of the vessel, or developed within its muscular coat; these are grouped together, the granular deposit varying in size according to the stage of the degenerative process. In the walls of the smaller capillaries, each cell maintains its contour at first, but is slightly increased in size. Granules are then formed near the nucleus, gradually encroaching upon the plasma of the cell, which in time becomes a mass of granules; the nucleus becomes involved in the same granular change, and finally disappears. In the arterioles the degenerative process continues until the evidences of muscular fibre disappear, and till the vessel looks like a mass of granular material, through which its outline can sometimes be dimly traced. A remarkable instance of infarction and secondary change, illustrating the fact that these conditions may occur *in utero*, was found in the brain of an infant who died in an epileptic convulsion immediately after birth. The mother had for years been epileptic, and during pregnancy had had repeated epileptic seizures, followed by paroxysms of violence. In the third left anterior convolution of this infant's brain, cysts were found, with fusiform dilatations of the vessels, and fatty degeneration of the walls and cells of the vessels and capillaries. One vessel occupied a place in a large cyst, wherein abundant evidences of new material, the product of inflammation, existed, and where absorption must have occurred.

A change which appears to depend upon over distension and subsequent relaxation is tortuosity and kinking; the deviation in direction ranges from a simple wavy course to the formation of a complete loop, as though a knot had been tied in the vessel. The tortuous vessel is not confined to any particular form of mental alienation, but exists in all forms in which distension and subsequent relaxation have occurred, and has been noted in acute mania, melancholia, dementia, and general paresis. I have not found the tortuous vessels in the brains of the lower animals, nor in the cerebral tissue of those who have died sane, except in the brain of a man executed for murder, and who had been insane some years before.

A condition that has given rise to considerable discussion in the department of cerebral pathology, is the existence of what have been called the "perivascular canals," in which the bloodvessels are found, and the "hyaline membrane" or "perivascular sheath," which surrounds the

vessel. The canal and sheath are not easily demonstrated in the normal brain, but in the diseased tissue they are readily observed. The membrane or sheath, as it is called, does not always envelop the vessel closely, but sometimes hangs loosely about it, and, where the vessel bifurcates, forms a triangular sac. Sometimes the sheath is found closely applied to the brain-tissue, this condition occurring where there has been excessive engorgement a short time prior to death, the sheath having been distended, and the brain-tissue crowded back. After death, the vessel recedes somewhat, leaving the membrane attached to the nerve-tissue, and the vessel itself adhering to one portion of the membrane; transverse sections show the vessel apparently lying in a tunnel, which is composed of the enveloping sheath applied to the wall of nerve-tissue. The vessel, partially distended with blood, is found in the canal, and tortuous vessels are sometimes found lying in comparatively straight canals.

The various conditions of the perivascular canal are best studied in fresh specimens, as we can thus acquire a better idea of the parts as they actually exist, all hardening agents causing some contraction of the tissue. Injected specimens, especially those of diseased cerebral tissue, will aid materially in studying these interesting conditions, but for tracing the ramifications of the canal, the hardened tissue answers best.

Lymph cells are found in the canal, in the fresh tissue, while smaller granular bodies glide over each other as pressure is made on the covering glass; it is the opinion of some Continental observers that these canals convey the products of waste from the cerebral tissue into a lymph space in the pia mater, an opinion which appears to be based upon good reasons, for the perivascular canal can be traced into the space between the layers of the pia mater. In all cases in which engorgement has existed for some time, crystals of hæmatoidine are found both upon the walls of the vessel, and also in the perivascular sheath. This deposit varies considerably in extent, sometimes occurring only at the bifurcation of the vessels (where it is always most abundant), and at other times completely incrusting the coat of the vessel, and thickly deposited upon the sheath. Another deposit which is found upon the vessel, much smaller than the crystals of hæmatoidine, appears as minute specks of a fatty character, varying in size from $\frac{1}{10000}$ to $\frac{1}{3000}$ of an inch in diameter; these are yellowish-brown in color, and under high powers appear crystalline, but they are not easily dissolved, nor are they affected in any way by staining fluids.

As a result of the excessive engorgement of the vessels, we have fusiform enlargements and miliary aneurisms. Fusiform enlargements appear to depend upon degeneration of the coats of the vessels, which sometimes rupture, when the blood is poured out into the perivascular canals, the hemorrhage being limited by the sheath; these limited hemorrhages are found only in the course of the smaller capillaries, where the force of the blood-current is not sufficient to burst the sheath after escaping from the vessel. This is not a frequent occurrence, but has been observed a sufficient number of times to make it noteworthy. These fusiform enlargements often extend to a bifurcation, the vessel again enlarging beyond until the next branch is reached, and so on to the finer capillaries. In all cases of fusiform enlargement, fatty metamorphosis, in varying degree, has been found. Miliary aneurisms are not as frequent as fusiform enlargements, but are more striking in appearance; they are larger, ranging from $\frac{1}{60}$ to $\frac{1}{25}$ of an inch in diameter,

and can sometimes be seen without the aid of a lens. The vessels upon which these aneurisms occur, do not appear to be as seriously diseased as those in which fusiform enlargements are found, but the aneurisms, too, depend upon morbid changes in the vessels' coats: degeneration of the walls (the *peri-arteritis* of Charcot), together with engorgement, appear to determine their existence in a certain number of cases, but other factors are concerned in their formation, as the aneurisms are not found in all cases of degeneration even though evidences of engorgement also exist. They are found in all parts of the brain-tissue, and in the membranes. I have found them in the gray matter of the spinal cord; in one notable case, the sac was situated near the origin of the first cervical nerve.

Hypertrophy and fatty degeneration of the capillaries are frequently noted in cases of protracted engorgement, this abnormal condition at times existing to such an extent as to destroy the vessel, the mass forming a nidus around which changes in the structure of the nerve-tissue occur. Occlusion of the capillaries sometimes results from stasis following engorgement, the minute thrombi being formed when the circulation is impeded. If the circulation of a bat's wing be arrested for a time, certain of the blood-corpuscles are seen to adhere together, forming minute thrombi. The agglomerated corpuscles are moved along with the current until they pass into a vessel of small calibre, where they finally lodge, generally at a bifurcation. The blood beyond the thrombus in the plugged capillary becomes stationary, and a clot results, secondary degenerative changes being then induced. In one case of acute mania, recently examined, these minute thrombi were found in great numbers, completely depriving large areas of the brain of their blood supply. The vessels beyond the plug are in some cases found empty, but in others distended with coagula, the adjacent brain-tissue being deeply stained with the coloring matter of the blood. When we recall for a moment the great vascularity of the brain, the effect of closure of the vessels must be regarded as a fruitful source of abnormal cerebral action, independent of the results which follow as a consequence of the deprivation of nutrition.

In cases of insanity of long duration, or where there has been serious disorganization of brain-tissue, a growth sometimes appears near the capillaries, projecting beyond the sheath into the brain-tissue; the growth is ovoid, one end being connected to the capillary. These growths are sometimes arranged in pairs, six or eight deep, the largest pair being at one end of the mass, and each pair being smaller than the preceding. They sometimes appear to surround the vessel, the outline of which appears distinctly in the mass. The edges of these growths are not sharp, and they do not have an investing membrane. The mass is granular, of a grayish-white color, and is not affected by carmine staining. There are no nuclei apparent, and although the nerve-tissue is destroyed wherever these growths occur, none of the *débris* caused by destruction of nerve-tissue can be found in or about them. They are not fatty, and there are no crystals of hæmatoidine upon them, but these may be seen in abundance upon the vessel. Gold and palladium staining color the growths slightly; osmic acid also colors them, but not as deeply as it does the surrounding tissue. This microscopic mass is to be distinguished from amyloid degeneration, and from the deposits found in syphilitic cases. The fact that these bodies have been found in the vicinity of capillaries, or in the course of obliterated vessels, has led me to infer that they are

dependent upon lesions of the circulatory apparatus. I have not observed them near the larger cerebral vessels. They are not aneurismal, and do not appear to have any direct connection with the vascular system.

Miliary hemorrhages are found frequently in cases in which aneurisms do not exist, the amount of blood extravasated depending upon the size of the vessel. Miliary hemorrhages vary in size from one-eighth to one-thirtieth of an inch, or even smaller. The hemorrhage is usually limited, but occasionally it involves large areas and produces characteristic symptoms. One point in connection with hemorrhage, not due to the rupture of aneurismal sacs, I have not seen noticed by any writer, and that is the tendency to extravasation of blood into the gray matter. This form of bleeding I have usually found confined to the posterior convolutions. A section through a convolution thus involved, shows that all the vessels are more or less diseased, and that the hemorrhage has taken place from all of the vessels in the vicinity, and apparently at about the same time; the smaller capillaries show small clots, which may at first be mistaken for aneurisms, but careful inspection will reveal the difference.

In these cases it is quite difficult to determine just where the capillary is ruptured, the closest scrutiny with the highest powers of the microscope sometimes failing to disclose the opening. It may be that the degenerated vessels permit a passive hemorrhage, the blood slowly transuding through the softened walls. This form of bleeding is not infrequent; I have found it in all forms of insanity consequent upon engorgement, but especially in those cases of dementia which some writers have denominated "acute." The hemorrhage is not always confined exclusively to the cortical substance of the brain, but is much more frequently found there than elsewhere, and is more extensive than in the white matter. The results are the same as in other forms of hemorrhage; absorption slowly occurs, and cysts are formed, or local softening takes place. Nearly always the remains of a vessel may be found in or near the cyst. In the convolutions of the brain of the infant, already alluded to, numerous cysts were found, in some of which the vessels were apparently intact, but enveloped with granular *débris*. The microscopic appearances of the circulatory apparatus indicate that in a certain proportion of cases the initiatory processes of disease have their origin in the capillaries or arterioles.

Nerve-fibres.—The nerve-fibres undergo changes which must seriously interfere with their normal functions, independently of the diseased tissues around them. The most common changes are hypertrophy and atrophy. Hypertrophy, though not frequent, is found in certain cases, and appears to depend upon increased growth of all the parts of the fibre. The tubular membrane, or sheath of Schwann, grows more rapidly than the other tissues, and is proportionately thicker than in its normal state. The fibres must be examined as soon after death as possible, and should not be placed in any fluid, as the outlines change rapidly after being immersed. In hypertrophied fibres, the tubular membrane sometimes appears to be formed of several delicate layers of tissue, presenting a wavy appearance. The myeline is increased in quantity, and is granular. The axis-cylinder contrasts strongly with the dark tubular membrane, and presents a molecular appearance.

The most remarkable hypertrophy of nerve-fibres that I have ever seen, was found in the cerebral tissue of a person aged forty-seven, who had been maniacal for a year. The brain tissues, generally, were diseased, the vascular system presenting the enlarged appearances already de-

scribed, and the coats of the capillaries having in many places undergone fatty changes. Twenty measurements of nerve-fibres were made soon after the patient's death, giving results as follows: one field contained a number of fibres $\frac{1}{200}$ of an inch in diameter; several fields showed fibres of an average diameter of $\frac{1}{250}$ of an inch; others again of $\frac{1}{330}$ of an inch; while in one instance the entire field was covered with fibres ranging from $\frac{1}{166}$ to $\frac{1}{530}$ of an inch. The largest fibres were found arranged in parallel bands. The same group of fibres, measured at different places, gave the same general results, the fibres ranging from $\frac{1}{530}$ to $\frac{1}{166}$ of an inch in diameter. The portions measured were taken from different parts of the brain, and there was no great difference observed in the several fields as to size. Atrophy of the nerve-fibres from pressure of adjacent growths, or from prolonged capillary engorgement, is of common occurrence in cases of chronic insanity, particularly in elderly people; so considerable is this atrophy, at times, that it is difficult to distinguish the tubular membrane, nothing but the axis-cylinder being clearly marked.

Changes in the Neuroglia.—Since Virchow first demonstrated the neuroglia, several forms of disease have been described which are attributed to morbid changes of this tissue. The most common form of change has been called sclerosis, to which name several affixes have been given; thus there are disseminated sclerosis, multiple cerebral sclerosis, and insular sclerosis. The varieties above enumerated can be seen without the aid of a lens, and they are believed to depend upon an increased growth of the neuroglia. This hyperplasia of the connective tissue, as it is sometimes called, plays an important part in several forms of nervous disease not necessarily involving the mental status of the individual affected.

Certain changes in the appearance of the cerebral tissues, which have recently been brought to the attention of neuro-pathologists, have been described under the name of Miliary Sclerosis. In this form of disease, it is usual to find, scattered about the field of the microscope, a number of white spots, situated in the white matter of the convolution. These spots are not stained by carmine, and contrast sharply with the surrounding tinted tissue; they vary in size from $\frac{1}{200}$ to $\frac{1}{35}$ of an inch in diameter; they are generally round, with well-defined margins; and the adjacent tissue has the appearance of having been pushed back, as though the new growth had formed rapidly. These spots are granular in appearance, and on some, connective-tissue fibres can be seen. They are found in the white matter of the convolutions, in the corpora striata, the optic thalami, the medulla oblongata, and the spinal cord; some are found between the gray and the white matter, and some in the gray matter itself. The nerve-fibres appear to bend around the spots in some places, or they may terminate in them. This is particularly noticeable in the medulla oblongata, where it is common to find the nerve-fibres either bending around, or frayed out in, the spots. Occasionally one of the spots forms about a capillary, which it destroys, in and beyond the mass. Some of the spots are ovoid in form, and, when so shaped, the long diameter corresponds to the axis of the nerve-fibres; again they are sometimes found lobulated, the lobes generally being of unequal size, but apparently developed from a central point; some of the spots have a delicate greenish tinge, and this tint is the same whether the tissue has been previously hardened in spirits, or in other agents.

These spots have been found in the fresh tissue; in one case, an entire

section of the medulla, made soon after death, showed the tissue studded with them; and subsequent sections made from the medulla, after it had been hardened, showed the same general appearance as in the fresh state. The spots are best studied, however, in hardened tissue that has been stained with carmine or with chloride of gold; when the latter substance is used, the spots are slightly colored, but not uniformly stained to the centre, except in the smallest masses; chloride of palladium colors them brown, and a solution of osmic acid turns them dark brown or black. When placed in a solution of osmic acid, they can be seen to change color immediately, before the adjacent nerve-tissue changes; three or four minutes' immersion in a one per-cent. solution suffices to render them black, and when thus stained the spots appear more granular than when stained with other agents.

There are several varieties of this form of degeneration, each one presenting distinct characteristics; sometimes the spots are decidedly fibrous in appearance, the fine stroma of delicate fibres being distinctly visible when examined with a one-tenth objective. In other cases they are amorphous, or but slightly granular, and with no apparent nucleus. The margins of some of the smaller granular spots are clear and distinct, while the margins of the larger spots in the same section may be irregular and apparently interlaced with the surrounding tissue. In the fibrous masses, the fibres do not run in one direction, but interlace with each other at all angles. The fibrous spots occur sometimes in tracts affected with gray degeneration, which would seem to indicate that the two forms of disease are separate and distinct. In one well-marked case of gray degeneration, affecting the medulla, pons, and cord, in an epileptic, the degenerated masses were found in several sections to be situated directly in the tissues affected by the gray degeneration. It was observed that where sclerosis did exist, the spot was distinctly fibrous; where sclerosis did not exist, the spot was not fibrous, but presented the slightly granular aspect already described. In the case alluded to there were large patches, $\frac{1}{20}$ inch in diameter, in the sclerous portion, while in the tissue not affected by gray degeneration there were granular masses $\frac{1}{1000}$ inch in diameter; some of these smaller patches were lobulated, and had well-defined edges. As above stated, none of the masses had nuclei, and no characteristic granular spot could be discovered which could be regarded as a nucleus; nor could any of the detritus of broken-down nerve-tissue be found in the masses, excepting where sclerosis or gray degeneration had previously existed. No trace of resemblance could be observed between the diseased spots and the normal tissue, even in the most recent cases, and before chemical agents had been employed to prepare the tissue for preservation.

Another form of disease, found quite frequently in certain cases, has been called Colloid Degeneration; the name does not appear satisfactory, for, except in color, the diseased parts do not resemble colloid growths, nor do they respond to the reagents which are known to affect colloid tissue. The so-called "colloid growths" of the brain are much smaller than the masses above described, ranging from $\frac{1}{500}$ to $\frac{1}{4000}$ of an inch in diameter; they are more numerous than the patches of miliary sclerosis, have well-defined margins, and are more even in appearance and smoother than the miliary spots. They occur altogether in the white matter, which in certain places is filled with them.

In cases of insanity complicated with syphilis, growths of another form have been noticed; these are as large as the miliary spots, but

not as distinct, the margins shading off into the surrounding tissue. They are more decidedly granular, or rather cellular, in structure, the cells having nuclei, and there being a fibrillar stroma. Still another form of growth has a distinct margin, but the mass appears to be surrounded by a dense membrane, arranged in layers, the interior of the spot appearing smooth and slightly molecular, but without a nucleus.

Five distinct forms of growth have been recognized, and subjected to such tests as experience has dictated, each presenting peculiar and distinct reactions with the agents employed, and each having an affinity for certain special staining materials; though osmic acid, in varying degrees of intensity, stained all the sections subjected to it, with such differences, however, as to leave little doubt that they were unlike each other in structure. It should be stated that these tissues were subjected to tests for cerebrin and lecithin, and to the most delicate tests for albuminates; and that the substance which the larger spots (miliary sclerosis) most resembled, and of which the test gave the most decided, characteristic reaction, was cerebrin; but that the reactions noted at the time were not sufficiently certain to warrant the assumption that this material entered largely into their composition. The smaller masses did not give the same reaction, and we are left without certain knowledge as to their true structure. It should also be stated that comparative examinations have been made of the central nerve-structure of the lower animals, as well as of the central nerve-tissue of persons who died sane, and who were suddenly deprived of life by accident. In nearly every instance, the tissues were placed, part in alcohol and part in chromic acid, or other hardening agent, but the results were substantially the same as far as the diseased portions were concerned.

The masses of degeneration are found most frequently in the anterior portions of the cerebrum, in the white matter of the convolutions; next in the medulla, pons, and cord; and less frequently in the cerebellum. In some cases, they are most numerous in the upper part of the medulla, and it has been observed that while they are sometimes very numerous about the region of the olivary bodies, there are comparatively few in the olivary bodies themselves. These masses are frequently found in the lateral columns of the cord, sometimes encroaching upon and almost obliterating the central canal, for a considerable distance; and in these cases the epithelium lining the canal is altered in structure, and oftentimes its outline cannot be seen. In attempting to follow the course of nerve-roots, these spots are sometimes found encroaching upon them, fraying out the fibres, and destroying the continuity of the root itself. In cases in which the roots of the nerves of special sense are seriously interfered with, hallucinations referable to the nerves affected are observed among the symptoms during life.

Nerve-cells of the Cortex.—The nerve-cells are subject to changes in structure and form, being in some cases hypertrophied, in others atrophied, and in others again in some stage of granular degeneration, or of pigmentation. Changes in the nerve-cells are quite constant. There is not a particular change for each form of mental disturbance, nor do I regard this as important, but the change in the structure of the cells is to my mind of the highest significance. While it has not been shown that the nerve-cells are directly concerned in mental processes, yet the majority of writers consider that intellectual vigor is dependent upon the normal state of the nerve-cells, and that these are immediately concerned in all our mental operations; and when it is found, in a large

number of cases, that degeneration of the nerve-cells is concomitant with impairment of intellect, it is fair to assume that mental integrity depends upon their being in a normal state. Granular changes in the cells are the most frequent in all forms of insanity, the degree of disorganization appearing to depend to some extent upon the violence of the disease; the most complete metamorphosis of cell-structure is sometimes found in acute cases which have run a rapid course. This form of degeneration has been denominated fatty, but the cells do not always show the reactions incident to this condition; the chemistry of the subject is, however, as yet crude, and the tests employed cannot be relied upon when such delicate tissues as nerve-cells are involved.

In the initial stage of granular degeneration, a few yellowish specks appear scattered about in the substance of the cell, at first not affecting the nucleus. These specks increase in number as the change progresses, until the entire body of the cell, including the nucleus, becomes involved, and the cell-wall partakes of the same change; ultimately the cell loses its contour, and is converted into a mass of granules, the polar prolongations having previously become atrophied and indistinct. In atrophy, the whole cell appears shrivelled, and its investing membrane is shrunken. The poles become considerably shorter, and appear to lose some of their substance, and the whole cell and its appendages seem to be surrounded by a clear space. This atrophic change sometimes continues until the cells lose their characteristic appearance, and the poles disappear. The large pyramidal cells are sometimes found in a condition of hypertrophy, but it is not a frequent occurrence, nor is it usual to find hypertrophied cells in all the convolutions; they appear most frequently in the parietal convolutions, and seem to depend upon some local cause. The atrophic and granular changes are nearly always general, as far as the pyramidal cells are concerned. These large cells are often seriously affected, while the smaller cells do not present the same form of change in so distinct a manner. In cases of dementia of long standing, the large pyramidal cells are affected by the granular change, more generally, I think, than in any other distinct type of mental disease. In general paresis, the large cells are also granular, but are less shrivelled, and contain pigment granules. When the nutrition of the brain-tissue has been interfered with by thrombi plugging the capillaries, the nerve-cells in the parts deprived of blood have been found atrophied and granular, and in these cases the lesion is localized, appearing only in those parts of the convolutions which are deprived of nutriment. That the same form of degeneration may be found in mania and melancholia, for instance, does not militate against the inference that both forms of alienation may arise from the same cause, the external expressions of the disease depending upon some other circumstances. Whatever may be the function of the cells, it cannot be properly performed when they are affected by a destructive process which transforms a part or all of their substance into a material so wholly unlike the normal structure that the characteristic features of the cell are entirely lost, and its attachments to the surrounding nerve-fibres severed.

That part of our subject which relates to the pathological states which are characteristic of certain forms of insanity, must be approached with caution, as it is yet a comparatively new field; but it is the practical result which is expected to follow from a study of the morbid conditions of the brain, and it must eventually play an important part in deter-

mining the therapeutics of mental diseases. To illustrate this subject, I have selected typical cases from the well-pronounced forms of insanity, and will briefly describe the pathological conditions found in each.

Acute Mania.—In one case of acute mania, death having taken place on the tenth day after the earliest manifestations of insanity, the capillaries were found in the first stage of fatty degeneration; the granules were situated along the inner walls of the vessels, and also between the vessels and their sheaths; and many of the capillaries which were undergoing this change were found plugged by minute thrombi. The perivascular canals were not seriously dilated, and the adjacent brain-tissue had not undergone atrophic change. The nerve-fibres were not impaired; small spots of degeneration were seen, but they were few in number, not more than six or eight being seen in a section; they were $\frac{1}{3000}$ of an inch in diameter, and were molecular, but with no nuclei; the margins were distinct. Sections stained with carmine presented the same general appearance; the perivascular sheaths were more distinct, and the contrast between the patches of degeneration and the nerve-tissue was greater; the patches were slightly tinged with the coloring matter. The pyramidal cells were clearly defined, and larger than in the normal state; in these enlarged cells fine molecular material was found in the plasma. In the parts of the convolution which had been deprived of blood by thrombi, the cells were more seriously impaired, the molecular matter in the body of the cells being more abundant, and some of the cells being atrophied.

In a case of puerperal mania, in which death occurred six weeks from the first manifestation of the disease, the pyramidal cells were found slightly enlarged, and molecular material was deposited in the plasma. The cells were not well stained, apparently repelling the coloring fluid, and the outlines were a little indistinct. The capillaries were undergoing the first stage of fatty change, and many were found plugged; there was also slight engorgement. The white matter was studded with spots of degeneration, ranging from $\frac{1}{4000}$ to $\frac{1}{3000}$ of an inch in diameter; these spots had no nuclei, and were not stained by the carmine.

In a third case, in which the acute symptoms were prominent for six months before death, the changes were much more evident. The pyramidal nerve-cells were decidedly granular and atrophied, the walls appearing shrivelled, and the poles having undergone atrophic change; the masses of degeneration were larger, ranging from $\frac{1}{1500}$ to $\frac{1}{500}$ of an inch in diameter; they were characterized by a distinct margin, and were round, while their structure was molecular. Their cut surface appeared concave; they were not stained by carmine, and presented a yellow tinge. Some large masses were found in the cortex; these were $\frac{1}{65}$ of an inch in diameter, and were lobulated, and without nuclei; they were unstained, and were found near capillaries.

Melancholia.—In typical cases of melancholia of short duration, the lesions observed in the early stages were the appearance of fatty granules in the walls of the vessels, with slight hypertrophy of the muscular coats, and some irregularity in the course of the affected vessels. The perivascular sheaths were slightly distended, sufficiently so to show a departure from the normal state, and to indicate that at some period the vessels had been engorged. The masses of degeneration were confined to the white matter, and ranged from $\frac{1}{4000}$ to $\frac{1}{2000}$ of an inch in diameter; they were molecular and without nuclei. The pyramidal cells were cloudy, slightly granular, but with distinct nuclei; the polar prolongations were unchanged. A notable feature was that the cells were

surrounded by clear spaces, although the bodies of the cells were not apparently atrophied. These spaces were more distinctly marked in recent, than in prolonged, cases of melancholia, in which latter the capillaries were found engorged, tortuous, and unsymmetrical. The canals and sheaths were well shown, the former being much enlarged. Crystals of hæmatoidine were found in quantities about the bifurcations, and in many cases there were miliary hemorrhages. The neuroglia was proliferating, the extremely fine fibres appearing coarse, so that its ramifications could be easily followed. The pyramidal cells were distinctly granular, and some were much hypertrophied. The small spots of degeneration were found in large numbers, unstained by carmine, and without nuclei.

Chronic Mania.—The microscopic appearances of brain-tissue varied somewhat in these cases, but certain changes were quite constant. The evidences of engorgement were marked, the coats of the vessels being extensively hypertrophied, and the remains of vessels being outlined in parts of the tissue. The perivascular canals were large, often dilated to six or eight times the calibre of the vessels. The sheaths were fibrous, and large quantities of minute crystalline material were found deposited upon them, this being noted even upon some of the finer capillaries. Crystals of hæmatoidine were also found in greater abundance than in some acute forms of disease. The large pyramidal cells were extensively degenerated, the shrunken atrophied cells, and the granular masses into which the cells are sometimes changed, being quite common; the nuclei were less distinctly seen than in other forms of disease, and the polar prolongations were impaired. It was in cases of prolonged insanity that the hypertrophied nerve-fibres were found, although these were not common, atrophied nerve-fibres being more frequently met with. The masses of degeneration were large, often $\frac{1}{25}$ of an inch in diameter, and could easily be detected without a lens, in carmine-stained sections.

Dementia.—In cases of long standing dementia, intense engorgement of the cerebral vessels was a prominent feature. Fusiform enlargements, miliary aneurisms, and miliary hemorrhages, were all frequently observed in this form of disease. The nerve-tissue adjacent to the vessels was stained with the coloring matter of the blood, and numerous small cysts, indicative of old miliary hemorrhages, were found in the gray matter. In a case of dementia of long standing, miliary hemorrhages were abundant in the gray matter of the parietal convolutions, and the cysts of former hemorrhages were numerous. The nerve-cells were granular, and sometimes enlarged; the nuclei and nucleoli indistinct; and granular masses often occupied the positions of the nerve-cells. In these cases, the granular cells appeared to have undergone disintegration, the molecules spreading over a considerable space, and free oil-globules being found near.

General Paresis.—The cases of general paresis presented some features in common with those of dementia. The lesions in the membranes have already been described; well-marked cases had extensive fatty degeneration of the capillary walls, and tortuosity of the vessels; crystals of hæmatoidine were found in great abundance, and the minute crystalline granules encrusted the walls of the vessels and the perivascular sheaths. The nerve-cells were shrunken and granular; the nuclei indistinct, or completely obscured; and the polar prolongations not to be seen. There was a marked increase of connective-tissue elements. In the cortical substance, in several cases, a number of stellate, crystalline bodies were

found; these varied from $\frac{1}{6000}$ to $\frac{1}{5000}$ of an inch in diameter, and had distinct, black nuclei. The masses of degeneration were lobulated, and were not as numerous as in other forms of insanity; they were usually attached to the walls of vessels, which were shrunken, and were often found close to the gray matter. The nerve-fibres were atrophied, and the appearance of the tissue was indicative of active degenerative processes. In the spinal cord there were frequent departures from the normal state, involving the membranes, which were in many cases affected like the membranes of the brain; the nerve-fibres of the posterior columns of the cord were atrophied in the advanced stages, and there was an increased growth of connective tissue.

Insanity of Epilepsy.—In insanity following epilepsy, the nerve cells were found pigmented to some extent, particularly in the medulla and upper part of the spinal cord. The cells of the posterior convolutions of the brain were similarly affected, the bodies of the cells being pigmented and atrophied, while the prolongations were not so seriously affected. Small cysts were found in some cases, and masses of degeneration appeared in the medulla in great numbers, the masses being of the largest size, cellular in structure, and with a fine fibrillar matrix. Changes in the nerve-fibres of the spinal cord were not infrequent; gray degeneration in its various stages was quite common, and longitudinal sections showed atrophied nerve-fibres, and increased growth of connective tissue.

There are, of course, broad divergences from the above described conditions, the great range of diversity being in the various states of the neuroglia and the masses of degeneration. We know very little about the physiology of cerebral connective-tissue, and still less of the influences which affect its integrity. The spots may all be due to an abnormal state of this tissue, but up to this time I have not been able to demonstrate this point satisfactorily. It does not seem probable that the various changes can be due to different stages of the same disease. In the cerebral tissue in one case, there was gray degeneration of the columns of Turck; and in this degenerated tissue, patches of so-called miliary sclerosis were found, and, at its side, the smaller masses which have been described under the name of colloid degeneration. In the patches of gray degeneration, connective-tissue fibres were abundant; in the masses of sclerosis situated in the degenerated columns, the spots were composed of cells imbedded in a fibrous matrix; and in the adjoining tissue were found the small spots with distinct margins and molecular structure. The patches of gray degeneration were deeply stained with gold, the masses of sclerosis much less so, and the molecular spots not at all. In unstained sections, the differences were as marked as in those which were stained. The polariscope and tourmalines gave evidence that the formations were not all of the same structure, and aided materially in determining the differences of the several forms of disease; none of the spots, when under the polariscope, gave the black cross and ring of amyloid bodies.

From a study of these varying conditions, the conviction has forced itself upon me that the degenerations are not always the result of proliferation of the neuroglia, but that in some instances they follow slow inflammatory processes, resulting in degeneration of portions of the nerve-tissue, and causing atrophy of nerve-fibre by direct pressure. It is noteworthy that in all cases in which well-formed spots of degeneration were found, of whatever type, there was an inflammatory condition of the meninges, and an exudation of serous fluid into the sub-arachnoid space, and into the lateral ventricles.

These observations are based upon an examination of one hundred and fifteen cases, embodying all common forms of mental alienation; and while they do not cover the entire ground, the principle microscopic lesions observed have been given. It need hardly be said that one of the results of this investigation, has been to place me among the ranks of those who believe in the somatic origin of insanity; and, further, I believe that the only successful method of arriving at a definite knowledge of the pathology of insanity, is to be found in a study of the pathological histology of the cerebral tissues. We must appeal to the microscope for an explanation of those mysterious phenomena which, under the name of insanity, have so long baffled the Philosopher, Theologian, and Physician. The mysteries will yield to the careful student of Microscopic Pathology.

[Thirty-six photo-micrographs, illustrating the various pathological alterations of brain-tissue described in the paper, were then exhibited by means of the lantern.]

Discussion on Dr. Kempster's Paper.

After the reading of the preceding paper, Dr. Isaac Ray, of Philadelphia, said:—I cannot see a paper of so much importance pass entirely without note or comment. Dr. Kempster deserves great commendation for his labors on the microscopic anatomy of the brain, and there can be no doubt that in this direction is the way to successful inquiry into the pathology of insanity. In the case of two persons dying of mental disease under apparently very similar conditions, we may find in the brain of one every possible lesion, and in that of the other, nothing at all. It is somewhat the fashion to sneer at these microscopic investigations, but however indifferent we may consider the results so far, it can scarcely be denied that it is to them that we are to look for further light on this obscure subject. It is foolish to taunt us with our inability to find a delusion packed away in some congeries of cells, or to see a crazy project stamped at full length on a slice of gray matter, but, certainly, if we learn from the microscope something of the initiatory steps in the morbid process—if we are made sure that in every case there is a dilatation of some cells, or a plugging of some vessels—we have made a substantial advance in our knowledge. Very few of us, I suppose, feel competent to discuss such a paper as this, and all may not be prepared to accept the conclusions of the writer, but, none the less, it ought to receive our most careful study.

The President, Dr. John P. Gray, of Utica, said:—I am glad to hear the remarks of Dr. Ray on the value of the microscope in the study of insanity. In the institution at Utica, where I inaugurated this work in the asylums of this country, though we have been engaged in these investigations for a number of years, I consider that we are only in our infancy in regard to the development of this subject. I do not understand Dr. Kempster to claim that the lesions which he has mentioned are constant and peculiar to, or only found in, certain forms of insanity; and indeed such a claim could not be sustained. Winslow remarked, a long time ago, that insanity was due to a peculiar inflammatory process. A recent article in the British and Foreign Medico-Chirurgical Review, on the other hand, in referring to the investigations at Utica, said that we were no nearer to the discovery of the cause of insanity, or to an explanation of its phenomena, through these researches, than our predecessors were through their methods. And it is maintained by good authority that similar morbid changes are found in other diseases of the brain. What we are endeavoring to do by microscopic investigation, is to determine the exact character and extent of the morbid lesions in insanity, and we think that photographic illustrations and illuminated sections of tissues, such as Dr. Kempster has here thrown upon canvas, are valuable methods of demonstrating these lesions. And all this should tend to stimulate further investigation.

THE RESPONSIBILITY OF THE INSANE FOR THEIR CRIMINAL ACTS.

BY

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OF PHILADELPHIA.

How far insanity should be received in excuse for crime, is a question on which, as we all know, there still exists much diversity of opinion. Notwithstanding that the study of metaphysics has, of late years, engaged the attention of some of the profoundest thinkers of the time; notwithstanding that better opportunities for the study of insanity have been afforded by the modern practice of gathering its subjects into large receptacles for care and custody; and notwithstanding that cases for legal adjudication in the courts of justice have been increasing with remarkable rapidity, we are yet, apparently, as far as ever from well-settled conclusions respecting the exculpatory effects of mental disease. Among lawyers and among physicians; among men of science and culture as well as among men who pretend to neither; among men of extreme conservatism as well as among those who are never satisfied with the present—in each and all of these classes, we find, on this subject, most diverse and even opposite views. This we may be inclined to regard as unwarrantable under the circumstances, proceeding as it does from neglect or careless use of the information at our disposal, yet, considering the part which passion, prejudice, and illogical reasoning, have in the formation of all opinions, it is neither strange nor extraordinary.

In the history of English jurisprudence, insanity, as an excuse for crime, scarcely made its appearance until within a comparatively recent period. Towards the middle of the seventeenth century, it may have been recognized as such in some of its grosser forms. With that better understanding of mental phenomena, and that milder treatment of human infirmity, which began to be visible in the literature and science of a somewhat later period, insanity became an unquestionable element both of moral and of legal irresponsibility. Lord Hale undertook to define, with some show of precision, the exact extent of its influence upon the mental movements, and consequently of its effect in impairing the responsibility of its subjects. In this attempt he was guided, unquestionably, by the best medical opinions of his time, as he found them in books, and in the discourse of eminent members of the medical profession. As the result of his inquiries, he came to the conclusion that insanity in its milder forms was not necessarily incompatible with responsibility for crime, while he freely admitted that in its severest manifestations it was an ample excuse for any criminal acts that might be committed under its influence. And ever since, his successors on the English bench have been seeking for some tests whereby these two grades of the disease might be distinguished, the one from the other, but always with very indifferent success; although, moved by that spirit of humanity which has been steadily infusing itself into the English law, their instructions in par-

ticular cases have become more and more frequently correct. The tests which have been promulgated from time to time, not being founded on any correct knowledge of insanity, have each and all proved unsatisfactory when scrutinized under the light of practical observation; but, notwithstanding, they are still put forth occasionally as the law of the land. The discredit into which they have fallen, has somewhat, but not in a corresponding degree, widened the application of the plea of insanity in criminal cases, while it seems also to have given rise to some extent to the very opposite condition, the now prevalent distrust of the plea being supported by reasons which, though far from being new, have recently become endowed with a significance quite unwarranted by what we know of mental operations under the influence of disease. They are no less satisfactory, however, to that large class of people who having taken their opinions from one another, with but little knowledge of the subject itself, find it convenient to allege some plausible grounds for their belief.

In the light derived from a good deal of intimate intercourse with the insane, and from the study of various authors who have written on the subject, I propose to examine the reasons now most often given for withdrawing that exemption from punishment which, in a greater or less degree, the insane have hitherto experienced. I am not so sanguine as to suppose that any efforts of this kind will turn the current of opinion now setting against these unfortunate victims of disease, but the solitary inquirer, sincerely seeking for the truth, should not be obliged to seek in vain.

Much, if not most, of the present feeling on this subject may be traced to the legal profession, by which the plea of insanity has never been very cordially received. To lawyers, it seems like an intrusive element having no proper place in our legal system, but which, nevertheless, cannot be kept out of it. It implies, as we all know, a scientific fact to be examined, and its significance determined, by the rules and in the spirit of modern scientific investigation. This is a task completely outside of the domain of law. The lawyer has had no scientific training, and his knowledge of physical, and especially of medical, science, is only that which floats on the surface of the current thought. The more that is learned about insanity, the more difficult does he find it, for lack of such training, to determine its exact relations to crime. Even when told that the insane, generally, know right from wrong, what is lawful and what is unlawful, and recognize the distinction between vice and virtue, he is still unable to see what element of crime is wanting in their criminal acts. He looks at them, not from the scientific, but from the legal, or, more properly speaking, the metaphysical, point of view.

When a person commits a criminal act, knowing that crime is wrong and forbidden by the laws of God and man, incited to it, perhaps, by a rational motive—to gratify some passion, or to avenge some injury—evincing design and contrivance in the execution of it, the lawyer is constrained to believe, as a strictly logical sequence, that such a person should be punished. In such an act, not a single ingredient of crime, as he has studied his books, is wanting. A person, he says, who imagines that another, sitting opposite to him at table, is making faces at him, is led to this belief by some morbid condition of the perceptive organs, for which he is not to blame, certainly, but which furnishes no excuse for shooting his imaginary foe, because he knows as well as he ever did that killing or maiming another is wrong and unlawful. Besides, he acted deliberately, he had malice in his heart, and, as the old indictments used

to have it, he was moved by the evil instigation of the devil. What then was wanting to render him responsible? Nothing, certainly, if we leave out of the account those springs of action, no less real and potent, which are introduced into the mental mechanism by disease, and ignore entirely some faculties of the mind, as necessary for determining and regulating the conduct as those which are recognized. He can find no excuse in his philosophy for one who slays the dearest objects of his affection, while admitting the wrong and deploring the necessity which obliges him to do it, simply because he has but a faint apprehension of the mental faculties chiefly concerned in the act, and scarcely any apprehension at all of how they are affected by disease. In short, of the two classes of faculties or manifestations that make up the mind, one, considered as the subject of disease, is altogether ignored, while the operation of the other under stress of disease is greatly misunderstood.

And thus it is that in their theories respecting the responsibility of the insane, the lawyers, in their imperfect understanding of the nature and operations of the mind, commit two serious mistakes; one is that, where the mental affection is too obvious to be overlooked, they measure and define the scope of its influence in an arbitrary manner, unsupported entirely by the facts of psychological science; while the other, as just intimated, consists in regarding the affectional faculties—the sentiments, emotions, and passions—as without any part in the play of disease, or with so little that it may be safely ignored. The first mistake may be witnessed in the rule propounded many years ago by Hoffbauer, a German jurist of great repute, that an insane person may be excused for any act prompted by a delusion, only as far as the act would have been excusable had the belief been a reality instead of a delusion. That is to say, if an insane person imagined that another was approaching him with intent to take his life, he might kill him, and be excused on the plea of insanity; but if he only imagined that this person was making faces at him, or had been following him from one place to another, he should not be so excused, because had the offence been real instead of imaginary, it would not have justified the killing. Never has there occurred a more remarkable instance of this mistake than that presented by the English law-lords, in 1843, on the occasion of the trial of McNaughton, for killing the secretary of Sir Robert Peel. McNaughton was possessed by the delusion that he was pursued by enemies who, though unknown and unseen, completely destroyed his peace. In fact, he was laboring under what is now called the *mania of persecution*. He had somehow connected Sir Robert Peel with this conspiracy, and concluded to shoot him; but mistook his secretary for him, and killed the latter instead of the prime minister. He was acquitted on the ground of insanity, but the public rebelled so strongly against the verdict that the matter was brought before Parliament, which instructed the law-judges, twelve in number, to confer together and decide what the rule should be in cases of this kind. They agreed upon the rule put forth by Hoffbauer, as above described, and accordingly it was announced authoritatively as the law of the land.

We may well be astonished that a rule which is to decide the question of life or death for all coming time, should evince the grossest ignorance of the mental operations when affected by disease, and should be put forth, too, by a body of men who might be supposed to be more correctly informed. The rule, be it observed, implies mental disorder, and consequent irresponsibility, the extent of which, however, is limited in an

arbitrary manner, with no reference at all to the pathological condition. It means that if a person imagine that enemies are waylaying him at every turn, he may not be accountable for that belief, because it has intruded itself into his mind without any will of his own; but that, it being there, he is accountable, to some extent at least, for what he thinks and does about it. He is required to act respecting it very much as if he were perfectly sane—killing his fancied enemy if he apprehends from him a murderous assault, as a matter of self-defence, but resorting to the courts for redress, if his enemy has only assailed his reputation, or is preventing him from obtaining certain estates. That there is any ground for such limitation, no one can admit who has been much conversant with the insane. Indeed, the fallacy of the principle in question is obvious enough without any experience of this kind. Everybody knows that the man who imagines that his legs have turned to glass, goes about as usual, instead of keeping still and thus making his conduct consistent with his belief.

How a principle so plainly and completely contradicted by the facts of science, should have obtained the currency that it has, is not easily explained. In England, this, as well as some other mistakes on this subject, may, possibly, be attributed to a famous dictum of Locke, who said that madmen reasoned correctly from false premises. Locke knew as little about insanity as the English law-judges, otherwise he would have known —what the humblest servant in a hospital for the insane knows—that the insane may wander at every step of their reasoning, from premises to conclusion. It is no more than might have been expected, that one who insanely believes that his neighbor has been telling bad stories about him, should also believe himself justified in taking that neighbor's life. The confusion of ideas, so flagrant and so common in discussions upon this subject, is well illustrated by the fact that the judge who presided at the trial of McNaughton, and who approved of his acquittal, subsequently joined the other judges in declaring Hoffbauer's rule to be the law of the land. Under this rule, McNaughton should have been convicted, for, crazy as he was, he certainly apprehended no personal harm from Sir Robert Peel. Another instance of this confusion of thought was exhibited by one from whose multifarious knowledge and keen sagacity something better might have been expected. Lord Brougham said in Parliament, on one occasion, that he could conceive of "a person whom Deity might not deem accountable, but who might be perfectly accountable to human laws." In other words, a mental disorder which would render a criminal act excusable in the eyes of God, might be entirely ignored in the judgments of men. In this statement Lord Brougham did not merely mean to declare—what nobody would deny—that man, with his limited apprehension, might fail to discern mental disturbances that would be perfectly obvious to the eye of Omniscience; he meant to assert the doctrine that a kind and degree of mental disorder, equally obvious to God and man, might lead one to excuse, and the other to punish, any criminal act that might be committed under its influence. How such a monstrous conclusion could be reached, except in defiance of all reason and religion, is not very obvious. We can account for it only on the supposition that, to the mind of the thorough-bred lawyer, it seemed imperative that the legal theory of guilt and responsibility should be upheld at all hazards. For certainly the act in question had in it all the elements of crime, and, of course, the doer of it must be punished, even though God undoubtedly were ready and willing to excuse him.

The other mistake pervading the theory of the law respecting the responsibility of the insane, is to leave out of the account altogether the moral faculties of our nature—the sentiments, passions, and emotions. Here again is another of those inconsistencies which abound in the current reasonings on this subject. While every one admits that in these faculties are the great springs of human conduct, that they incite men to good or to evil, and furnish the motives and impulses by which they are governed, the idea that they, or rather the cerebral organism with which they are connected, may become diseased as well as the reflective faculties, is regarded as preposterous and of dangerous tendency. And yet we have the same proof of the one fact that we have of the other. The conditions of health and disease are precisely the same in both. Both are manifested by the same material organ, the brain, by far the larger portion of which, according to the most prevalent opinion, is given to the moral powers. Disease in any part of the brain must necessarily be followed by derangement of its appropriate function, so that it is in the order of nature that when a certain part of the brain is diseased, there will be some disorder of the affectional faculties. How this conclusion can be denied by any one who believes that disease of the brain may produce loss of memory, of judgment, of attention, and of mental application, is not very obvious. The only possible explanation of this curious inconsistency is that disease of the moral faculties so often resembles manifestations of moral depravity, that it has been mistaken for the latter, especially by those who are not accustomed to regard this subject from a physiological point of view—their notions, as far as they imply any exercise of thought, being founded solely on metaphysical considerations.

And here we see the influence of that school of mental philosophy in which our predecessors were trained. We may safely say, I think, that had Herbert Spencer, and Bain, and Morell, lived and written in the days of John Locke, the popular notions respecting insanity would not now be precisely what they are. Under their teaching, the affectional faculties would have held their rightful place in the mental economy, and would have played as necessary a part in its operations as that of the others in completing the grand result implied in a thinking, social, responsible being. Until they are regarded in this way, we need not expect much improvement in the law of insanity. The tests of responsibility will continue to be what they now are—confined exclusively to the action of the reflective powers. The question will continue to be, did he know right from wrong?—did he know that the act was contrary to the laws of God and man?—did he evince forethought and contrivance in executing it?—ignoring the equally important questions, could he see the moral complexion of the act in its true colors?—had he the power, unaffected by disease, to pursue the right and avoid the wrong; to resist the natural impulses to wrong-doing by the superior force of conscience? Responsibility implies the integrity of *all* the mental powers, moral as well as intellectual. If the mental movement is disturbed by the intrusion of a foreign element, it is immaterial at what precise point this element enters. To suppose otherwise, is as absurd as it would be to suppose that a delicate piece of mechanism could be deranged only at a certain point.

As a consequence of this confusion of ideas, there has occurred a lack of uniformity in the practical application of the rules of law, which, considering the momentous issue involved, suggests no very comfortable reflections. The court may be satisfied of the irresponsibility of the

prisoner, while, under a strict construction of the rules, he must be regarded as guilty; or the jury may prefer to found their verdict on the matters of fact disclosed in the evidence, rather than on the rules of law laid down by the court. And such must always be the case as long as the matter of insanity is treated as a question of law rather than as a question of fact, for here is the source of much of the error that has been committed on this subject. It is especially visible in the current doctrine—as current now almost as it was in the time of Lord Hale—in regard to the exculpatory effect of what is called partial insanity. In some forms of disease, the patient does not seem to be entirely bereft of reason. On some subjects he converses with no lack of correctness or propriety; some business he transacts with his customary shrewdness; and many people meet him in the ordinary intercourse of life without suspecting any aberration of mind. It is not questioned that he still is, in some degree, a rational creature. And certainly it is not surprising that the world should be slow to admit such a mental condition as an absolute excuse for crime. If the person have been entirely rational for some purposes, or in some relations, why may not this circle of rationality have embraced the act for which he is tried? This is a fair question, undoubtedly, and is entitled to a respectful answer. What we complain of, is that, instead of being answered in the light of that knowledge of the workings of the insane mind which is derived from long, clinical observation of the disease, the question is supposed, in actual practice, to carry its answer with it. The logic runs thus: the act *may have been* the product of the reason left untouched by disease; therefore it *was*. We insist that the fact should be proved, and we claim in the interest of true science, as well as of humanity, that the burden of proof lies with the side that denies the influence of the alleged disability. The existence of insanity in never so small degree being shown, it is for the other side to show that it had no part nor lot in bringing about the criminal act. Now we do not deny that this is ever possible, but we do say that in most, if not all, cases, we are positively unable to fix the limits of the operation of disease, and to say that the act in question was not the offspring of disease. No man pretends to discern all the springs and turns of thought even in the rudest sane mind, and it certainly is no easier when the subject of observation is thrown out of its ordinary courses by a disturbing force. We know enough, however, on this subject, to be convinced that, very often, thoughts apparently the most remote and disconnected from one another, are found, on further research, to be closely associated, and thus to lead to conduct otherwise most strange and mysterious. He must have been a superficial or careless observer of insanity, who has not often discovered the most violent acts to be the outcome of a wild and tortuous succession of thoughts. To say that we see no logical connection between the mental condition as obviously manifested, and the criminal act, is simply a confession of ignorance, not a display of knowledge.

In contending that in criminal prosecutions, when insanity is pleaded in defence, the burden of proof should fall upon the side that resists it, we are only following the rule most used in civil cases. There, the existence of insanity having been shown, it is incumbent on the party wishing to establish the act in question, to prove that the person had recovered, or that the act was performed quite independently of, and was untainted by, disease. If a will be in dispute, and the testator be shown to have labored under some unquestionable delusion or weakness of mind,

the presumption is that the will is rendered invalid by the mental affection, and it is left for those who would establish it to prove the contrary, and the rule is the same in reference to contracts. It goes further even, for many a contract has been voided which displayed no indication of unsoundness, and which could not be traced to undue influence. There is not the slightest reason for this difference, in the nature of mental disease. Its agency in the occurrence of crime is as far beyond all our resources of detection, as it is in the making of a will or contract. If any difference is to be admitted, it should be the other way, inasmuch as an issue of life or death is a far more serious matter than one of property alone.

There is nothing in the popular conception of insanity so common or with so little foundation in the nature of things, as the idea that the extent of the malign influence exerted by insanity on the mind, can be accurately measured by the external appearance and deportment as viewed by the casual observer. It is supposed that a person who can converse sensibly, behave properly, and transact business, cannot be so insane as to be irresponsible for his criminal acts. In every trial where insanity is pleaded in defence, this idea is generally the main stay of the prosecution. A host of witnesses is summoned, who testify to perfect soundness of mind. One had frequently met the defendant, passed the time of day with him, and talked about the weather; another had employed him in some ordinary trust or duty; another had had some business-transactions with him, selling him, perhaps, some little necessaries of life, or paying him a trifle of debt or interest; another had met him mingling with company at a watering-place, or at some social gathering; and these persons, one and all, declare that they witnessed nothing strange or unnatural in his looks, discourse, or deportment; and those who had business-dealings with him, and especially if they got a good bargain out of him, declare him to have been uncommonly shrewd. This evidence, though entirely negative, is none the less effective with most of those who hear it. Its force is scarcely weakened by the statement, which they may admit in general terms, that insanity often leaves some operations of the mind untouched, and thus escapes the notice of the casual observer. And yet this trait of insanity would seem to be too common to be so disregarded in actual practice, for every hospital report, every newspaper that we take up, bears witness to its frequency.

In the normal condition, nothing, under ordinary circumstances, is dearer to a man than his life. To preserve it is one of the most powerful instincts of his nature, and, if in certain exigencies he willingly part with it, it is for the rational purpose of securing what seems to him a greater good, or of avoiding a present, real, unequivocal evil. And yet a large proportion of suicides are committed by persons urged by no apparent motives, in the possession of every outward circumstance calculated to make life desirable, and giving no indication whatever of mental disturbance. Up to the last moment, they may have transacted business, conversed sensibly with friends, and in all the relations of life conducted themselves with their customary propriety. Yet who will venture to say that, notwithstanding all these apparent indications of good mental health, suicide, committed under the circumstances just mentioned, does not signify a kind and degree of insanity as destructive of moral freedom and responsibility as the most demonstrative forms of the disease? It is also a well known fact that many of those persons who attempt unsuccessfully to commit suicide, while appearing to the casual observer

entirely sane, subsequently have only the most dim and uncertain consciousness of the attempt. Now, there is no reason to doubt that homicide, and perhaps some other criminal acts, may be committed in a similar state of mind—evincing just as much coolness, deliberation, and contrivance, and just as complete an absence of all the demonstrative signs of mental disease. It is not creditable to the intelligence of our time, that the apparent correctness and propriety of the person in the common relations of life, should be regarded as incompatible with the presence of insanity, or, at least, with any such insanity as should exempt him from responsibility.

Unquestionably, during the present century, the tendency of public sentiment, following, no doubt, that of the clinical study of mental diseases, has been to enlarge the exculpatory effects of the latter. Now and then there have been signs of reaction, never stronger, probably, than at this present moment. To men whose opinions are formed by the current prejudices around them, rather than by any careful study of facts, it seems as if the plea of insanity had been making crime a little too easy, and that a wholesome check to this facility would be found in punishing the insane like other criminals. The argument by which this doctrine is generally supported is that many of the insane know right from wrong, that they are governed by motives, and may restrain their passions. The facts implied in the argument, we willingly admit, for nobody denies that many of the actions of many of the insane are rationally considered and rationally performed. Nobody denies that their conduct may be determined by the hope of reward or the fear of harm. One patient strives hard to obtain a privilege or a favor, while another is deterred from an assault upon a fellow-patient by remembering the threshing he got in a former encounter. The same man who believes that he is coming to want, and destroys himself and family to save them from misery, may refrain, if he please, from exposing his person, from soiling his clothes or from assaulting his neighbor. Why then should he not be punished for offences that are not inspired by his disease? To those much conversant with the insane, the reasons against it are obvious and conclusive. The more that one learns of insanity, the more inscrutable are the movements of the mind affected by it, and the less confidence is felt in any attempt to discern exactly their connections, and to say which can be safely attributed to sanity and which to insanity. If this be so, under the daily and hourly observation of a skilful inquirer, what confidence can be placed in any positive conclusions founded on the testimony of those who have seen the accused at remote periods, who know nothing of insanity beyond what is learnt by the casual sight, now and then, of a person reputed to be crazy, and who are unable by lack of culture to convey to others the impressions thus made on their own minds. If any one could tell us—physician or philosopher, court, counsel, or jury—by what means, with the slightest approach to certainty, we might make the distinction between the working of the sane and that of the insane element, we would not object to the proposed rule. At any rate, let the burden of proof rest on the party which alleges that the criminal act was prompted not at all by the presence of disease.

There is another reason for not punishing the insane, which, feeble as it may be to the common mind, is conclusive to the close observer of mental disease. Admitting, as we do, that much of the daily life of the insane shows no trace of disease, we are none the less sure that the presence of disease, however limited it may appear, is generally incom-

patible with the most correct and vigorous exercise of all the faculties of the mind. It impairs the freedom of the will; it blunts the sense of right and wrong, of the good and true; it weakens the power of self-control, and diminishes the normal amount of restraint upon the appetites and passions. No one engaged in the service of a hospital can have failed to observe how fiercely the passions of the patients are excited by little provocations that would scarcely have ruffled their tempers before; how disproportioned to the offence is the measure of their retaliation; how grossly exaggerated are every feeling and impulse awakened by the ordinary experiences of life; how often native propriety of manner and delicacy of sentiment are replaced by a rude demeanor, and by a coarse, reckless disregard of the rights and feelings of others. Even when the criminal act seems most clearly beyond the range of the morbid influence, there is always reason to suspect, if not to positively believe, that it would not have been committed except for this enfeeblement of the power of self-control, or this blunting of the moral perceptions.

Such being the fact, it shows how groundless is the doctrine, somewhat countenanced just now, of a limited responsibility, as applied to the partially insane, according to which they are always to be punished for their criminal acts, though in a less degree than they would be if unquestionably sane. I need only allude to the doctrine, as its unsoundness has been sufficiently shown by the views already expressed. A moment's reflection must convince any one that no device of legislation could make the application of it in practice otherwise than absurd. The idea of a court or jury constructing a scale of insanity according to its severity, and graduating the measure of punishment thereby, is simply ridiculous, and would be rendered still more so by any attempt to apply it to a given case.

Recently, we have seen the revival of a doctrine, never much in vogue, however, that the insane should be punished in order to deter them, by force of example, from breaking the laws. In support of this view, instances are related of insane persons, who, when talking over the criminal acts of others understood to be insane, have said to each other with much self-complacency, that, being insane, they would not be punished. In this belief, they would naturally use no self-control, and would feel free to break every law on the statute-book. Unquestionably the insane know very well that they may not be punished for their offences, as they know a great many other things. They are able to reason correctly on the subject, as they do on many other subjects. The fact is not extraordinary, nor has it recently come to light, and, considered in connection with other facts, it implies no responsibility. The truth is, that very few of the insane believe that they are insane; consequently, immunity from punishment does not present itself to them as an inducement to commit crime, nor to relax in the slightest degree their control over their passions. While they perceive well enough the abstract nature of crime, they cannot see it as applied to any particular act in their own cases. They are outside of the range of such application, and are a law to themselves. Some are urged by a necessity which they cannot resist, even knowing that they do wrong, and knowing the usual consequences. Some are prompted by strange suggestions: the voice of an unearthly presence, a terror that overrides every thought of consequences, or a spur of savage ferocity unmingled with the slightest sense of humanity. These people act without reference to law or gospel, under motives and impulses as remote from their natural dispositions and principles as the

poles are asunder. The fact that persons reputed to be insane had suffered the usual penalty for their crimes, would have no more restraining influence than blows or threats in hushing the cries of a new-born infant. No human law can be more imperative than that higher law within him which impels many an insane man to some act of violence, and no consideration of justice or pity can come in to lighten the stroke provoked by the trivial offence of a neighbor. Indeed, this idea of the insane being deterred from criminal conduct by penal laws, is utterly destitute of foundation, and we challenge its advocates to produce a single instance in point.

In order to make this idea practically efficient as a means of restraint, some steps would be required to give it an authoritative character, for unless the insane were somehow convinced of their responsibility, clearly and fully, of course it could have no restraining power. There would seem to be an insuperable difficulty in the way of making this idea immediately effective. If left to the courts, the most that could be expected would be now and then a decision declaring insanity to be no excuse for criminal acts. And supposing these occasions to become more and more frequent, until the rule should generally prevail, there still would be a considerable period of doubt and uncertainty as to the actual state of the law. The case could be properly met, if at all, only by an act of the legislature, and, in the present state of public sentiment, nothing is less likely to happen than an act making insanity, of whatever kind or degree, no excuse for crime. If, however, the legislature, without going to this extent, should undertake to specify what forms of the disease should or should not have this effect, as it has in some European countries, the result would be a lamentable failure, producing only confusion and distrust. Our notions of the actual limits within which the operation of any form of insanity is confined, are too vague for such a purpose, and the imperfection of our nomenclature would prevent any approach to uniformity of meaning. And thus we stand: The legislature is indisposed to act, and the rules adopted by the courts are irrelevant, inadequate, and conflicting. Nor from this unsatisfactory condition of the law can we expect any relief, until the whole question of insanity shall be regarded as a question of fact, not of law, as at present; and, thus regarded, shall be taken altogether from the court, and, like other matters of fact, be given to the jury, which would be governed by the weight of evidence and the individual convictions of the jurors. This course is virtually taken by juries, even now, and it is held up as a flagrant instance of our scandalous indulgence of crime, that many a verdict of "not guilty, by reason of insanity," is rendered, in spite of the instructions of the court.

I cannot leave the subject without adverting to an argument in favor of punishing the insane like others, on which unusual stress has been laid in some recent trials. It is represented that in our hospitals for the insane, the discipline is maintained by the use of rewards and punishments. This implies that the patients are deterred from wrong-doing by the understanding that it will be followed by punishment, and the inference is drawn that the same motive would operate in the same way outside, as well as inside, of a hospital. If the statement on which this reasoning is founded were strictly true, it should be taken in connection with the other well-known fact, that many of the insane are not deterred from violence or any other wrong by fear of any consequences whatever. Neither threats nor blows, nor death itself, can turn them from their chosen

course. And thus arises the difficulty which has already presented itself in another shape, that of determining to which of these two classes the accused may belong. By what kind of evidence are we to be convinced that the prisoner at the bar would have refrained from committing the offence for which he is on trial, had he believed that he would be punished?

But we are really not reduced to this dilemma. The idea that the inmates of our hospitals are punished for their delinquencies, is false—entirely without foundation in fact. They are deprived of a privilege or of an indulgence, which they have shown themselves unable to use without harm to themselves or others—an inability which may have been as much beyond their control as the usual manifestations of bodily suffering. Such deprivations, as well as rewards for good behavior, are used as a means of trying and strengthening the power of self-control—a power which in the insane is confessedly weak. Its exact degree of weakness or strength, no one much acquainted with them will undertake to measure; and who then can measure it for the purpose of apportioning the proper amount of judicial punishment? So far is it from being true that the patients in our hospitals are punished for any reason whatever, that the attendants are prohibited from returning a blow, or retaliating for insults or injuries, on pain of dismissal; and yet, when provoked by serious assaults, they might often say with truth, "he knew better; he knew he was doing wrong; and a little summary punishment will help to make him do better in the future." But if to punish the insane be good law in a court of justice, why should it not be in a hospital? We would have it distinctly understood that it is not good law in the latter, not because there is never a case in which punishment might prove a powerful help to improvement, but because no insight short of Omniscience can ever know for certain which are such cases. It may be urged, perhaps, that exception should be made of cases in which it is shown by satisfactory evidence that the accused had often been deterred from wrong-doing by the fear of punishment. The statement is quite too vague to serve as the ground of a general rule; and besides, it is open to the objection of showing a negative rather than a positive state of facts. What a man refrains from doing under certain circumstances, is no safe measure of the extent of his self-control under other and very different circumstances.

That there has been, of late years, much practical disregard of the rules of law, on the part of juries, must be admitted; but the course of judicial decisions and verdicts within a very recent period, shows how grievously potent they still are. About two years ago, one Fordham, a patient in an asylum for the insane, murdered an attendant. It appeared in evidence that he was an epileptic, quarrelsome, and often violent; that he had quarrelled with the attendant, and had declared that he would have revenge. He was tried, and convicted of murder. The court, Mr. Justice Denman, charged the jury that the mere fact that the prisoner was laboring under a delusion as to his treatment in the asylum, as no doubt he was, was not sufficient excuse in law. "If a man killed another," said he, "while under a delusion that he himself was about to be killed, and that he was acting in self-defence, he would not be punishable; but if a man did so for some supposed injury to his character or fortune, then he would be responsible." Sentence of death was passed in the usual form, but the punishment was afterwards commuted to penal servitude for life. Some three or four years ago, Mr. Lutwidge, one of the members of the

Board of Lunacy, while passing through a hospital which he was visiting officially, was assaulted by a patient, who inflicted upon him a wound from which he soon died. The patient's insanity was unquestioned, though he had not been supposed to be dangerous. In speaking of the case, in their annual report, the twenty-eighth, the Board say that the man "was quite responsible for his actions." Since this case occurred a man named Blamfield has been tried for murdering a fellow workman. The act was done suddenly, without apparent premeditation or provocation. Blamfield had been a patient in a lunatic asylum, from which he had been discharged, uncured, some months before. The medical superintendent of the hospital testified that, while in his charge, Blamfield had had a fear of punishment for misconduct, and had been governed by this feeling. He, therefore, considered him responsible for his acts. The man was convicted of murder, and sentenced to be hanged; but his punishment was subsequently commuted.

Such are the more prominent difficulties now in the way of making the plea of insanity in criminal cases as effective as it should be. The question may naturally be asked: How can these difficulties be most readily removed, and the law be administered in conformity with the truths of science? It is scarcely within my purpose to speak upon this point, but it may not come amiss to indicate very generally the direction from which reform may come.

Let me say, in the first place, that if courts are to continue to measure the exculpatory effects of insanity by certain arbitrary tests, they are bound to avail themselves, as did Lord Hale, of all the existing medical knowledge on this subject. Instead of repeating, one after another, rules and distinctions, of which the sole claims to respect consist in the fact of their having been uttered before, it is their duty to learn as much as possible of the nature of insanity from the only true source of knowledge—the practical observation of the insane. Legal knowledge is, for the most part, so technical that the decisions of the courts on most matters are received by the public in faith, nothing doubting; but on this subject, the public will have an opinion of its own. The people who would shrink from the presumption of understanding a question of contingent remainders, or of eminent domain, may have little respect for any rule of law touching the responsibility of the insane. Have we not a right to ask the judges to make themselves acquainted with the nature of insanity by all the means within their reach, so that, instead of directing the jury by a servile repetition of something that others have said before them, with no other authority than the fact that it *has been* said, they shall present only the established facts of science, and such considerations as are naturally suggested by them? Let it be remembered that we are dealing with a branch of medical jurisprudence, and, therefore, that the study here recommended belongs as well to the lawyer as to the physician. I do not suppose, indeed, that the soundest instructions would always be followed by a proper verdict, for juries are too fond of manifesting their independence by disregarding whatever may conflict with their own foregone conclusions. But it would remove somewhat the distrust now felt of judicial decisions, in which the advancement of knowledge is entirely ignored, and would prepare the way for a better and more satisfactory administration of justice.

A still better step, as above intimated, would be that already taken by the courts of New Hampshire, whereby the question of responsibility

is treated as a matter of fact, not of law, to be determined, like other matters of fact, by the jury. In the performance of this duty, juries would be governed by the evidence, aided by the explanations of the experts. This would not necessarily insure a correct verdict; for the evidence might be conflicting, the experts might disagree, and the jury might be governed more by its prejudices than by the real merits of the case. It is doubtful, however, if anything better could be obtained under our mode of procedure, and this is not likely to be changed at present. Some improvement might follow from putting into the jury-box men of higher personal character, moral and intellectual, than such as are often found there. This improvement could be more easily made, I am sorry to say, than could one equally if not more efficient, whereby the experts should prepare themselves more carefully for their duty, and should discharge it, divested of every unworthy bias.

Discussion on Dr. Ray's Paper.

After the reading of the preceding paper, Dr. Clement A. Walker, of Boston, said:—I wish to express my sense of obligation to Dr. Ray for the clear and distinct manner in which he has presented this subject in his valuable paper. Just now, such papers are much needed, for there seems to be at present a reaction setting in against the insane, causing them to be held responsible for acts committed while under the influence of insanity. This reaction is particularly strong in Massachusetts, where the action of the executive in commuting the death penalty to imprisonment, in the case of a man convicted of murder, and who was by many regarded as insane, has been received with astonishment, and has been condemned as an unjust act. There are some in our specialty who may differ with the ideas of Dr. Ray in regard to the limited responsibility of the insane, but his views in this respect have my full endorsement.

The President, Dr. John P. Gray, of Utica, said:—I do not agree with Dr. Ray in drawing such a distinct line between the intellectual and moral elements of the mind, for this would divide the mind into parts, and would give, as a logical result, moral and intellectual disorders, independent of each other. I believe in the essential unity of mind, and I cannot assent to his view either as being sound psychology, or as true in clinical observation. I observe, however, that, though Dr. Ray makes this theoretical separation, yet when he comes to treat of actual insanity, and of responsibility, he reunites the moral and intellectual faculties.

THE SIMULATION OF INSANITY BY THE INSANE.

BY

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THE literature of insanity feigned by the sane mind is extensive, and dates back to the earliest period of recorded history. The feigning of Ulysses in profane, and of David in sacred, story, are familiar to all, as is likewise the history of Lucius Junius Brutus, who saved his life by successfully assuming imbecility, and Shakspeare's two oft-quoted examples in the characters of Edgar and Hamlet—the latter, I think, blending some actual mental disease with simulation; but upon the subject of the present paper, the simulation of insanity by the insane, not much has been written, though enough is known to make it a proper subject of inquiry.

Not less interesting to the student of morbid psychology than the well-defined, well-recognized forms of insanity, are those obscure, anomalous conditions of mind which occasionally appear, but in regard to which he fails to obtain any light from the standard books. Though more numerous, probably, than they are generally supposed to be, yet they are comparatively so rare, and so imperfectly understood, that, for the most part, after exciting a little temporary curiosity, they pass from attention, and are forgotten. And yet they must ever constitute a very important class of mental disorders, for the reason that their existence, however infrequent, must necessarily modify the conclusions that might be drawn from the more common forms of mental disease. In fact, no physician needs to be told that many important steps in the progress of his science have been made by the careful and persistent observation of what, at first, seemed to be anomalous and exceptional cases.[1]

The meagre record of cases of simulation by the insane, to be found in the literature of psychiatry, may be due partly to the fact that the necessity of searching these cases out, has not been so imperative as the detection of feigning by the actually sane, who so often simulate insanity to defeat the ends of justice, and for other sinister purposes; partly to the fact that proof of simulation, in the case of persons already adjudged to be insane, possesses no real, practical value; but mainly to the fact, which I think observation has established and will continue to prove, that the insane do not, in general, assume either different degrees or forms of insanity from those with which they are actually afflicted.

We might here paraphrase a familiar quotation, and say *in maniâ veritas*, for the lunatic is generally true to the promptings of, and mainly dominated by, his acquired, morbid nature. To the practised eye, a tinge of insanity colors nearly all that a general lunatic says or does, and it is doubtful if, in the acute stage of profound and constant general mania, the actually insane do ever simulate. The general insurrection and rebellion of all the ideational and emotional centres which seem to

[1] Dr. I. Ray, Remarks introductory to case of Bernard Cangly: American Journal of Insanity, July, 1865.

characterize this form of mental alienation, the universal tumult into which all the faculties of the mind are at this time thrown, would seem to leave no room for the planning and contrivance essential to simulation, though it be never so imperfectly done. The assuming of a character different from the dominant and all absorbing morbid impulses of the individual, would be contrary to all observation in acute, general mania, and is to my mind impossible, notwithstanding the existence, in this form of mental disease, of considerable method and shrewdness in planning and executing in the direction of morbid fancies and inclinations. To attempt simulation would only seem possible, if possible at all in this form of mania, after the violence and excitement had passed away and the disturbed faculties had become comparatively calm, with the return of some degree of rational appreciation of acts committed and of penalties incurred, and to be avoided, and with an obliviousness, on the part of the individual, of the fact of his insanity. Here the question would present itself as to the actual existence of insanity at the time of feigning, while its pre-existence might be readily conceded.

Feigning has the nature and quality of rationality, and is rarely, if ever, practised without a motive. It proceeds *primâ facie* from a rational mind, just as motiveless acts proceed *primâ facie* from a mind deranged; yet we know that all acts without motives are not the offspring of insanity, as we know that acts prompted by rational motive do not proceed from the sound mind only. We sometimes see, however, a kind of unconscious imitation, in some exceptional, morbid, mental states, of psychopathic and neuropathic conditions seen in others, and occasionally a kind of simulation lacking the conscious element in real insanity—a mere freak of disease—which, however, is scarcely worthy to be called true simulation. The insane appear at times, when they have an object to accomplish, more crazy than, and different from, what they really are; this is the sense in which we use the term simulation, and this condition is akin to that of feigning by the sane.

Of course we do not expect to find feigning in advanced, general dementia; but we may not search for it in vain in the intervals and remissions of recurrent or periodical mania; after the mania transitoria; possibly in certain stages and forms of what is called chronic, general mania; in the hysterical and partial forms, the so-called monomanias; and in the moral or affectional forms of insanity—the "*manie sans délire*" of Pinel. Let me, however, repeat, to be plain, that in that profound, general involvement of the mind in disordered action, which depends probably upon more or less implication of the whole brain, and which we term acute, general mania, feigning would seem an impossibility. The psychical conditions essential on the one hand to simulation, and on the other to the truthful acting out of acute, general, mental disease, seem incompatible; the real victim of such a malady would not be sane enough to simulate, except during a so-called lucid interval, and comparative or entire freedom from disease.

Simulation, while it presupposes a degree of sanity, does not require that the patient should be wholly sound in mind, and might be attempted by a convalescent patient, not thoroughly recovered, for the purpose of remaining longer in the hospital or for some other reason. Supposed feigning, in what appears to be a case of acute, general mania, should lead us to suspect the correctness of our diagnosis, and to review the steps by which we have reached the conclusion that the person is actually insane. I am not prepared to say this much of other forms of mental

disease, except dementia. We may admit the possibility of subsequent feigning after the subsidence of a paroxysm of acute, general mania, and still hold to its general improbability, simulation being, as has been said, an attribute of sanity, just as is the existence of a reasonable motive, though both do often actuate the mind deranged. We should fall into a grave error, however, should we deny the possibility of other forms of insanity than those which I have excepted, being accompanied with either acts of feigning, or motives; an error which facts of daily observation among the insane fully prove.

The government of insane-asylum households, as every physician knows, is conducted upon the idea that the insane, like the sane, though in a degree more or less modified by disease, are actuated by ordinary motives, and can be kept within certain rational limits of conduct and expression by the conditional rewards and inducements to correct behavior there employed. The rude and indecorous often act with propriety when such a course is made the condition upon which they are permitted to take part in the amusements and religious exercises of the asylum, or to have other rational indulgences allowed them. Proper letters are written, and delusions suppressed by the writers thereof, in some instances, after the patients have learned that very insane letters are discountenanced by the superintendent, and returned to the writers for improvement in regard to their rational tenor. The love of absent wife or husband, father, son, or daughter, as the case may be, and the natural desire to communicate with them, sometimes leads to compliance with the superintendent's somewhat compulsory request, and to the expunging of all evidences of insanity from their letters, by the patients themselves; and at this point the re-establishment of the rational exercise of volition, in restraining and refraining from the expression of morbid fancies, sometimes commences, and the patient's convalescence begins.

The fact, therefore, that the insane are more or less influenced by rational restraints and motives, like the sane, though in an impaired and lessened degree, would seem to need no further illustration. All who have much observed them, know that they are constantly induced to do the above and other acts which may result in benefit to themselves. They not unfrequently pretend to have abandoned cherished delusions, and to have entirely recovered their reason, with the view of securing a premature discharge from the asylum; and even good judges of insanity among medical superintendents have sometimes been deceived by the plausible pretences of such patients, who, failing to convince the asylum medical-officers, have sometimes succeeded in getting the hearing of courts, and have been released on writs of habeas corpus, only to convince every one, after a time, of their perfect fitness for perpetual restraint.

The co-existence of insanity with the power of devising the most plausible explanations of erratic and insane conduct, is sometimes seen before courts of justice, as in the case of Col. M., District Attorney in one of the Southwestern States, under President Jackson, as detailed by Dr. Ray.[1] Mr. M. entertained the delusion that he was cousin to the Duke of Wellington, and to Napoleon; he cut off his own nose, and, after the rhinoplastic operation had been performed, cut out the cicatrix on his forehead whence the nasal flap had been taken. "He was a passionate, dangerous lunatic," according to Dr. Bell, and yet so plausible in explanation and extenuation of his conduct, and in defence of his personal

[1] Medical Jurisprudence of Insanity, 4th ed., p. 196.

and legal rights, that no asylum could hold, and no tribunal seriously punish him.

The celebrated suit of Wood *v.* Dr. Monroe, in England, for false imprisonment, as cited by Bucknill and other writers, also illustrates how a really insane person may succeed in concealing his mental weakness for a time from the most astute observers. The severest examination failed to detect Wood's mental infirmity until he was asked what had become of the Princess with whom he corresponded in cherry-juice, which immediately caused him to reveal his insanity, and he lost his case. Discovering the cause of his failure, he renewed the suit in London, and then all the ingenuity of the bar, and the authority of the court, could not elicit an exposure of his delusions, though he still entertained them.

The simulation of insanity would seem no more difficult than the assumption of sanity by the insane. If they can conceal, or explain away so as to deceive the best judges, mental defects which they actually possess, why may they not assume abnormal traits which they do not possess? In the corridors of an insane asylum we sometimes see one lunatic contemptuously or in sport caricaturing the whims and vagaries of another, all the while fancying himself the sanest of men, while he regards his brother lunatic as the simplest of simpletons, or the most knavish of knaves. "Amidst our criminal population, too, are hundreds who can hardly be said to be sane and responsible, but who, in the lower ranks of life, commit a succession of crimes of no great magnitude, which render them the almost perpetual inhabitants of jails. Some of them are so violent, outrageous, and destructive—so silly in their motiveless fury, and so childish in mind—that we may call them imbecile, or insane, and have good grounds for our opinion."[1] These are the illy fed and clothed, badly raised and housed, inhabitants of the densely populated portions of our large cities, or the neglected children of drunken, epileptic, or otherwise mentally maimed, parents, whose blood has been poisoned with alcohol, opium, nicotine, and the noxious vapors usually abounding where these cerebro-mental abortions come forth and grow up into dwarfed and diseased maturity. At the maternal fount they drink in disease, and are never afterwards entirely well—never perfectly sound in mind, and never able to act out a natural life. When these human abnormities grow up to manhood and womanhood, having led a life of deceit, we might reasonably expect to find them simulating insanity, as they sometimes do, when detected in, and incarcerated for, criminal acts, being all the while unconscious that they are already really victims of disease. This class should receive more careful examination, with a view to the discovery of simulation among its members. That many cases are not recorded, may be due to the fact that sufficient search for them has not been made, simulation by the actually insane not having, until a comparatively recent period, been conceded by writers. I can recall the names of no authorities more remote than Griesinger and Baillarger, who have admitted the possible co-existence of simulation with real insanity.

Armand Laurent,[2] as lately as 1866, gave several illustrative cases, especially in connection with imbecility. A case was reported in the *American Journal of Insanity* for 1863, but the most recent and unequiv-

[1] Blandford's Lectures, 1871, p. 390.
[2] Étude Médico-Légale sur la Simulation. Par le docteur Armand Laurent. Parie, 1866.

ocal recognition of the fact of simulation conjoined with actual disease, by high authority, is to be found in the report of the case of Michael Trimbur, in the number of the same journal for October, 1874.

It would be interesting to know what might have been the modification of Dr. Parchappe's view of the mental condition of Lambert, whose case is reproduced by Dr. Ray,[1] had the physician of the asylum at Rouen had in view the possibility of blended simulation with some remaining insanity. It will be remembered that Dr. Parchappe pronounced the case one of simulation of unconsciousness, after the prisoner had come to himself, in order the better to escape responsibility for his acts, he having, in a paroxysm, induced either by the virus or by the fear of hydrophobia, murdered his mistress and another woman without provocation, and giving as a justification for the homicidal act, in regard to the former of his victims, the insane reason that he desired to bestow her money in charity, which, he said, she would never have done had she lived. On the day of the murder, Lambert admitted killing his mistress, and repeatedly exclaimed, "Jesus my God, my fortune is made," and begged his captors to release him because, he said, he "had eight more murders to commit." On the following day he denied having killed his mistress, but said "if I did, I do not remember it." Four days after the murder he recognized the hatchet with which he had done the deed, but denied all knowledge of the murder. I do not offer this as certainly a case of simulation conjoined with insanity. It may or may not have been such, and, as my purpose is rather to elicit discussion than to advance positive opinions, I have deemed an allusion to it not out of place. Dr. Ray, in summing up this case, concludes that it was one of feigned insanity, while Dr. Parchappe, as we have seen, thought that it was an example of simulation after recovery. May not the simulation have begun before recovery had been completed, after a realization, upon the part of Lambert, that he had committed heinous crimes from the penalty of which there appeared to him no escape, except in assuming unconsciousness of his acts?

Cases like that of the criminal in the prison at St. Ange, as related by Prof. Monteggia and translated by Marc and Ray, and like that of Samuel S. Rich,[2] which occurred in this country, come to mind, in this connection, as having shown perhaps phases of simulation, which might have been revealed had the possibility of simulation co-existing with insanity been entertained by the profession in those times. There was no history of epilepsy in the case related by Prof. Monteggia, and though no such rigid search could have then been made for epileptiform complication as would be made now, there is not the slightest suggestion of anything of this character in the history of the case as it has come down to us.

It may not be transcending the limits of scientific propriety, to suggest simulation in connection with real insanity as a possible explanation of some of the features of the famous case of Joseph Waltz,[3] still fresh in the minds of all. There was undoubtedly simulation, and it was most bunglingly done; Waltz pretended to be suffering from dementia, which was certainly not the case. His "don't know" answers, made so often to interrogatories concerning facts which he clearly knew (such as the names of his mother and father, and his own age), and the post-mortem examination, clearly prove this point.

[1] Op. cit., p. 415.
[2] American Journal of Insanity, April, 1860.
[3] Ibid., July, 1874.

Just here the thought occurs, that when equally honest and experienced experts view a case, which they have had equal opportunities of observing, on the one hand as one of insanity, and on the other as wholly one of feigning, it may be reasonable to look for co-existent simulation and real disease, to explain the discrepancy of opinion. The case of Waltz also suggests another possibility, viz., that in some cases, the eagerness of counsel to make out a case of insanity from too meagre data, aided unintentionally by the suggestive questionings of the physicians who visit the prisoner at the counsel's request, may sometimes lead a prisoner, not in the beginning so disposed, to attempt feigning; and that if the sane may thus obtain an idea that simulation can be successfully practised, we need not be surprised to see the same thing undertaken by some real, though not pronounced, lunatics.

In a letter from Dr. Bucknill, referring to the case of Jesse Pomeroy, whom Dr. B. had visited with Dr. Edward Clarke in the jail at Boston, in April, 1875, the writer says "some physicians had suggested concealed epilepsy . . . . as the cause of this boy's blood-thirsty propensities, and had questioned him as to the existence of an *aura*. Whether this boy had got the idea into his head or not, I know not, but he told Dr. Clarke and myself that he often had the feeling that a light feather was drawn across his forehead from one temple to the other. Now, as Dr. Clarke remarked to me at the time, this is not the accurate description of an *aura*, which follows the course of nerves."

"Generally, after the acute stage has passed off, a maniac has no difficulty in remembering his friends and acquaintances, the places he has been accustomed to frequent, names, dates, and events, and the occurrences of his life. The ordinary relations of things are, with some exceptions, as easily and clearly perceived as ever, and his discrimination of character seems to be marked by his usual shrewdness."[1] His replies to questions may or may not indicate delusions or other extravagances of thought, while his whole demeanor and conversation may show that he has some appreciation of his previous mental condition, and a fair conception of his present surroundings. Under these circumstances, it is not difficult to suppose that a really insane person, finding himself arrested, and in the hands of the law, on the charge of murder or other crime, which he knows that he has been seen to commit, and from the penalty of which there seems to him no escape except through the plea of insanity, might conclude to simulate such a form of insanity as in his opinion would secure his exculpation. Such a person might not believe in the existence of his own real mental disease, and might fear that those who were to try him would be equally incredulous; the insane are not generally conscious of the extent and degree of their mental derangement. Admitting, then, the existence of a sufficient degree of rationality, in an insane person, to prompt to an effort at self-preservation through the act of feigning, would he probably assume a more exaggerated form of mental disorder, just as sane men usually do with the view of making a favorable impression, and with the customary result of over-acting and detection? We cannot reason out an answer to this question, and practical illustrations are too few to enable us to generalize on the subject.

An insane person, having once been an inmate of an asylum, would there have opportunity to become familiar with the ordinary character-

[1] Ray, op. cit., p. 390.

istics of insanity, and would not, I think, be so likely to assume the tragic and exaggerated forms of madness, as one more ignorant. His capacity to successfully reproduce what he had observed in the asylum, would depend upon the degree of mental soundness existing in him at the time at which he had been an inmate, and remaining with him at the time of attempted feigning. Insanity is a crippled, rather than a destroyed, or obliterated, mentality.

From the nature of insanity, we see that feigning is possible oftener than it is shown by experience to occur in connection with the usual forms of the disease. Insanity is an impairment of one or more of the mental faculties, by reason of disease involving the brain; and as it may exist in every degree, observing the same pathological laws as any other disease, it is evident that acts which in themselves are rational in character, may be done by the insane. A sick man is seldom so sick, unless it be in the last and hopeless stage of his malady, that he can do none of the acts which he could perform when well. Among the acts, therefore, which the insane must be deemed capable of performing, we must include simulation. It is not uncommon for the healthy human mind to dissemble, especially in civilized life; and this natural trait does not always wholly forsake the mind diseased, though it must be confessed that the insane wear less of a mask than the mentally sound, and thus we come again to the general truth as applied to the insane, *in maniâ veritas.* Some exceptions to this rule have come to my notice, and further observation may discover more; but not enough to invalidate the rule as applied to insanity in general: *Exceptio probat regulum.* A mental phenomenon worthy of note here, but not germane to the subject, is the unconscious or semi-conscious *imitation*, rather than simulation, of insanity, which is displayed on certain occasions by those who largely inherit the insane neurosis. It consists in a sort of sympathetic taking-on of an evanescent form of insanity by other members of a family, under great excitement, as when one of their number has become profoundly afflicted with some marked form of mental disorder. A father or mother, for instance, brings to the asylum a son or daughter, when, from the exaggerated and unnatural conduct of the parent, aside from the natural manifestation of grief to be looked for on such an occasion, but which is sometimes wanting, the superintendent finds it difficult to determine which for the time acts the most insanely, parent or child. The conduct of the parent appears anything but rational, yet a return to home, divested, in a measure, of anxiety and the weariness of watching, with a season of rest and sleep, and better appetite and digestion, suffice to restore the disturbed balance of the mental faculties, and the parent, by reason of regular life and habits, escapes the affliction of positive insanity, though possessing and transmitting a neuropathic diathesis but one remove from it.

Insanity, as Prichard observes, sometimes co-exists with an apparently unimpaired state of the intellectual faculties; though "the ideational portion of the faculties," as Blandford remarks, "is so intimately joined to the emotional, that the two must be sound together, or unsound together." Admitting the existence of so slight an intellectual aberration, in certain kinds of insanity, that the intellectual lesion is more theoretical than apparent, it is easy in such cases to concede the possibility of simulation on the part of the patient, to extenuate erratic and immoral conduct, the result of disease. The subjects of moral insanity sometimes simulate inebriety, as well as insanity, to palliate and excuse what ap-

pears to them and to others, not expert in detecting mental aberration, inexcusable conduct. The case of Col. M., already alluded to, was one of this kind; many of his freaks were excused and explained away, when they could not be denied, upon the plea that he had drunk a little too much on that particular occasion.

It is not easy to perfectly imitate diseases involving the mind in disorder, notwithstanding that Zacchias has said that feigning is easy, and detection difficult. The unconscious "method that is in madness, the constant and consistent reference to the predominant idea, which the practical observer detects amidst the greatest irregularity of conduct and language,"[1] requires all the mental faculties in their fullest vigor, and unimpaired by disease, for successful personation, and is then rarely successfully feigned before the eye of the physician who is experienced in detecting the true features of morbid mentality. While, therefore, we may concede the possibility of occasional successful feigning by the mind in full possession of all of its faculties, we should not expect the crippled mind of an insane person to deceive us, and the fact of simulation being detected should not preclude the possibility of co-existing insanity in any particular case.

Dr. Ray, in his report of the case of Trimbur,[2] says: "The criminal classes, to which most of these simulators belong, know as well as everybody else that the plea of insanity is one of the dodges whereby people now escape the punishment of their crimes, and they may not forget to act accordingly when they become insane themselves." This was the case with Trimbur, who, "being unconscious of his own real insanity, but with mind enough to understand his situation and to remember what he had heard about insanity in connection with crime, concluded to make a show of being crazy."

The following case, communicated to the writer by Dr. Joseph Workman, for many years the distinguished medical superintendent of the Toronto (Canada) Lunatic Asylum, seems equally illustrative of blended simulation and real disease, and, as it has never been published, I give it here in Dr. Workman's words:—

Some ten or twelve years ago, I met [says Dr. Workman] with a case of genuine simulation in a man who had murdered his wife. He had previously been a patient under my care, for about a year, when he undoubtedly was insane. Just as I was on the point of discharging him as recovered, he eloped. I did not use much exertion to recapture him. He went home to his farm, and got on well until his insanity returned, taking the form of jealousy of his wife, which, I need not say, was utterly causeless. One day, in the sugaring season, in the bush, he killed her with a billet of firewood. He was apprehended—did not in fact try to escape—admitted his crime, and was tried at the next assizes. I was summoned as an expert witness, and had a long interview with him in the jail before the trial. I knew him at first glance, and asked him if he did not remember me? He said that he did not; that he did not think that he had ever seen me. "Why, John," I said, "you must remember me well; you lived in the same house with me for over a year, and talked with me hundreds of times. You remember being in the asylum?" No! He had no such recollection, but people had told him that he had been there. "Well, you have not forgotten your old friend Mr. E., the steward?" He did not know him at all, and so on throughout all our colloquy. I had the most thorough conviction of this man's stupid mendacity and bootless simulation, and at the same time of his insanity at the time of committing the murder, and I stated both to the jury. He was

[1] Ray, op. cit., p. 388.

[2] American Journal of Insanity, October, 1874.

acquitted on the ground of insanity, and was committed to the asylum for criminals, at Kingston, where he is still detained.

Two or three years after his trial, as I was passing through the asylum in company with the medical superintendent, a patient stepped up and held out his hand, addressing me very courteously, and inquiring after all my family. I did not recognize him until the medical superintendent told me that he was my old friend J. C. "Oh! John," said I, "how is it that you know me so well to-day, but did not know me at all in Guelph jail?" His reply was, "I did not want to know you that time." This man had certainly a very powerful motive for simulation of mental frailty, but he played his part too unskilfully to impose on my credulity. Jealousy was not the *cause*, but the *form* of his insanity, and when he had forever got rid of the subject of his delusions, a sober after-thought assumed the government. He was, as I believed, still insane, yet possessing that degree of stupid cunning which not a few sane people evince. He knew that he would be hanged unless acquitted on the ground of insanity, for in Canada murderers generally have but little chance of escaping the death-penalty. Had he been thoroughly sane, he would have known that sufficient proof of his past insanity, both in the asylum and afterwards, could have been produced, and he would have abstained from his clumsy simulation, or he would have acted his part more cleverly. I could not say that he committed the common error of simulation—overdoing his work. He did not rave, babble, or declaim, or indulge in foolish antics and grimaces.

In criminal cases, when an individual, in whom there exists undoubted evidence of mental disease, actually pretends to have a form or symptoms of mental disorder not in harmony with the kind of insanity which actually afflicts him, or not in the natural course of his disease, the presumption is reasonable that there also exists in him a sufficient degree of rational volition and appreciation of surroundings to modify the degree of his responsibility to law. The perplexing question then arises as to whether the individual's state of mind at the time of feigning be the same that it was at the time of violating the law; the presumption is in favor of a clearer mind at the time of the simulation than at the height of the insanity, and this appears to have been the case with Dr. Workman's patient. The mind disturbed by disease, varies in the intensity and degree of its disordered manifestations at different times, even as the healthy mind is known to vary on different occasions in the display of its natural peculiarities and powers.

Other questions are here presented, of a medico-legal character, relative to criminal responsibility and testamentary capacity in certain cases, which, in the present state of our knowledge of psychology, cannot be satisfactorily answered in general terms. Eminent alienists all admit, in the abstract, the existence of different and variable degrees of rational capacity and responsibility in mental disorder. There are, as Locke says, "degrees of madness as there are of folly," but to determine precisely what amount of impaired mind-power exists in particular cases, may often puzzle the most learned and experienced psychological experts. The law has attempted to cut the Gordian knot, to untie which has so long baffled the most thorough students of the mind diseased, by deciding that the capacity to distinguish right from wrong, should settle the question of responsibility to law in criminal cases; but practically we know that an individual may clearly know right from wrong, and yet, under the overpowering influence of cerebral disease, be irresistibly impelled to do the wrong. The importunities of the insane to be restrained, when suggestions of violence, prompted by morbid states of the brain, arise in the mind, and when the patient's own recollection of his past

experience teaches him that resistance to his insane impulses beyond a certain limit is impossible, are familiar to all accustomed to come much in contact with the insane in asylums. The physiological fact of unconscious cerebration is applicable to the mental operations in disease, as well as in health, and to many acts of the insane, especially of the epileptic class, but certainly not to all. The legal test therefore fails, and indeed it has been much qualified of late in the rulings of the courts. We must concede, however, that this test can be justly extended much further than at first blush would appear to be proper, for unconscious cerebration in a paroxysm of insane fury would save many a lunatic who, a short time before or after, would fully comprehend the nature and quality of his acts.

The facts admitted in this digression would tend to prove the possibility of simulation by the insane, even if it were not established by experience; knowing, as many insane persons do, that they have unconscious states in which their irresponsibility is conceded by all, it would not be strange for an insane man to pretend to have been in such a state, when he really had not, to escape censure or punishment for some act which he knows that he ought not to have done, and the impulse to do which he knows that he could have resisted. I do not now allude to those acts of the insane which, from their history, we might expect them to commit, such for instance as suicide in the suicidal form of madness, homicide in the homicidal, burning in the pyromaniacal, stealing in the kleptomaniacal, etc.

There is one fact which might in some instances deter insane persons from feigning, even when the disposition and capacity existed, and that is the knowledge which they possess of the estimation in which they are held, as irresponsible. Who, accustomed to live with the insane and thus made familiar with their true character, does not know how keenly conscious they are of the existence of this erroneous estimate of their real condition as regards responsibility, removed, as they are in the asylum, from those sources of irritation which have caused, or which tend to keep fresh opened, their mental wounds? A patient once told me that he would have his liberty, or kill me. I said, "Then you would be hanged." He answered, "They could not do that, I am insane." "But," said I, "you would not be insane on that subject. You know it would be wrong, and your insanity is not homicidal." He said, and said truly, "No jury would ever hang a lunatic for anything that he might do in an insane asylum." Not being able thus to intimidate him, I approached him ever afterwards with caution, and always dodged, when practicable, the discussion of the question of his personal liberty. This patient used the word lunatic sneeringly, for, though admitting that he was not always right in his mind, he did not consider himself seriously insane, though, at times, he was really very much so, his paroxysms of excitement lasting many days at a time. It is easy to conceive of circumstances under which such a patient might simulate, as well as rely upon, his reputation for insanity.

The fact of unconscious cerebration, before alluded to, as we see it manifested in cases of hysteria, mingled with conscious and partly volitional, mental activity, explains the simulation of real insanity which sometimes co-exists with this singular morbid state. In hysterical insanity, there seems to be a morbid desire to act out actually controllable vagaries, as well as those really insane promptings which are beyond the control of the will. The insane of hysterical tendencies often act in

a seemingly controllable, but very insane, manner, apparently through a morbid craving for extraordinary sympathy and attention. Occasionally, however, their actions are apparently wholly motiveless, as with other and aggravated forms of insanity.

Though simulation is rarely practised without motive, it is possible for the motive to be sometimes a very foolish one, and, while it may be unaccompanied by delusion, yet scarcely explainable upon the hypothesis of perfect sanity. Such a case would lead to great perplexity in the mind of the physician.

The egotistic feeling so often uppermost in the mind diseased, causing the insane to seek in so many ways to attract attention, or excite wonder and commiseration, is only an exaggeration of a not uncommon, natural trait of rational minds. It may be donbted whether the case reported by Dr. Bell, an abstract of which may be found in Dr. Ray's[1] excellent work on the jurisprudence of insanity, was without real disease as well as blended simulation. Dr. Ray, in introducing the case, regards it as "somewhat curious, considering the youth of the subject, the apparent want of motive, and the severity of the symptoms."

The lad, thirteen years old, had fallen on his head two years previous to admission, and ever since that period had exhibited some anomalous symptoms of disease, which had been referred by his physicians to derangement of the digestive organs. For the last few months, the symptoms had been more severe and decided. He had refused food for long periods, had had spasms, had lain with his eyes fixed and his legs drawn up, would hold his breath and strike. On admission to the asylum, he presented the appearance of a sickly, emaciated boy, under puberty, unable to stand, exhausted by suffering, breathing quickly, and passing his evacuations in bed. Every few minutes he had a frightful spasm, commencing with a convulsive shaking of the head, pawing of the hands, and turning up of the eyes. Soon his hands would vibrate against his sides and chest, his countenance would be dreadfully distorted, and then would commence a horrid scream that might be heard over the whole premises. In this condition, with occasional remissions, and the addition, at one time, of diarrhœa, he remained for about a month. . . . Being watched through a hole in a blanket hung before his window, he was observed to jump up and stride about his room as actively as any body, but at the slightest noise resumed his old position, screaming and groaning.

Dr. Bell broke in upon him before he could regain his bed, chided him for his deceit, and bade him walk into the hall. "The spell is broken," says the record, "the feeble knees are made strong, the convulsed and distorted visage is calm and smooth, and the young deceiver goes forth clothed and in his right mind." Dr. Chipley, commenting on this case,[2] says, "We can scarcely conceive that one would assume a character so painful to sustain, without some deliberate purpose, or an end to be accomplished;" and yet no motive such as might be supposed to actuate the rational mind, is at all apparent in this case. The fact of the boy's having refrained from his usual manifestations on the unexpected appearance of Dr. Bell, whom he doubtless held in no small degree of awe as the all-powerful head of the establishment, while it proves a power of control over the actions under great external influences, and is strongly presumptive of feigning, does not, to my mind, conclusively establish the fact of entire sanity. There is nothing absolutely incompatible with insanity in sudden cessation of its symptomatic manifestations. If this youth

[1] Op. cit., p. 405.

[2] American Journal of Insanity, July, 1865.

was willing to make such continued, great, and painful sacrifices of comfort and character for the ridiculous and unreasonable purpose of exciting wonder and commiseration, such a motive, coupled with the youth's history and all the attendant circumstances, is well calculated to excite our suspicions as to his entire mental soundness. There were at the time, doubtless, others in the hospital more insane than this boy, and perhaps others, regarded as undoubtedly insane, who were fully as rational as he. A good deal of sanity may still exist among those who are too insane to mingle with the rational world.

Visitors, passing through the corridors of a hospital for the insane, often remark this fact, and the asylum-officers are often asked why certain patients are restrained of their liberty, because of their presenting to the casual observer so little appearance of insanity. So proper are the manners and conversation of patients, at times, that an unfounded distrust of public hospitals for the insane has sometimes been engendered in the public mind from this source—the insane themselves behaving so well in the presence of visitors, and at other times, under the present wise and scientific system of classification, medical and moral management, and restraint—the latter, in this country as in England, hardly exceeding non-restraint.

It would unduly lengthen this paper to detail the many instances which might be gathered of insane persons, on certain great occasions which made profound impressions upon their minds, having behaved with unaccustomed propriety, and having suspended for the time-being all manifestations of insanity, or to mention instances of great and unexpected events having been the beginning of recovery. Every one accustomed to the care of large bodies of the insane, knows that grand and unusual occurrences, which would startle and profoundly impress the rational mind, sometimes favorably impress the insane, notwithstanding that, in the main, they are either regarded with indifference, or cause aggravation of existing excitement. Some instances in point might be mentioned in connection with the burning of asylums. No such event has ever happened in my own experience, but the burning of a large stable and barn on the asylum premises at Fulton, early one evening, in full view of a good portion of the patients, gave opportunity to witness, in some degree, the different conduct of different lunatics under such circumstances. A very few of the patients were more excited than usual; the majority were indifferent; and some gave us valuable aid in our efforts to suppress the flames and save the stock.

The coercive methods of treatment, recommended and practised by Celsus, demonstrated the power of self-restraint in some cases, under the influence of overmastering fear; and in our own day, threats and punishments, though for obvious reasons neither practised nor advised as curative agencies, are not without some influence in subduing some refractory and noisy patients into submissive silence, as may be learned by visiting some of the almshouses and jails to which many of these unfortunates are yet consigned in the United States. It may be conceded that to be influenced by fear to such an extent as to suspend all display of insanity, is, in some cases, good evidence of feigning; but it must not be so regarded in all. Fodéré has been criticized, by a writer from whom I have already quoted,[1] for having relied upon the test of fear in the case of a female, who acted her part, if she were only acting,

[1] Dr. Chipley, American Journal of Insanity, July, 1865.

so perfectly that the doctor was on the point of certifying the case. He returned to her door, however, and said with a stern voice, "To-morrow I will visit her again, and if she continue to howl, if she be not dressed, and her chamber not put in order, you must apply a red-hot iron between her shoulders." He found things in order the next morning, and on this proof alone, with strong evidence to the contrary, immediately decided that it was a case of simulation. "But was this decision justified," asks the writer, "by the simple fact that the patient changed her conduct under the terror inspired by severe threats?" And he answers the question by referring to the self-control exercised by patients under the influence of fear, or the hope of reward. The potency of authority in suddenly suppressing through fear all appearances of mental derangement in those who are really insane, is, of course, exceptional; I can hardly conceive of such instrumentalities being successful in aggravated cases of general cerebro-mental disease, yet their occasional influence in controlling the less general forms of insanity, especially where simulation co-exists, is not to be doubted.

In hospitals for the insane, the power of one having authority is sometimes shown in the restraint which some patients put upon morbid and quasi-morbid displays, in the presence of the chief physician, and the arrest of paroxysms of hysteria by commands and threats, before the disease has reached the point of recognized insanity, is nothing new to the profession at large. This characteristic of partial control under ordinary circumstances, reaching the point of entire control under extraordinary circumstances, in cases of hysterical insanity, is too familiar to the profession to need further notice; it extends also, as I have endeavored to show, to other cases.

The power of self-restraint implies the power of simulation. Hysterical patients are undoubtedly more prone than others to "put an antic disposition on," as Hamlet has it, and are more disposed to act insanely than to exercise aright what power they really possess of at least restraining within more rational bounds the ridiculous displays which they often make.

The following case presents, I think, some interesting features:—

Reuben S., æt. 25, married, of limited education and of intellect below the average,[1] with a history of domestic infelicity, masturbation, and three weeks' insanity before admission, entered the Missouri State Lunatic Asylum on April 6, 1867, and was discharged, recovered, on November 15 of the same year. He had been an *attaché* of a sort of travelling circus, and theatrical and minstrel troupe, before the development of his insanity. After becoming insane, he had been seized with a sudden desire to preach, and, securing an audience in one of the country towns of Missouri, he conducted the services in such an obscene, blasphemous, and outrageous manner, that he was driven from the pulpit. His insanity becoming apparent to the neighborhood soon after, he was sent to the asylum.

At the asylum he endeavored to preach in the same way, but being put into an empty, quiet room, with a single window looking out on the lawn, and no audience, he soon grew tired of addressing vacancy, and changed his performances to tearing up his bedding and clothing, and soiling the walls and floor of his room. His occupation in the corridor changed to negro minstrelsy of the most ridiculously insane character, mingled with some really good hits at men and things which he had learned. He was fond of applause, and much

[1] I doubt this statement as to his natural intellect. His history was not given by near relatives or by those who had known him long.

encouraged when he excited laughter. The manner and intensity of his performances could be very much varied by our signification of approval or disapproval. He would seriously hurt himself, if permitted, in his attempted feats of ground and lofty tumbling. I never had the slightest doubt of his insanity, and have not to this day.

This patient's propensity for acting, after he had come to eat and sleep well, gradually wore off, but, during his convalescence, the least encouragement from officers or attendants would start him to performing. To keep him quiet, it was necessary that no notice should be taken of his antics, and the attendants were instructed accordingly. After his discharge from the asylum, he went to a distant part of the State, but soon returned to Fulton, and engaged in the business of a barber. He did not change his name, but denied ever having been in the asylum, though all the attendants knew him as our former patient, and often met him. He conducted his business properly enough, as far as I know, but the attendants who visited him, none of whom he would ever recognize, still thought him insane. It soon got noised about that he had been an inmate of the asylum, and he and his brother, who was also his partner, left for other parts.

There was no reason for believing this man to be wholly feigning. There were no charges of any kind against him, and no friends were interested in having him shielded from any punishment; moreover, he made several clever attempts to escape, at one time remaining away for a whole day before being recaptured. It is true that his performances were mainly given when he could get an audience, and were prompted by love of applause and notice; but he slept very little, when not narcotized, and occupied himself at all hours of the night in tearing his blankets, littering his room with straw, and doing every conceivable mischief, in addition to soiling his room and person. Before we had concluded to employ the necessary restraint, he was always doing something ludicrous or destructive, and the poor fellow's antics and appearance excited more laughter than any other patient on the male side of the house. When he came to the asylum he was extremely emaciated, indifferent to food, and, indeed, to everything except notice and applause. Egotism was constantly uppermost with him. His pulse was quick, and he was never known to be quiet while awake. As he convalesced he grew quiet and thoughtful, his pulse became slower, and he slept better, and improved in appetite and flesh.

This case was certainly a remarkable one: Could any conceivable motive have been found, we should have suspected simple feigning. The patient was evidently not aware that he was acting the part of a lunatic to an audience of insane persons, and in this respect the case differs from others, the simulator, sane or insane, generally appreciating, in some degree, the nature and extent of his acting and surroundings. Still the case serves to show the power which the insane possess of acting somewhat for a purpose, much after the manner of other people.

Though a mania for display is not uncommon with the insane, its appreciable modification by the applause or disapproval of those about the patient, would be taken, in most instances, for pure feigning without the co-existence of cerebral disease. Everything in this case but the one fact of the patient's acting most insanely when most observed, contradicted the notion of feigning. His physical condition, his antecedent history, the nature of his acts, and, after recovery, his changed demeanor, all pointed to the existence of real insanity.

I turn now briefly to a class of cases regarded by many as more devilish than lunatic, viz., the morally insane; who know how they are considered, and why they are in the asylum, and who take advantage of their reputed insanity to do many things from which they might perhaps refrain if they chose, but to which disease really prompts them. They

sometimes pretend to a degree of intellectual impairment not natural to them in their diseased condition.

These patients know where they are and why they are there, and to a certain extent are able to behave themselves with propriety when motives are powerful enough to induce them to do so, particularly under the absolute authority which they know that they have over them at the asylum—exercised, it is true, in the mildest and least irritating way, but usually with firmness and certainty. At home, with those whom they have been accustomed to manage, they are uncontrollable, and behave themselves like very devils, so that they get no sympathy. The world outside does not understand them, and cannot get along with them, and indeed those in charge of asylums wish that these patients could be cared for elsewhere.

With distorted views of men and things about them, just short of delusion; dissatisfied and suspicious, often without the shadow of a cause; sometimes devoid of affection and gratitude; sleeping and eating poorly; never in a state of mental composure, but always quarrelling with the cook, the laundress, the attendants, or some fellow-patient; with bowels often habitually constipated, circulation disturbed, and general appearance of ill health, if these patients are insane—and I think that they are—they can also simulate to an aggravating degree, when it serves their purpose, abnormal mental states not actually existing in them.

The possibility of self-control under exceptionally powerful external influences coming to the aid of the will of persons reputed to be insane, does not preclude all possibility of the existence of real insanity. A more or less modified power of self-control exists in the incubative stage of most forms of insanity, even under ordinary circumstances, and persists oftentimes until the case has passed into the form of final and hopeless dementia. After recovery, our patients sometimes tell us how they resisted morbid promptings to extravagant words and conduct, before these found unwilling and resistless expression. Suicides and homicides by the insane are seldom without premonitory preparations, threatenings, and warnings, and much of the freedom of intercourse, coupled with safety, of asylum-life is due to this fact. Even in cases of well-advanced insanity, morbid impulses of various kinds are sometimes long and repeatedly resisted before their final consummation.

The power to restrain and suppress insane promptings, so frequently exhibited by the insane, implies the power to simulate the features of insanity. In all asylums for the insane, there are chronic cases in which the original delusion, though still persisting, ceases to dominate the patient as in the beginning—cases in which the aggravated demonstrations and exclamations of the patient are disproportionate to his delusion. The insane man's oft-repeated actions and expressions become a sort of second nature, as habit becomes with the sane. There is a kind of automatic simulation here; these patients act out a state of feeling which is not real to them. If once tormented with a painful delusion, they cease to suffer as acutely as in the beginning, if they suffer at all, and their attention is more easily diverted from self.

Our asylums afford many examples in which the patients, yielding more or less to their delusions, also more or less modify or suspend the expression of them. This fact is illustrated and recognized in the moral treatment of the insane. Even sane persons are not free from dissimulation, and while insanity in the main reverses the natural charac-

ter, it sometimes throws off the natural disguise put upon the conduct and conversation in health; thus we may find the natural disposition to dissemble, in not very profound forms of mental disease, displaying itself less guardedly than in health. Hysteria is an apt illustration of this fact, both before and after it becomes actual insanity.

The insane, like the sane, though on the whole more truthful than the latter, are not always and altogether what they seem in acting out their real psychic impulses. They are sometimes more crazy than they appear, and sometimes appear more insane than they really are. We have generally to scrutinize an insane character closely to fully comprehend it. Esquirol's estimate of the value of constant observation, in order to become familiar with the subject, expressed very nearly the truth: We must live with them to fully know them. Simulation of insanity may exist in connection with actual psychical defect, the result of disease of the brain, just as disease of the stomach, lungs, or other organ, may be accompanied with a greater or less degree of healthy function. Only death obliterates and destroys all function, while disease, short of death, modifies and perverts organic activity.

Physical disease, involving the so-called physical organs in contradistinction to the organ of the mind, is not always or usually entire physical destruction; and so disease involving the mind and its organ, is not always or usually entire mental overthrow or obliteration; rather is the latter the exception, than the rule. Do we not, therefore, err, if we seek to find in every case of apparent mental aberration all simulation, or all insanity? Is there anything in the nature of insanity, as we recognize its different forms, incompatible with simulation? I think not, except it be in profound and advanced general mania and dementia; and from this cursory survey of a field which grows wider as we view it, I conclude that it is not only not impossible for the insane to simulate insanity, for a purpose, in any but its graver forms of profound general mental involvement, but that they do sometimes actually simulate acts and forms of insanity for which there exists no pathological warrant that can be discovered in the real disease by which they are affected.

Discussion on Dr. Hughes's Paper.

After the reading of the preceding paper, Dr. Isaac Ray, of Philadelphia, said:—Until the present century, insanity was seldom put forth as an excuse for crime. Its frequent occurrence in our day has made it a duty of physicians to make themselves acquainted with its phenomena far more accurately than was before required. It often becomes their duty to say, in a matter of life or death, whether a certain person is sane or apparently insane, and, if the latter, whether the apparent insanity is real or simulated. Most of the insane know as well as other people that insanity is an excuse for crime, and there is no reason why they may not strive to use it as such, when occasion calls, by feigning some manifestations of the disease over and above those belonging to their own particular form of the malady. To do this requires no more shrewdness and self-command than it does to conceal their delusions, as they sometimes do. The fact that they are already insane does not preclude the need of simulation, for the well-known reason that the insane generally do not recognize their own infirmity. To them, the need of the excuse seems just as strong and just as apparent as it would to others.

Dr. Hughes's mention of Dr. Bell's case calls to mind a form of mental dis-

order, not unfrequently met with in general practice, in which the real and the simulated are curiously mingled together. It occurs mostly in women, beginning usually in a morbid fondness for sympathy, and for the attentions bestowed on the sick and suffering, and originating either in some hysterical condition, or in a decidedly insane temperament. There is nothing which persons thus disposed will not endeavor or endure, and the aches and ails which they affect, from a simple cough to fits and dislocations, no man can number. We are all familiar with them, no doubt. I knew of one who, not content with keeping a couple of watchers up all night for months together, would, when at all displeased, dislocate her jaw; and another who would, under similar circumstances, dislocate her hip. In such cases there is, undoubtedly, much mental disease, and also much downright simulation. And it is not always easy to determine what is to be attributed to the one, and what to the other, while a mistake may lead to great wrong and suffering. I call to mind the case of a young girl of some culture and refinement who kept her bed more than a year, unable to move without assistance, and regarded by friends and physicians as extremely ill. At last some one, walking in the garden, observed her, through a window in her room, rise from the bed and jump about as lively as a cricket. Of course there was an end to this sort of performance, but there soon appeared a good deal of mental disorder, which finally made her so troublesome that she was placed in our hospital. There, for weeks together, she behaved with the utmost propriety, evincing neither in conduct nor in conversation the slightest sign of insanity. This condition would alternate with periods when she lay in bed, unconscious apparently of everything around her, her face highly flushed, and her pulse fast and full. She took food only through the tube, and passed her evacuations in bed. From us she passed into the charge of an Indian doctor, by whom, it was said, she was cured. Some light is thrown on the psychological history of this case by the fact that a brother and sister were spiritualists, and that her mother presented many indications of the insane temperament. Now, had this young woman committed a criminal act, I fear that the plea of insanity would have availed her little in view of this instance of detected simulation.

ON THE BEST MODE OF PROVIDING FOR THE SUBJECTS OF CHRONIC INSANITY.

BY

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MAN lives by reason; the brute by instinct. The greater size and complexity, and the higher organization, of the organs of reason, render them more susceptible to derangement than those of instinct; while the complex necessities, motives, and struggles, which belong to a life of reason, evolve the numerous exciting causes of such derangement just in proportion as the reasoning being develops his pre-eminent intellectual powers, and puts his feeble instinct into abeyance. When reason or motive is perverted by cerebral disorder or defect, man is both a dependent and an aggressive being. The utter incapacity to provide for himself which causes him to be regarded as an infant, in the eye of the law, is more apparent because of its most frequent occurrence at a time of life when he is most independent in his normal powers, and when both his absolute and artificial wants are most numerous and pressing. Intensity of purpose and action is often in direct ratio to perversion of judgment or motive, and the insane man is not only disqualified for providing for his numerous wants, but has the inclination and power, if left to himself, to be exceedingly dangerous to life and property.

Insanity is a term used to express the mental symptoms of a physical disease or defect which generally admits of partial relief, and which often admits of entire cure. If cured, the subject of the disease regains all his inestimable, human prerogatives—the ability to mingle in society, and freedom to will and to do of his own good pleasure. If relieved, as far as each case admits of relief by the diligent and skilful use of the appliances of modern science and benevolence, a life of sickness and privation is rendered comfortable and often measurably happy. Such a helpless, dangerous, privative, and yet remediable when not curable, infirmity, appeals not only to the deepest sympathies, but to the self-interest of the whole race among which it prevails. Insanity is everywhere regarded as the chief ill to which flesh is heir; and it is the sentiment of enlightened Christendom, however imperfectly and variously that sentiment may be expressed, that the insane of all classes should have all their needs provided for, because they cannot provide for them themselves; should have the medical and moral treatment that will alleviate, if not cure, their grievous disease; and should, as a rule, be restrained of their liberty, because it is unsafe for them to be at large.

If not cured, the pathological conditions of the brain which give rise to mental derangement, rapidly pass into a chronic condition, whose average duration is about equal to one-half of the mean length of human life. The result of efforts to establish by observation and pathological research the relations between the duration of mental disease and its curability, is the practice of classifying cases of not more than one year's duration as recent and presumptively curable, and those of more than

one year's standing as chronic and probably incurable. Experience shows that this generalization is useful in estimating the tendencies in respect to recovery, or duration of illness, of large masses of the insane, and the provisions required for their proper maintenance and treatment; but when it is considered that, under the circumstances most favorable to recovery, about fifty per cent. of all the cases of insanity that occur in a community pass this arbitrary boundary-line which has been set to divide recent from chronic mental disease, while the rapidly diminishing probabilities of recovery are not found to be extinct in individual cases until the expiration of the third or fourth year after the invasion of the malady, it will be seen that such a generalization can be only an approximation to the truth, with many exceptions. As the recent insanity of a community imparts an increment of not far from fifty per cent. of all the cases that annually occur, to those of technically chronic mental disorder, while an uncertain period of several years expires before individual cases become hopeless, the two classes are, for a considerable period at least, inseparably linked with each other, and either a philosophical or a practical consideration of "the best mode of providing for the subjects of chronic insanity," should obviously form a harmonious part of a comprehensive scheme of providing for the treatment of insanity of every duration and form.

In the year 1866, the Association of Medical Superintendents of American Institutions for the Insane,[1] a body of the highest acknowledged authority in America in relation to the wants and claims of the insane, after an exhaustive discussion of the exact subject of this paper, adopted the following five propositions, the first four unanimously, and the last one by a considerable majority of the members present at the meeting of that year:—

I. The large States should be divided into geographical districts of such size that a hospital, situated at or near the centre of each district, may be practically accessible to all the people living within its boundaries, and available for their benefit in case of mental disorder.

II. All State, county, and city hospitals for the insane should receive all persons, belonging to the vicinage designed to be accommodated by each hospital, who are affected with insanity proper, whatever may be the form or nature of the bodily disease accompanying the mental disorder.

III. All hospitals for the insane should be constructed, organized, and managed, substantially in accordance with the propositions adopted by the Association in 1851 and 1853, and still in force.[2]

[1] Including those of the United States of America and of the Provinces of the Dominion of Canada.

[2] Propositions offered by Dr. Kirkbride, and adopted at the meeting of the Association of Superintendents held in Philadelphia, May 21, 1851:—

(1) Every hospital for the insane should be in the country, not within less than two miles of a large town, and easily accessible at all seasons.

(2) No hospital for the insane, however limited its capacity, should have less than fifty acres of land devoted to gardens and pleasure grounds for its patients. At least one hundred acres should be possessed by every State hospital, or other institution for 200 patients, to which number these propositions apply unless otherwise mentioned.

(3) Means should be provided to raise ten thousand gallons of water, daily, to reservoirs that will supply the highest parts of the building.

(4) No hospital for the insane should be built without the plan having been first submitted to some physician or physicians who have had charge of a similar establishment, or who are practically acquainted with all the details of their arrangements, and having received his or their full approbation.

(5) The highest number that can with propriety be treated in one building is 250, while 200 is a preferable maximum.

IV. The facilities of classification or ward separation possessed by each institution, should equal the requirements of the different conditions of the

(6) All such buildings should be constructed of stone or brick, should have slate or metallic roofs, and, as far as possible, should be made secure from accidents by fire.

(7) Every hospital having provision for 200 or more patients, should have in it at least eight distinct wards for each sex, making sixteen classes in the entire establishment.

(8) Each ward should have in it a parlor, a corridor, single lodging-rooms for patients, an associated dormitory communicating with a chamber for two attendants, a clothes-room, a bath-room, a water-closet, a dining-room, a dumb-waiter, and a speaking-tube leading to the kitchen or other central part of the building.

(9) No apartments should be provided for the confinement of patients, or as their lodging-rooms, that are not entirely above ground.

(10) No class of rooms should ever be constructed without some kind of window in each, communicating directly with the external atmosphere.

(11) No chamber for the use of a single patient should ever be less than eight by ten feet, nor should the ceiling of any story occupied by patients be less than twelve feet in height.

(12) The floors of patients' apartments should always be of wood.

(13) The stairways should always be of iron, stone, or other indestructible material, ample in size and number, and easy of ascent, to afford convenient egress in case of accident from fire.

(14) A large hospital should consist of a main central building, with wings.

(15) The main central building should contain the offices, receiving rooms for company, and apartments, entirely private, for the superintending physician and his family, in case that officer reside in the hospital building.

(16) The wings should be so arranged that if rooms be placed on both sides of a corridor, the corridor should be furnished at both ends with movable, glazed sashes, for the free admission of both light and air.

(17) The lighting should be by gas, on account of its convenience, cleanliness, safety, and economy.

(18) The apartments for washing clothing, etc., should be detached from the hospital building.

(19) The drainage should be under ground, and all the inlets to the sewers should be properly secured, to prevent offensive emanations.

(20) All hospitals should be warmed by passing an abundance of pure, fresh air, from the external atmosphere, over pipes or plates containing steam under low pressure, or hot water, the temperature of which at the boiler does not exceed 212 degrees Fahr., and which are placed in the basement or cellar of the building to be heated.

(21) A complete system of forced ventilation, in connection with the heating, is indispensable to give purity to the air of a hospital for the insane, and no expense that is required to effect this object thoroughly, can be deemed either misplaced or injudicious.

(22) The boilers for generating steam for warming the building, should be in a detached structure, connected with which may be the engine for pumping water and for driving the washing apparatus and other machinery.

(23) All water-closets should, as far as possible, be made of indestructible materials, be simple in their arrangement, and have a strong downward ventilation connected with them.

(24) The floors of bath-rooms, water-closets, and basement-stories, should, as far as possible, be made of materials that will not absorb moisture.

(25) The wards for the most excited class should be constructed with rooms on but one side of a corridor not less than ten feet wide, and the external windows should be large, and should afford pleasant views.

(26) Wherever practicable, the pleasure grounds of a hospital for the insane should be surrounded by a substantial wall, so placed as not to be unpleasantly visible from the building.

The following propositions, also offered by Dr. Kirkbride, were adopted at a meeting of the superintendents held in Baltimore, May 10, 1853:—

(1) The general controlling power of a Hospital for the Insane should be vested in a board of trustees or managers: if of a State institution, selected in such manner as will be likely most effectually to protect it from all influences connected with political measures or political changes; if of a private corporation, by those properly authorized to vote.

(2) The board of trustees should not exceed twelve in number, and should be composed of individuals possessing the public confidence; distinguished for liberality, intelligence, and active benevolence; above all political influence; and able and willing faithfully to attend to the duties of their station. Their tenure of office should be so arranged, where

several classes received by such institution, whether these different conditions be mental or physical in their character.

V. The enlargement of a city, county, or State institution for the insane, which, in the extent and character of the district in which it is situated, is conveniently accessible to all the people of such district, may be properly carried, as required, to the extent of accommodating 600 patients, embracing the usual proportions of curable and incurable insane in a particular community.

No one of these propositions has since been either repealed or modified, and the experience of the ten years which have elapsed since they

changes are deemed desirable, that the terms of not more than one-third of the whole number shall expire in any one year.

(3) The board of trustees should appoint the physician, and, on his nomination, and not otherwise, the assistant physician, steward, and matron. It should, as a board or by committee, visit and examine every part of the institution, at frequent, stated intervals, not less than semi-monthly, and at such other times as it may deem expedient, and should exercise so careful a supervision over the expenditures and general operations of the hospital, as to give to the community a proper degree of confidence in the correctness of its management.

(4) The physician should be the superintendent and chief executive officer of the establishment. Besides being a well-educated physician, he should possess the mental, physical, and social qualities to fit him for the post. He should serve during good behavior, and should reside on, or very near, the premises, and his compensation should be so liberal as to enable him to devote his whole time and energies to the welfare of the hospital. He should nominate to the board suitable persons to act as assistant physician, steward, and matron; he should have the entire control of the medical, moral, and dietetic treatment of the patients, and the unrestricted power of appointment and discharge of all persons engaged in their care; and should exercise a general supervision and direction of every department of the institution.

(5) The assistant physician or physicians, where more than one are required, should be graduates of medicine, of such character and qualifications as to be able to represent and to perform the ordinary duties of the physician during his absence.

(6) The steward, under the direction of the superintending physician and by his order, should make all purchases for the institution; keep the accounts; make engagements with, pay, and discharge those employed about the establishment; have a supervision of the farm, garden, and grounds; and perform such other duties as may be assigned him.

(7) The matron, under the direction of the superintendent, should have a general supervision of the domestic arrangements of the house, and, under the same direction, do what she can to promote the comfort and restoration of the patients.

(8) In institutions containing more than 200 patients, a second assistant physician and an apothecary should be employed, to the latter of whom, other duties, in the male wards, may be conveniently assigned.

(9) If a chaplain be deemed desirable as a permanent officer, he should be selected by the superintendent, and, like all others engaged in the care of the patients, should be entirely under his direction.

(10) In every hospital for the insane, there should be one supervisor for each sex, exercising a general oversight of all the attendants and patients, and forming a medium of communication between them and the officers.

(11) In no institution should the number of persons in immediate attendance on the patients be in a lower ratio than one attendant for every ten patients; and a much larger proportion of attendants will commonly be desirable.

(12) The fullest authority should be given to the superintendent to take every precaution that can guard against fire or accident within an institution, and to secure this an efficient night-watch should always be provided.

(13) The situation and circumstances of different institutions may require a considerable number of persons to be employed in various other positions; but in every hospital, at least all those that have been referred to, are deemed not only desirable but absolutely necessary, to give all the advantages that may be hoped for from a liberal and enlightened treatment of the insane.

(14) All persons employed in the care of the insane should be active, vigilant, cheerful, and in good health. They should be of a kind and benevolent disposition, should be educated, and in all respects trustworthy, and their compensation should be sufficiently liberal to secure the services of individuals of this description.

were adopted, has fully established their sonndness and practicability (except, possibly, in respect to the last, which fixes an arbitrary maximum number of patients that may be accommodated in one institution), not less than ten States and one Province, if not more, having proceeded to provide for their insane upon the principles which they enunciate, and to embody them in their statutes. As I have intimated, the best provision for the subjects of chronic insanity should be embraced in a comprehensive plan of providing for the insane of all classes. The above propositions were intended to embrace such a plan, and, as I was their author, it will suit my convenience in the treatment of this question to attempt to reproduce the considerations that led to the views which they express: and perhaps this mode of presenting the matter will be more satisfactory to the Section, and more useful to the public, than any other. I propose to consider the propositions *seriatim*, and to lay particular stress upon the bearing of each upon the care of the subjects of chronic insanity.

The first proposition declares that "the large States should be divided into geographical districts of such size that a hospital situated at or near the centre of each district, may be practically accessible to all the people living within its boundaries, and available for their benefit in case of mental disorder." The obvious purpose of this is to affirm that institutions for the insane must be within the reach of the people whom they are intended to accommodate, in order that their insane may be sent to them; and it was framed in view of the fact that many hundreds of persons suffering from acute insanity had not received the hospital treatment that had been provided for them, and had consequently run into the hopeless stages of the disease, because of the long, fatiguing, and expensive journeys necessary to reach either the nearest hospitals, or those to which they were entitled to go. It is obvious that the principles of this proposition lie at the very foundation of an adequate provision for the insane, whether the form of their disease be recent or chronic. In an article on "The Use of Insane Hospitals," in the number of the Journal of Insanity for January, 1866, by Dr. Edward Jarvis, will be found a table which shows that, in the course of a series of years, twenty-two institutions in the United States and the Dominion of Canada received annually an average of one patient to 3974 of the population of the districts in which they were situated, and that the ratio of patients to population constantly and rapidly diminished in every case as the distances from the institutions increased, until an average of only one patient in 18,978 of the population of the fourth tier or belt of counties from the hospital centres, was sent to them. It is not reasonable to suppose that institutions for the insane breed insanity in the populations immediately surrounding them, nor that insanity is of more frequent occurrence in their near neighborhood than in more remote districts, and hence the only reasonable conclusion is that the remote districts were, solely on account of their remoteness, to a great extent deprived of the use of hospitals. This less use of hospitals by distant districts, is doubtless in some part due to the less acquaintance with their management and benefits than is acquired by people situated nearer to them, but in greater part to the disinclination of public authorities, and the inability of friends, to incur the expenses of transporting patients long distances with the necessary escorts, and to the apprehended danger to the lives of delicate persons from the exposures and fatigues of a protracted journey.

It follows, as Dr. Jarvis says, that hospitals are somewhat local in their operation, and that, in order that their benefits may be enjoyed in case

of need, they must be, in the language of the proposition, practically accessible to all the people residing within the boundaries of the district which each is intended to accommodate. A necessity so vital to the welfare of the people, becomes the bounden duty of the State. No rule limiting hospital districts to an exact and uniform area can be laid down. Indeed, it would not be possible to divide the States and Provinces in such a way that a central institution should not be more or less difficult of access from one or more remote corners of the district which it was intended to accommodate, but intelligent legislators will in most cases be able to approximately meet the requirements of the proposition, if they are imbued with a sense of its vital importance to the needs of an afflicted class of their constituents. In districting States and Provinces having large areas of unsettled, or partially settled, territory, reference should be had to the probable direction and extent of the growth of their populations, lest a sufficient number of institutions for the whole area of the State or Province should be so placed in the original centres of population as not to accommodate the subsequent settlements. Indisputable facts show that the remote inhabitants of a district must be able to reach its hospital by a journey that can be made during the waking hours of a single day, or they will not avail themselves of its benefits. Such a generalization will aid in the practical solution of this problem, for it will readily be seen that insane persons can be conveyed to a hospital by railroad from a distance of seventy-five or a hundred miles with more convenience and less expense than they can be conveyed half that distance by ordinary carriage-roads, and that the number and directions of the railroads and the character of the carriage-roads of a proposed district, should have much influence in determining its size and shape, and the situation of its hospital. The practicability of the early treatment of acute insanity will increase the number of recoveries, and proportionately diminish the number of cases of chronic insanity to be provided for. This brief discussion of this first and fundamental element of all adequate public provision for the insane, is, therefore, pertinent to the particular question under consideration.

I now come to the second proposition of the series. It is that all State, county, and city hospitals for the insane, should receive all persons that belong to the vicinage designed to be accommodated by each hospital, who are affected with insanity proper, whatever be the form or nature of the bodily disease accompanying the mental disorder. Except in some of the New England States, the ordinary poor of the United States are generally maintained in county alms-houses, and, in harmony with that practice, and mainly from mistaken motives of economy, most county authorities are disposed to provide for their insane poor as well as for their other dependent classes. The populous and wealthy counties, whose insane are numbered by hundreds, and are sufficient to fill one or more institutions of suitable size, may properly establish and maintain hospitals of their own, and thus save the cost of transportation, and secure the benefits of local employment and trade, and of easy access. It may with reason be maintained that as State legislators and officials are from their position, if not from their personal character, likely to entertain broader and more liberal views of their duty to the sick and unfortunate, and to be less under the influence of petty local strifes and parsimonies, than the officials of the subordinate municipalities, the States and Provinces should take direct charge of this class of their dependents, so peculiar in its wants and in its liability to suffer from neglect or abuse. There are

several county and city institutions the material appointments and management of which are equal to those of the average State and Provincial asylums, and it is due to truth and professional character to declare, in this public manner, that the unsatisfactory condition of others is attributable to defective means and organization, and not to the very respectable medical gentlemen who now have, or have had, charge of them. Without exception, these gentlemen appear to have discharged their duties with humanity and ability, and with very remarkable industry and perseverance in view of the difficulties and discouragements that have attended their work. City institutions for the insane are subject to the same objections as those of the county, and to the additional objection that a city will seldom, if ever, be able to afford within its limits the area of ground necessary for salubrity, privacy, and cultivation; and rather than that a city should go outside of its territory to establish and maintain an institution for its own exclusive benefit, it would be more feasible for the common authority over the city and neighboring municipalities to take charge of this sacred function of government, and administer it for their equal benefit.

To effectively prevent the peculiar abuses that are liable to creep into institutions for the insane governed by local authorities, State and Provincial statutes, with heavy penalties for their violation, should require them to be constructed, organized, and conducted, substantially as the State and Provincial hospitals or asylums are usually appointed and maintained; and should especially require them to receive, whenever the friends may desire it, the insane of the favored classes in respect to property, either as pay patients, or as free patients, supported, like all others, from the common fund of the municipality. One of these courses is pursued by all the State institutions, and there is no one feature of their organization that is as effective as this in preventing their management from falling below a liberal, curative standard. The poor will generally be well taken care of when they receive such treatment as the middle classes in respect to fortune are willing to pay for, or are satisfied with without payment, except indirectly as taxpayers. The rich will generally be sent to the corporate or private institutions, all the inmates of which are independent, and can more conveniently receive in them such indulgences as their habits and tastes require, and their means allow, than they can in large institutions, most or all of the inmates of which are a public charge. The second proposition, in theory at least, solves the question before the Section. The reception and care by every public hospital for the insane of all persons belonging to the vicinage—that is, within the hospital district—who are affected with insanity proper, includes, of course, the reception of those whose insanity is chronic, and this is believed to be the best of all the provisions for their care which have been, or are, or possibly can be made, or which have been suggested or advocated.

The movement of the insane, under this scheme, will be simply as follows: The recently insane are promptly placed in district hospitals, and there receive the treatment most conducive to their comfort and recovery. About one-half of them recover, and return to their homes and employments, or die, in the course of the year that follows their admission; the other half remain where they are without additional expense for transportation, and continue to receive care and treatment adapted to the condition of each case. Those who still give promise of improvement, are treated with that end in view, and occasionally reward perse-

vering efforts in their behalf by entire recovery of reason; those who continue to be actively maniacal, or melancholic, or who exhibit suicidal, homicidal, or other specially dangerous propensities, and the paroxysmal cases, whether simple or epileptic, receive the care that secures to each case the highest degree of comfort and safety of which it is susceptible, *and that is as necessary now as in the acute stage of the disease.* A few, perhaps ten per cent., of the annual additions to the chronic members of the hospital family, pass early into the mechanical, but not always unhappy, life of passive dements, whose bodily habits are regulated, and whose comforts are ministered to, by the order and appliances of the institution, while they engage, with more or less interest in its character and results, in such labor as they are capable of, in the wards, kitchens, laundry, and stables, and in the gardens and fields, when the sun is not too hot for their weak brains, nor the air too cold for their feeble circulations. The subjects of chronic insanity render considerable assistance in the wards occupied in part by the recent cases, and somewhat reduce the number of attendants that need to be employed; while the labor of the insane is mostly performed by the chronic patients, the recovery of those who have been recently affected is occasionally much promoted by industrial exercise, and they will here and there fall into a party of regular workers with more facility than they will work either by themselves, or in company with others whose cases are entirely of their own class. The habitual employment of a large number of patients is calculated to suggest and justify a variety of occupations, among which the recently affected may find those adapted to their respective capacities. The insane may be much benefited by labor, and may also be greatly injured by it: compulsory labor would cause the rapid and cruel sacrifice of many of the insane, both of the acute and chronic classes. The condition of the brain and the probable effects of labor upon it, should be carefully considered before each patient is put to work, and the effects of his work as carefully watched. The medical judgment that prescribes and regulates the labor of the insane, should be as able and critical as that which prescribes the drugs that are administered to them; and a medical staff accustomed to consider the conditions under which a large number of subjects of both recent and chronic insanity may labor without detriment, if not with advantage, will become more acute and just in its discriminations, and less liable to fall into injurious errors, than a staff of less experience in this particular. The extreme vicissitudes of the American climate, with its fierce summer-heats and depressing winter-colds, seem to admit of less labor by the insane in the open air than the more equable climate of Europe. If the disease be more active here than there, as many represent, our insane cannot advantageously work as much, either in-doors or out, as theirs, and yet, if such a sense of the importance of labor as a sanitary measure in treating the insane, especially of the working classes, prevailed here as appears to prevail abroad, the *systematic* employment of our patients would probably become a constant feature in the administration of our hospitals, with an improvement in the health, contentment, quietude, cheerfulness, and happiness of their inmates.

The scheme of providing for the insane of all classes in institutions situated at or near the centres of limited districts, renders it practicable for the kindred of the chronic patients to visit them occasionally, and, in addition to the immediate happiness which such visits confer, opens the way to the return of a portion of the subjects of passive, settled de-

mentia to the care and support of their families. The practice of furloughing patients, which obtains among the English asylums, has been resorted to, to a limited extent, in this country and the Provinces. If it should here become an approved measure, either of treatment or of relief of over-crowded institutions, it will be practicable and frequently resorted to only under the district system. A patient's return on furlough to his home, one or two hundred miles from the hospital, must in practice prove equivalent to a final discharge, both from the custody and from the oversight of the institution. The subjects of chronic insanity, to whom the habits of the hospital have become a second nature, are efficient aids in maintaining its discipline. Most of the recent patients, admitted one by one, unconsciously imitate the companions with whom they are thrown, and readily fall into the order of the hospital, without friction, and without special instruction or exercise of authority which many would excitedly, if not violently, repel.

In America, the insane of every degree of culture and refinement, and of ignorance and rudeness, as well as of every nationality, are received into all our institutions. The character of the institution into which a patient is sent, is mainly determined by his means, or those of his near friends who may aid him in his extremity. Each class has its conditions of happiness: The first for every class, is society after its kind; after that, the cultivated demand articles of taste and delicacy, which are an inconvenience to the ignorant; the ignorant demand the substantial conditions of physical comfort and health, which, by themselves, are bare, deficient, and depressing to the refined. After insanity has been under treatment for a year, those cases that are complicated with paralysis and other indubitable evidences of organic cerebral disease, may be classed with the cases of four or more years' duration, while cases which exhibit no evidences of organic disease may still be classed among the hopeful, and should be treated as such; but beyond the rule, which should be inflexibly adhered to, that the violent and turbulent should be entirely separated from the quiet and orderly, all rules of classification in American institutions are, and should be, regarded as generalizations which afford much aid in a proper distribution of the inmates of each establishment, but from which there must be constant departures in every effort to secure the highest comfort and the utmost alleviation of which individual cases are susceptible. As a general rule, epileptics should be placed in one or more wards by themselves, but it would be an unnecessary cruelty to subject a person of refined sensibilities, whose paroxysms are light and infrequent, to the constant observation of the painful scenes of an epileptic ward; such a patient may often be an agreeable and useful associate of the convalescents. On the other hand, culture is sometimes brutalized by dissipation and disease, and, though not physically violent, the possessor of early social and educational advantages is sometimes a most unfit associate for real gentlemen and ladies. It must be obvious, and this is the lesson of experience, that such a critical classification can be carried into more satisfactory details in the treatment of the collected insane of a community, than in the exclusive treatment of either class, or of a single division.

In estimating the necessary conditions of the best provision for the subjects of chronic insanity, it should be borne in mind that this is precisely the same disease in its nature and phenomena as recent insanity: they differ only in duration, in the proportion of cases of passive dementia, and in the prospect of restoration. It follows that the active

forms of chronic, mental disease require precisely the same treatment as the recent. The protection of society and of the individual, and the mitigation of the pains and privations of disease, are as much demanded in the case of the chronic maniac, whether his mania be constant, recurrent, periodical, or epileptic, as in that of the recent; and it stands to reason that a medical staff, constantly accustomed to treat recent insanity with the expectation of curing it, is likely to be better prepared, both by knowledge and habit, to treat chronic disease for its alleviation, than one whose professional efforts in the exclusive treatment of chronic disease are very rarely rewarded by a full restoration.

The theoretical grounds of the rule that all public institutions should receive the subjects of both recent and chronic insanity, are sustained by the crucial test of experience. Nearly every institution for the insane in Christendom is to-day occupied by cases of every variety, duration, and manifestation; from the last admitted, in which the outbreak occurred but a short time ago, to the most hopeless case of fatuity. The exceptions are a very few—not, I think, as many as half a dozen altogether—which are occupied exclusively by chronic and presumptively incurable inmates; I know of none which receives only recent cases and discharges them as soon as they become chronic. The presence of recent and chronic cases in all institutions is, of course, due in part to the retention of cases which were admitted when recent, and which have passed to the chronic state; but it is by no means wholly due to a circumstance so natural and purposeless. Not only was the subject of the care and treatment of the victims of chronic insanity fully discussed, both in Great Britain and on the continent of Europe, long before the question became an urgent one in this country, but, after trying various experiments in separating, to a greater or less degree and in various ways, the acute from the chronic cases, the Europeans have all, I understand, come back to what in England is called the *public-asylum*, and here the *State-asylum, or hospital, system.* In fact, if the public institutions for the insane in the United States and the Provinces of the Dominion were to be distinguished by the character of their inmates in the respect under consideration, they should be styled asylums for the subjects of chronic or incurable insanity, and if the recent and chronic cases were to be separated, the most numerous class (the chronic) would naturally take the present institutions, which are much too large for the recent, and the pertinent and pressing question of the day would then be *the best provision for the recent or acutely insane.* Estimates kindly furnished me by 27 superintendents of the United States, show that, on an average, $12\frac{1}{2}$ per cent. of the cases of insanity in 22 States are of less than one year's duration, and the remainder chronic. Five of their superintendents estimate the average ratio of recent to chronic cases of insanity in the Provinces of Canada to be $7\frac{8}{10}$ per cent. As only five of the States make full provision for all their insane, in institutions having a resident medical head, it may be that the ratio of recent to chronic cases, under treatment in them, is a little greater than in the whole insane population of those States; but I am more inclined to think that the estimates of recent cases are too high, and will not be borne out by the results of treatment. The proposition is not only sustained by the practice of the institutions of both the old and new worlds, but by the authority of the ablest and most experienced men in the specialty; among readers of the English language the names of Dr. Bucknill and Dr. Robertson, of England, and Dr. Earle, of this country, are best known in this connection.

Perhaps a full consideration of the plan of providing for the subjects of chronic insanity, now as fully set forth as the time allowed me will permit, should embrace answers to the objections that have been, or may be, made to it. It has been said that the retention of chronic cases in the hospitals in which they have been placed, brings no direct relief to the wretched lunatics in the county alms-house receptacles. It does not, except by arresting all additions to their number, which would certainly be a great and happy achievement; but the objection is answered by a simple declaration of the obvious truth, that it is less expensive and in every way more feasible to provide for the alms-house insane in district institutions than in any other proper way. It will cost less for the necessary enlargement of buildings and their appointments, and for transportation. If a sufficient number of district institutions already exist, they can be enlarged both so as to retain all the patients whom they do not cure, until they die, if their condition require it, and to receive the alms-house patients at less expense than would be incurred by providing for them either independent central, or district, asylums of a proper kind; and if there are not enough district institutions to make them practically accessible to all acute cases, they certainly should, for reasons that have been given, be provided as rapidly as possible with accommodations for all the chronic cases of the vicinage.

It has been said that the States (I do not recollect that it has been said of the Canadian Provinces) will not incur the outlay necessary to provide institutions with such appointments and management as are deemed necessary for recent cases; but this declaration is shown to be unfounded by the activity and liberality which are displayed at this moment by the greater number of the leading States, in establishing excellent district institutions for all their insane, of both the acute and chronic classes. Five States have provided for every case within their borders not preferably provided for by friends, and others will soon have discharged the same duty. An editorial table in a recent number of the American Journal of Insanity shows that the total present and prospective capacity of the institutions for the insane in the United States is for 35,325 inmates, at a cost, according to a table prepared by Dr. Conrad, of the Maryland Hospital, of at least $35,000,000, with an annual expenditure, at this time, for maintenance, of $5,000,000. This has mainly been accomplished in the last half of that century of our national existence which has just terminated, scarcely as many as the odd 325 of the number now under treatment having been provided for fifty years ago. I am sorry to be compelled to acknowledge that there is not yet sufficient hospital provision for the insane in the United States, and that in a few instances that which exists is seriously defective in material appointment and organization; but I cheerfully submit the work of the first century of the Republic in the personal care and legal protection of the insane, to the judgment of our impartial psychological brethren abroad, with much confidence that it will be deemed to be indicative of a high degree of enlightenment and liberality on the part of the American people who have sustained it with their means; of true science and humanity on the part of American psychologists; and of genuine Christian benevolence on the part of the noble men and the philanthropic woman,[1] who have executed it with the best efforts of their heads and hearts.

As the third, fourth, and fifth propositions relate to the means and

[1] Miss D. L. Dix.

modes which render the approved and prevailing plan of providing for the subjects of chronic insanity practicable and successful, I shall briefly consider them together. The propositions of 1851 and 1853, referred to in the series of 1866, and quoted in the foot-note to page 1128, express in considerable detail the essential conditions, both material and administrative, of the successful treatment of the insane, and, being of acknowledged authority, have prevented many costly errors of ignorance in establishing the institutions of the country, and have efficiently promoted the general uniformity and excellence of their management. With the experience of the quarter of a century during which those propositions, 40 in number, have been in force, they are, with slight modifications, considered as judicious and binding as when they were put forth, except that a change of circumstances has rendered it necessary to depart from two or three of the rules which use arbitrary numbers in expressing their requirements. The increased numbers of the insane to be provided for, and the larger ratio of the chronic to the curable inmates of our institutions at the present time, as compared with that of twenty-five years ago, have rendered it necessary and admissible to treat a much larger number of patients in one institution than was then considered proper. Like many institutions of government, education, and business, those for providing for the insane have grown upon our hands until they have become much larger than was anticipated when those propositions were framed and adopted. Other things being equal, the evidence of experience and authority shows that hospitals having 500 patients are managed as advantageously to their inmates, and quite as economically, as those having half that number; and there is nothing in experience, nor, to my mind, in theory to show that hospitals containing a still larger number of inmates, "embracing," in the language of the fifth proposition of the series of 1866, "the usual proportions of curable and incurable insane in a particular community" are not equally well managed. Of course it is necessary, and, I believe, conducive to the welfare of patients and the advancement of the specialty, that the ratio of staff-physicians should not only be proportionate to the number and character of the patients, but that the physicians should be charged with more authority and responsibility than they usually possessed twenty-five or more years ago. That they should be men of ability, liberal education, and sound principles, no one will deny. It does not appear to me that the number of assistant physicians required in large institutions for the insane can be determined by a rule of universal applicability, but I think that a proportion of one to one hundred cases of active disease, whether acute or chronic, and one to two hundred chronic cases that are ordinarily quiet and orderly, will, on the one hand, afford no opportunity for the rust and vices of idleness, and will, on the other, be sufficient for the performance of every useful professional office in the medical and moral treatment of the patients, with time for the relaxation necessary to the maintenance of cheerful health, and for professional and general improvement.

The prevalent overcrowding of institutions for the insane, is highly prejudicial to the welfare of their inmates. Not only should this be avoided by the provision of ample room for the number of cases under treatment, but the facilities of classification or ward separation possessed by institutions which receive all the insane of a community, should considerably exceed those ordinarily provided, in order that, in addition to the usual classification, based mainly on conduct and the activity of disease, there should

be sub-classes of epileptics, of dipsomaniacs, of regularly working patients, and of patients who are too demented, or who, though able, are too indolent, to work—each of these classes requiring some special provision and treatment.

In enlarging district institutions, in addition to extending the original structure as far as it will admit, I think that one or more detached buildings or wards may be desirable in effecting all useful purposes of subdivision. I certainly see no objection to them for the use of the quiet classes of agricultural patients, if not placed too far from the main structure, if the topography of the grounds favor the erection of such wards, and if it be thought otherwise desirable; but I should always consider it an indispensable condition of good management, that the assistant physician having immediate charge of the inmates of a block or group of blocks, should have such a residence that he could not only conveniently see his patients often, but that he could be readily consulted at all hours of the day and night. In some cases, the fitness of accommodations might be most economically and conveniently secured by the erection of new and improved buildings on the same grounds with old and imperfect ones, and the devotion of the former to the recent cases and their suitable associates among the chronic, and of the latter to the most hopeless cases or to those belonging to the collateral classification which I have recommended.

When a district institution contains as many as 600 patients of both sexes, and the number is likely to considerably increase, a second complete set of hospital buildings should be provided in the same neighborhood, and, if practicable, on continuous grounds, and the sexes separated by retaining one in the old buildings and placing the other in the new. The buildings for each sex should be separately inclosed, and not much less than half a mile distant from each other, unless a hill or wood should present a natural barrier to the view and hearing of one from the other. The two sets of buildings being in the same neighborhood, patients of both sexes can at the same time be accompanied to and from them by the same friends or officers, and the same relatives can visit patients of both sexes by one journey, which will not unfrequently be both a convenience and a saving of expense. The employment of each sex will conveniently and economically supplement that of the other. The men will raise vegetables, milk, etc., for both establishments, and the women may do the washing, mending, etc., for the men as well as for themselves. The separate care of each sex will render admissible many relaxations of the restraints to personal liberty that prudence requires when both sexes occupy connecting wings of the same continuous structure. After the separation, the superintendent may remain in charge of both establishments, with an adjunct in immediate charge of each, or there may be an independent superintendent of each, under the same board of management, as circumstances may render most expedient.

All public buildings, particularly those belonging to a government of the power and dignity of a State or Province, should present just claims to architectural fitness and taste. Edifices occupied by the insane should be at least neat and cheerful in their appearance; and their construction of durable materials in the most enduring manner will prove most economical in the end. Cost should be held subordinate to every essential sanitary provision, as drainage, ventilation, the supply of light, heat, and water, and abundant room and means of classification; and just in proportion as such provisions are subordinated to necessary cost in the construc-

tion and fitting up of buildings for the insane, do these become custodial receptacles which deny to their inmates the benefits of hospital treatment. There is, however, some incongruity in providing highly ornamental and costly structures mainly for the treatment of the insane poor, most of whom have little or no æsthetic capacity to appreciate and be benefited by the outlay; and it may be feared that the taste and ambition of legislatures that at one period authorizes the high embellishment of hospital structures at considerable cost, will at another be accused of extravagance, and that, in the strifes of political parties, such expenditures will be made the pretext, if not the reason, for withholding appropriations for the most necessary purposes. Happily, the first cost of the essential provisions for the most humane and beneficial treatment of the insane of the dependent classes, has now been determined by a wide and varied experience, and is found to be much less than has been sometimes expended for the purpose, and not to exceed the means of any State. What the States and Provinces have already accomplished in this direction, and most of it in the short period of less than fifty years, while they have been at the same time not only providing their capitols, court-houses, penitentiaries, and other buildings that directly appertain to the necessary functions of government, and also making considerable outlays for institutions for the blind, for the deaf and dumb, and for feeble-minded youth, affords as positive a guaranty as anything that lies in the future can, that at no distant day all of them will adequately care for all their insane poor, as well as no little excuse for what appears to impatient benevolence and sometimes unpractical science, to be inexcusable tardiness in the discharge of their imperative duty.

Discussion on Dr. Nichols's Paper.

After the reading of the preceding paper, Dr. Isaac Ray, of Philadelphia, said:—The increase of insanity among us has led to much inquiry as to the best means of caring for its subjeets, who are mostly of the indigent class, with the greatest degree of economy compatible with a proper regard to their comfort and cure. One of the conclusions arrived at by many intelligent men, some of them superintendents of our hospitals for the insane, is that the larger our hospitals, the more cheaply they may be built and maintained. This conelusion I have always regarded as unsound, and the more the experiment has been tried, the more it has confirmed me in this opinion. True, an institution for 200 patients would, undoubtedly, eost less, proportionately, than one for 40 or 50 patients, other things being equal. But the rule is not absolute. There eomes a point where the economical result is reversed, so that the larger the hospital, the more expensive it will prove. Indeed, it would be hard to show how increase in size can be followed by decrease in expense. A well organized hospital-staff should consist of a superintendent, two assistant physicians, a steward, matron, and a force of attendants equal to one to every five or ten patients. A farmer, and more or fewer assistants, will be needed for outside work. Not less than this is required in a hospital of 300 patients. Consequently, a hospital for 600 must require double the number of assistant-physicians, and double the number of attendants, and of indoor and outdoor servants. The matron and steward will each require a helper. Where or how the eost of maintenanee is diminished, is not very obvious. True, the eost of one superintendent is supposed to be saved, but, unquestionably, the larger the establishment the larger would be the salary of the superintendent, and as more eare and responsibility would be thrown on the assistants, their compensation would naturally be increased. So that little, if anything, could be saved in

that direction. Besides, I believe, it is generally understood among those who have had some experience in the financial history of institutions requiring the expenditure of more or less money, that the practice of economy is much affected by the amount of money disbursed. The smaller it is, the more frugally it is spent. In the daily handling of large sums, there comes, as a result of such familiarity, a sort of contempt for small sums not likely to produce the strictest economy. This is human nature, and implies no lack of honesty or sagacity. Though not very familiar with the reports of our hospitals, of late years, yet I have the impression that our largest hospitals show no decided advantage over the smaller, in the cost of maintenance. Any little difference in their favor may be, very likely, the result of other causes. According to the report of the Willard Hospital, for the year 1874, the average cost was $3.17 per week, if I recollect rightly, which is little less than it is in some of our smaller State hospitals. And yet it is supposed that in the former the patients are all of the kind that can be properly maintained at the least expense. And this figure is likely to be increased in future years, by the repairs and renewals, always a formidable item in the expenditures, and none the less, certainly, where the buildings are cheaply constructed, and all the appointments of a makeshift kind.

Nor do I see how the expense of construction can be diminished by the plan much favored just now, of having the hospital consist of several buildings, separate and independent, scattered over the premises, instead of a single structure receiving all the patients under its roof. The idea that anything is gained by this mode of construction is such an obvious fallacy that one hardly knows how to meet it. It certainly has always been supposed that, in building, the less you have of wall and roof, the less will be the first cost; and if that be so, it is self-evident that 20 or 30 patients can be more cheaply provided for in a hall, in the usual way, than in a building designed only for them. Nor does it need any special proof to show that the expense of warming and ventilating several detached buildings must be far greater than would be needed for a single one of equal capacity. The fact is obvious at sight. In short, the clamor against "palatial" structures, to use the popular phrase, is little better than claptrap, thoughtlessly used by people supremely ignorant of the cost of building at the present day, as well as of the peculiar requirements of a hospital for the insane. As the result of my own observation and experience, I am convinced that four hospitals for 300 patients each, can be both built and maintained at a less cost than one for 1200 patients, equal provision being made in both cases for the kind of care to which the insane, even in the lowest grades of the disease, are entitled.

I doubt, however, whether it is possible to have, in these mammoth establishments, certain qualities of administration indispensable to their highest purposes. The animating spirit, the close and thorough supervision, inspiring, guiding, correcting every movement, and essential to our highest idea of hospital management, will be but feebly manifested under such conditions. The patient is but an atom in the great mass around him, losing the attributes of humanity, sane and insane, in the technical character of patient.

Contending as I now do, and as I always have done, against the establishment of large hospitals, I cannot give an unqualified approval to Dr. Nichols's paper. With this exception, however, I heartily concur in all his conclusions.

Dr. Thomas S. Kirkbride, of Philadelphia, said:—I am sure that we must all appreciate the value and importance of the excellent paper which we have just heard read. There is little in it with which I do not agree entirely. On one point, however, I must confess that my views differ from those expressed by my excellent friend, and that is, in regard to the best size for hospitals for the insane. We all know that this is a point on which there has, of late years at least, been a difference of opinion in the Association of Medical Superintendents. As it now stands, indeed, it is the only one of our many propositions that has not received an unanimous, or very nearly unanimous,

approval by the members. I still believe that the original proposition was right. I believe that the size then recommended is really better than any other, but I am also fully aware of all the arguments that can be brought to justify much larger hospitals, especially those of *expediency* and even *possibility.* It is fully shown by reliable statistics, as I believe, that the people of the State will derive more benefit from several small hospitals in different parts of the State, than from one large one at a central point; and I think that it will also be found that the former can be provided with quite as small an expenditure of money, and can be carried on at no greater eost per patient.

Dr. Nichols has recommended the plan, which has now been fully tested, and which, even by the first decision of the Assoeiation, allows as many as 500 patients being collected in one vicinity; and this is by having separate hospitals for the sexes. This plan I feel that I have a right to say is no longer an experiment. A trial of sixteen years under my own observation has shown that, in every respect, it is a most valuable arrangement, having many advantages and no disadvantages. It has also received unqualified approbation from many other distinguished psychologists in addition to Dr. Niehols. I am aware that it has been said that, while I argue in favor of hospitals for 250 patients, practically, I have been providing one to accommodate twice that number. This, however, is a mistake. The Pennsylvania Hospital for the Insane, at Philadelphia, is really made up of two entirely distinct hospitals, one for each sex, and it has been so, as already said, for more than sixteen years. Both hospitals have the same board of managers, and, for reasons not neeessary to detail here, have always had the same physician-in-chief and superintendent. It is anticipated, however, that ultimately each will have its own separate superintendent, and that the only connection between them will be the board of managers.

Now, what we have done, is exactly what I would propose for any hospital. First build your hospital for 250 patients. This will commonly be occupied by both sexes. When this is filled, or nearly so, build another for a similar number of patients. Separate the sexes; have distinct organizations—all on the same tract of land, but with entirely separate pleasure-grounds. There may be the same water-works, gas-works, bakery, machine-shop, and other common arrangements, that will add to the economy of the institution. This provides for 500 patients in one locality, and yet conforms to the original proposition of the Association. I do not propose entering into any extended diseussion of the reasons why I believe that these smaller hospitals have advantages, but there is one that I cannot avoid referring to, and that is the personal intercourse which a superintendent is able to give to his patients, when their number is not so great as to prevent his paying daily, or very nearly daily, visits to each. I believe this to be one of the most important of all his duties, and one which generally, if he is rightly constituted for his position, *no one can do for him.* It is true that an assistant may be superior to his chief in this respect, but this is not to be anticipated, and if it is so, the assistant ought to have charge of a hospital himself. Two hundred and fifty patients, or about that number, are as many as any superintendent can visit daily, with tolerable satisfaction to himself and to them.

Now, I do not wish to be misunderstood in this matter. I still think, as the Association at first thought, that this size of 250 is the very best, but I do not mean to say that no other is admissible. I am well aware how much easier it is to get an appropriation for the extension of a hospital, than for the building of a new one. What I mean to say is this:—Build your hospitals, in different parts of a State, of a capacity for 250 patients, and in sufficient numbers to accommodate *all* your insane, *if you can do so.* If you cannot do this, and yet can build single hospitals for 500, 700, or even 1000 patients at one point, by all means do so. The hospitals you must have at all hazards. If you cannot get what I regard as the best arrangement, come as near to it as you can, let the size be what it may.

The President, Dr. John P. Gray, of Utica, said:—I should object to putting buildings half a mile apart, if for no other reason, because such an arrangement would render it difficult, if not impossible, for the superintendent to consult constantly with his subordinate officers. I was chairman of the commission which located the Willard Asylum. We found the point on the lake the most desirable place for the main hospital building; the building now called the "branch," and more than half a mile away, was then erected; it was the old State Agricultural College, and was subsequently remodelled to receive patients. But there is a resident physician in the "branch," as there are in other separate buildings, since constructed, and these physicians, for all practical purposes, exercise the power and control of resident superintendents, for the communication between these distant structures and the main hospital-building is by telegraph. Although that is an institution for the subjects of chronic insanity, there are five medical men, a superintendent and four assistants, for a thousand patients—and they are needed. Though as a commissioner I voted to place the main building on the lake, I did not like the feature of having buildings half a mile or more apart. In this instance it seemed a necessity, though undesirable.

As to the other question, as to the number to be accommodated in a single institution, or under one superintendent, there is great difference of opinion. Some think it just as practicable to have 2000 as 1000, or less. I think that 600, the maximum of the proposition of the Association of Superintendents, passed a few years ago, is as many as should be under one superintendent, even if the buildings be separated, and each have a resident physician. In England, some of the best public institutions are larger than those in this country, and, according to Dr. Bucknill, in his recent letters on American Asylums, published in the Lancet, are not as well supplied with medical officers. That at Wakefield, the West-Riding Asylum, standing at the head, has 1800 patients.

In reply to the criticisms of Dr. Kirkbride on the qualifications of assistant physicians, I think it important to have assistants who have had hospital experience. I should object to taking young men just out of a medical college, and without medical experience, and putting them in such responsible places. Dr. Kirkbride's remarks as to qualifications would apply with double force in an asylum where the buildings were separated.

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THE END.